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On the Method of Determining the Range of Accommodation, &c. By L. HAPPE.

19. *Die Theorie des Sehens und räumlichen Vorstellens vom physikalischen, physiologischen und psychologischen Standpunkte aus betrachtet.* Von C. S. CORNELIUS.—*Halle,* 1861.

Vision in its Physical, Physiological, and Psychological Relations. By C. S. CORNELIUS.

FOR the first fifteen hundred years of the Christian era, the views of Plato and Aristotle in regard to the visual process universally prevailed, occasionally indeed somewhat modified in form, but rather deteriorated than improved. The discovery of the camera obscura by J. B. Porter, in 1560, introduced a new era, which was still further and more decidedly entered upon by Scheiner (1625–1652), who pointed out that an image similar to that formed in the camera was produced in the eye. But the honour of being the founder of physiological optics is justly due to Kepler (1604), who, indeed, had made more progress in this branch of science three hundred years ago, than many recent physiologists. He was the first to explain the physical formation of the inverted image in the eye, and showed that it was placed on the retina, and not on the choroïd or crystalline lens, as had been often imagined. He recognised the necessity of some change in the eye to accommodate it to different distances, and supposed this to be effected by an alteration in the position of the lens; and he also treated on various other questions, such as single vision with the two eyes, double vision, why objects appear erect although their images are inverted, and the apparent size of objects.

The discovery of Mariotte, in 1668, that the entrance of the optic nerve is insensible to light, led him to the opinion that the choroïd must be the really sensitive membrane. A controversy on this subject ensued, and continued for a hundred years, until Haller and Bernouilli seemed to finally settle the question by their researches.*

The discoveries of Newton form an era in the history of physical optics; physiological optics, however, made no corresponding advance, but until the present century remained in every essential particular at the point to which Kepler had brought them. During this time, it is true that many valuable observations were made by Porterfield, Lecat, Haller, Young, &c., though insufficient to cause any great or general progress. Early in the present century we have the very valuable work of Johannes Müller, worthily accompanied or followed by the investigations of Purkinje, Tourtual, Volkmann, Listing, Donders, and, finally, Helmholtz, the discoverer of the ophthalmoscope, and the author of a most elaborate and important treatise on physiological optics.

The limited space allotted to a review will not permit any detailed survey of so comprehensive a subject; but we shall endeavour to supply a sufficient account of its most elementary and, we might say, fundamental parts. On these we shall enter at some length, in part because of their importance, and still more because they have been hitherto almost completely neglected in this country.†

The eye is formed by a number of transparent media, differing in their refractive power, and which are separated at various distances by surfaces of different degrees of curvature. Now the most important question, and the one on which the whole of physiological optics may be said to depend, is this—viz., what is the course of a ray of light in the eye, supposing its direction to be known previously to its entering the eye?

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* Curiously enough, Draper, in his 'Human Physiology,' New York, 1856, has brought forward a theory, according to which light does not act immediately on the retina, but passes through this membrane, is absorbed by the pigment layer of the choroid, and then re-acts on the rod-layer, which is excited by the warmth so caused, and not by the luminous waves as light. This is evidently Mariotte's theory modified; the choroid instead of the retina being thus supposed to be the membrane in which light is transformed physiologically into what is no longer light.

† The formulæ introduced are of the simplest character, so that they may be readily understood by any one acquainted with the elements of mathematical optics; should the reader, however, meet with any difficulty, the chapter on optics, in Goodwin's 'Course of Mathematics,' may be of some assistance. The millimetre, so often employed in the following lines, is equal to 03937 of an English inch.

This, again, is only a special application of the more general mathematical problem: an expression is required for the course of the rays in a system of transparent media of any number and refractive powers, situated at any distance from one another, and of any spherical curvature, placed, however, perpendicularly to one and the same axis.

If the answer for this general problem could be found, it would only be necessary to replace the indefinite by definite values to render it at once applicable to the eye.

Our countryman Cotes* was the first who tried to solve this problem in its most general form. Since his time many mathematicians have occupied themselves with the search after simple expressions, and we may mention more especially three of the first rank, Euler,† Lagrange,‡ and Gauss.§ The researches of the last author have led to the most elegant, simple, and comprehensive rules, by the application of which the course of rays, obeying certain conditions and passing through media subject also to certain conditions, may be determined with sufficient exactness. These rules have now been almost universally adopted in Germany. Here of course we can do little more than indicate the results, at the same time endeavouring to explain the processes sufficiently for clear comprehension; for further details we must refer the reader to Fick's very excellent work on 'Medical Physics;' to Zehender's full account; or to the admirable treatise by Professor Helmholtz.

The most striking result is the great analogy between refraction by a compound system, and that by a single refracting surface. It may, therefore, be well to briefly enumerate some of the principal qualities of the latter, and proceed to trace the analogous points in a compound system. As already mentioned, we shall simplify our subject by assuming the following conditions: that the media cause only single refraction; that they are homogeneous, and separated from one another by spherical surfaces, of which the centres are all placed on the same straight line, the axis. The rays themselves at their commencement must make only very small angles with the axis or its prolongation, and also must form with the surfaces right angles, or at least angles scarcely differing from right angles; in other words, their angles of incidence are extremely small.

Let now p stand for the distance from the focus of incident rays to the vertex; p' for the corresponding distance of the focus of refracted rays from the vertex; r for the radius of the spherical surface. We shall use the terms behind and before with reference to the course of the rays of light, and shall consider the radius positive when the centre of the spherical surface is behind, negative when before the same surface; p positive when situated before, and negative when behind it; p' positive behind, negative before. Thus a positive radius will correspond to a convex surface, a positive focus of incident rays to a real focus, and a positive focus of refracted rays also to a real focus, and *vice versâ* for the negative.

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* Smith: A Complete System of Optics. Camb. 1738. Vol. ii. Remarks, p. 76.

† Euler: Histoire de l'Acad. Royale de Berlin, pour l'année 1757, p. 283. Ibid. pour l'année 1761, p. 201. Hist. de l'Acad. Royale des Sciences de Paris, 1765, p. 555.

‡ De la Grange: Nouveaux Mémoires de l'Acad. Royale de Berlin pour l'année 1778, p. 162. Ibid., pour l'année 1803. Classe Mathématique, p. 1.

§ Gauss: Dioptrische Untersuchungen. In den Abhandl. der Kön. Ges. d. Wiss. zu Göttingen. Thl. 1 von den Jahren 1838 bis 1843.

The formula for incident rays diverging from a real focus will be

$$\frac{1}{p}+\frac{n}{p'}=\frac{n-1}{r}$$

This shows that under such conditions a pencil of homocentric* rays remains homocentric after refraction.

If the rays in the denser medium after refraction are parallel,

$$(p'=\infty)\ p = \frac{r}{n-1}$$

And this we may call the first principal focus.

If the incident rays in the rarer medium are parallel,

$$p' = \frac{nr}{n-1} \text{ (2nd principal focus)}$$

If now at the point p we suppose a plane perpendicular to the axis, and take any points in that plane which are situated at a very small distance from p, in comparison with the distance of p from the refracting surface, we shall find that these points will have their images in a plane perpendicular to the axis at the point p'. Correspondingly, if such a plane be drawn through the first principal focus (this plane may be called the first focal plane), the rays radiating from a point which is situated near the axis in that plane will be parallel to one another after refraction; and it is evident that in the same way the corresponding points of a plane drawn through the second principal focus will be the foci of rays which were parallel before refraction. This, however, is equivalent to saying that an image of any object will be formed, provided the various luminous points of which it is composed obey the above conditions. By means of these planes it is easy to find the direction of the refracted ray, the course of the incident ray being given. Draw an auxiliary line, parallel to the incident ray, from the first principal focus to the refracting surface; from the point where these meet, draw a line parallel with the axis to the second focal plane; the line connecting the point where this auxiliary ray meets the second focal plane, with the point where the original ray was incident, will delineate the course of the ray after refraction. Or still more simply, we might take as an auxiliary line that which passes parallel to the incident ray directly through the centre of the spherical surface, and prolong it to the second focal plane: the point found will be the same.

Refraction by a system of spherical surfaces.—It is not difficult to find a general solution of this problem;† but when the number of refracting media is great, the expressions become exceedingly complex, and numerical calculations excessively tedious and liable to error. Hence it is of importance to employ as simple an expression as possible, yet without any material loss of accuracy. Gauss showed that the problem became simplified if instead of referring the course of the incident ray to the vertex of the first surface, and that of the ray after its last refraction to the vertex of the last surface, they were referred to two other points E and E*, such that the refracted ray strikes a plane erected vertically to the axis at E*, at a point of which the co-ordinates are exactly the same as those of the point at which the incident ray strikes a plane vertical to the axis at E. These two points E and E* are called by him principal points (*Hauptpunkte*); they are analogous to the vertex of a single refracting surface, for E has the same reference to the incident ray, E* to the refracted ray, that the vertex of a single surface has to both: the planes erected at the points E and E* vertically to the axis are called principal planes. E is the first principal point, E* the second; the planes are correspondingly called the first and second. Similarly he showed that there are two points, F and F*, corresponding to the two principal foci of a single refracting surface, and which he called the principal foci of the system. Planes drawn perpendicularly to the axis through these points are called focal planes. F is situated in the first medium, F* in the last. As in the case of a single surface, F in a compound system is the first principal focus, and rays diverging from it will be parallel in the last medium; F* is the second principal focus, and rays converging to it will be parallel in the first medium. The focal planes of a compound system have just the same properties as those of a single refracting surface. Corresponding to the centre of a single surface, there are two points in a compound system, called by Listing knot-points (*Knotenpunkte*), for the first one, D, bears the same relation to the incident ray, the second, D*, to the refracted ray, as the centre of a single surface does to both; or in other words, a ray passing towards the first point would be simply carried along the axis as far as the second one, and would then proceed in its original direction—i.e., for every incident ray that passes through the first knot-point a parallel ray passes through the second knot-point. These points enable us to determine in a compound system the course of a refracted ray, the incident one being given. Let the incident ray strike the first focal plane in *a* and the first principal plane in *b*. Draw a line parallel to *a b* through the first principal focus, it will meet the first principal plane in *c*. Draw a line from *b* parallel with the axis till it meets the second principal plane in *d*. Finally, draw a line parallel likewise with the axis from *c* until it meet the second focal plane in *e*. The straight line connecting *d* to *e* will be the course of the refracted ray in the last medium. This construction may be still further simplified by means of the knot points; all that is necessary is to draw a line (D* *e*) parallel to *a b* from the second knot-point D* to the second focal plane, and *e* is found again.

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* Homocentric, having one central point to which they converge, or from which they diverge.

† Encyclop. Metrop., art. Light, p. 381.

Explanation.—As already mentioned, the refracted ray will strike the second principal plane at a point, of which the co-ordinates are the same as those of the point at which the incident ray strikes the first principal plane; hence the refracted ray corresponding to the incident ray *a b* must pass through *d*. Again, rays that are parallel in the first medium will come to a focus at some point of the second focal plane; hence the incident ray *a b* and the auxiliary ray, which are parallel in the first medium, must pass through the same point of the second focal plane after they have been refracted. The course of the auxiliary ray is readily determined, for all rays that pass through the first principal focus are parallel with the axis after refraction, and between the principal planes no deviation from the axis takes place, as mentioned above. Thus it is evident that the point *e* is the focus of a pencil of incident rays parallel to *a b*, hence the particular ray *a b* must pass through *e*. It has, however, been already shown that it must also pass through *d;* therefore *d e* must be the direction of the incident ray *a b* after refraction.

In a compound system, a ray incident in the direction of the first knot-point, will after refraction be represented by a parallel ray, passing from the second knot-point onwards; we have, in fact, made use of an auxiliary ray parallel to *a b*, of which the course is given; and again, owing to the incident rays being parallel, we know that they will meet at some point of the second focal plane. If the position of a point is given, we may readily find its image, by selecting from all the rays which pass from it two particular ones, and constructing their course in the manner already pointed out; the point at which they intersect is of course the one required. The two rays best adapted for this purpose are, first, the ray parallel to the axis, for in the last medium it must pass through the second principal focus; and, secondarily, the ray passing to the first knot-point, which in the last medium will pass through the second knot-point, and be parallel to its original course. The reader will find it a useful exercise to prove the truth of the following relations between the image and the object.

If the object is placed before the first focal plane, and on one side of the axis, the image does not lie before the second focal plane, and is placed on the opposite side of the axis; both object and image are real. Of this a special case is when the object is at an infinite distance before F, the image is in the second focal plane, for the incident rays are parallel. A second case is when the object is placed in the first focal plane, the image is at infinite distance behind the second; the refracted rays are parallel. If the object is placed between the first focal plane and the first surface of separation, its image becomes a virtual one, lies before the object, and on the same side of the axis. If the object gradually passes behind the first surface of separation, becoming virtual, the image remains at first also virtual; in front of the first principal plane we arrive at a plane in which both the image and object are placed, although not at the same point. If the object is in the first principal plane, the image is in the second principal plane on the same side of the axis, and at the same distance from it, as the object. When the object passes still further backwards, it again arrives at a spot at which both object and image are placed in the same plane. Still further backwards, the image remains in front of the object, moving in the same direction, although much more slowly, and finally reaches the second focal plane, when the object is at an infinite distance—i.e., when the incident rays which had been gradually becoming less convergent, have become parallel.

The following summary of the properties of these points may now be intelligible:—

Cardinal points are formed by three pairs of imaginary points, focal, principal and knot-points. Each pair is composed of a first and second point; the first is invariably the one that has reference to the course of the ray in the first medium, the second the one that has reference to the course of the ray in the last medium.

The first focal point.—Every ray passing through it before refraction is parallel with the axis after refraction.

The second focal point.—Every ray that passes through it was, previously to refraction, parallel with the axis.

Principal points.—Rays passing through the first principal point before refraction, will pass through the second principal point after refraction.

Knot-points.—Every ray passing directly through the first knot-point will after refraction pass through the second knot-point; the refracted will be parallel with the incident ray. Rays passing through the knot-points are called axes (*Richtungs-strahlen* or *-linien*), principal or secondary, as the case may be.

Planes drawn perpendicularly to the axis through the focal, principal, and knot-points, are called respectively focal, principal, and knot-planes.

The distance between the two knot-points is equal to that between the two principal points.

The distance of the first principal point from the first focal point is called *the first principal focal distance;* the distance of the second principal point from the second focal point is the *second principal focal distance.*

The difference of the two principal focal distances equals the distance between the corresponding principal and knot-points: the first principal focal distance bears to the second the same ratio that the index of refraction of the first medium does to that of the second.

Focal planes.—Rays proceeding from a point of the first focal plane are parallel to one another after refraction—i.e., to the secondary axis. Rays parallel in the first medium have their focus where the secondary axis meets the second focal plane.

Listing's ideal eye.—Examining the human eyeball from before backwards, we meet with three curved surfaces—that of the cornea, and the anterior and posterior surfaces of the lens; and four media—the substance of the cornea, the aqueous humour, the substance of the lens, and that of the vitreous body. These curved surfaces are not exactly spherical, and their centres are not precisely on the same line, so that the principles already adduced cannot in a strict sense be applied to the eye. If, however, we take only very small portions of the surfaces, we may evidently consider them as spherical, and at the same time may regard the centres of these portions as all on the straight line drawn from the vertex of the cornea to the centre of the macula lutea (the optic or visual axis). By this means we shall be able to employ the principles already laid down, and shall obtain approximations in no way differing from those employed in other sections of physical science. Conclusions so deduced approach the true ones, just in proportion as the hypotheses on which they are founded approximate to the true facts. Such approximations furnish us with a simpler subject for consideration, in fact, with an ideal which we can afterwards compare with the real, and on which we can trace the effects of the various points previously disregarded.

Indeed, by considering these (vertex) portions of the refracting surfaces of the eye as spherical segments, and their centres as placed on the visual axis, we have transformed the eye into a system of spherical refracting surfaces, of which the centres are all on the same straight line, and this may well be called an ideal or diagrammatic eye (*das schematische Auge*). Of course, we can apply all the principles just developed to such an ideal eye without further limitation. It must, however, be always remembered, and owing to its importance a repetition may be allowed, that all the principles, formulæ, &c., hitherto developed, are valid only for those rays that form very small angles (strictly speaking, infinitely small) with the axis, and which at the same time strike the refracting surfaces very near their vertices, so that the angles of incidence may be extremely small. An example may render this more forcible: If this page be placed eight inches from the eye, and the number of the page be the part fixed, it must not be expected that the pencils of rays proceeding from the letters immediately beneath will be refracted according to the same laws as the rays proceeding from the number fixed; the former rays would form too great angles with the axis, hence their course cannot be at all determined by the constructions previously given. If the pupil is of large size, even some of the outer rays proceeding from the point fixed will fall too obliquely to be treated by our rules. In regard to the eye, these can only include rays proceeding from points which are very near the point fixed (and which is itself in the prolonged optic axis), in comparison with the distance of the points themselves from the eye, and even such rays only in case they fall near the vertex of the cornea. We may express the latter statement in another form; the rules hitherto laid down presuppose a very small pupil. This is evident from the fact that the points in a plane perpendicular to the optic axis are to have their images formed in one and the same plane, and this can be true of only a very small part of the retina. The more lateral images must be found by other rules; these will only apply to images formed on a piece of the retina around the extremity of the axis, and so small that it may be considered as a plane surface perpendicular to the axis. Some of our laws may indeed be applicable to lateral rays; Volkmann,* for example, has concluded from his experiments, that lateral images are formed on axes which pass through the same knot-points that serve for the construction of the central images. This circumstance could not, however, have been foreseen, at all events, not from the calculations of Gauss; indeed, it is very possible that it depends on the deviation of the refracting surfaces from the spherical form.

The next step in constructing such an ideal eye as may serve for a basis for further considerations in physiological optics, will be to assign definite values to the optical constants, to the radii of curvature, to the indices of refraction, &c. The values assigned should be such as really occur in normal eyes; at the same time, when combined together, they should form an optical apparatus equivalent in its action to a normal eye adapted for distant objects, or as commonly considered, at rest. Listing arranged such a system of optical constants as to form what is usually called Listing's ideal eye. He takes only four media into account: air, aqueous humour, lens, and vitreous body. He thus considers the aqueous humour as reaching to the anterior surface of the cornea, a further simplification, of which the admissibility has been since confirmed by Helmholtz. A diaphragm or iris, with a central aperture or pupil, is supposed to exist in the ideal just as in the real eye. The following are the values assigned by Listing :—

Index of refraction of air $= 1$
" " aqueous humour $= \frac{103}{77}$
" " substance of lens $= \frac{16}{11}$
" " vitreous body $\frac{103}{77}$
Radius of cornea $= + 8$ mm.
" anterior surface of lens $= + 10$ mm.
" posterior surface of lens $= - 6$ mm.

The distance from the anterior surface of the cornea to the anterior surface of the lens = 4 mm. = distance from the first to the second refracting surface. The thickness of the lens = the distance from the second to the third refracting surface = 4 mm.

These values, so far as they are accessible to direct measurement on the living person, agree pretty well with those since found by Helmholtz.† By means of these values the positions of the cardinal points have been calculated as follows :—

The first principal point lies 2·1746 mm. behind the vertex of the cornea; the second principal point is 5·4276 mm. before the vertex of the posterior surface of the lens. Since, however, the latter is itself 8 mm. behind the vertex of the cornea, the distance between the two principal points will be 0·3978 mm.

The first principal focus is 12·8326 mm. before the cornea, and the second principal focus is 14·6470 mm. behind the posterior surface of the lens. Therefore the first principal focal distance = the distance of the first principal focus from the first principal point = 12·8326 +

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* Volkmann in Wagner's Handwörterbuch, Band iii. Abth. 1, S. 286–9.

† Gräfe's Archiv, Band i. Abth. 2.

2·1746 = 15·0072 mm.; and in like manner the second principal focal distance equals 20·0746 mm.

The first knot-point is situated 7·2420 mm. behind the vertex of the cornea, and 0·7580 mm. before the posterior surface of the lens: the second knot-point is 0·3602 mm. before the posterior surface of the lens, and there is therefore between the two knot-points a distance of 0·3978 mm., the same as between the two principal points.

Listing considered the ideal eye to be adapted for rays proceeding from very distant objects—i.e., parallel rays have their focus on the retina; thus, the centre of the retina would coincide with the second principal focus.

Direct measurements of the antero-posterior diameter of the eye, from the vertex of the cornea to the diametrically opposite point of the outer surface of the sclerotic, have shown that its average length is about 24·25 mm.; we have already (= second principal focal distance + distance between the two principal points + distance of the first principal point from the cornea = 20·0746 + 0·3978 + 2·1746 = 22·6470 = distance of macula lutea from vertex of cornea) 22·6470 mm. as the distance of the second principal focus from the vertex of the cornea, and the remaining 1·6 mm. may be considered as the thickness of the sclerotic, &c. Fig. 1 represents a horizontal section of the right ideal eye, as seen from above, and magnified three linear dimensions. C is the centre around which the eye rotates, 12 mm. from the vertex of the cornea; F, F* are the principal foci, E, E* the principal points, D, D* the knot-points, N° the vertex of the cornea, N′ that of the anterior surface, and N* that of the posterior surface of the lens.

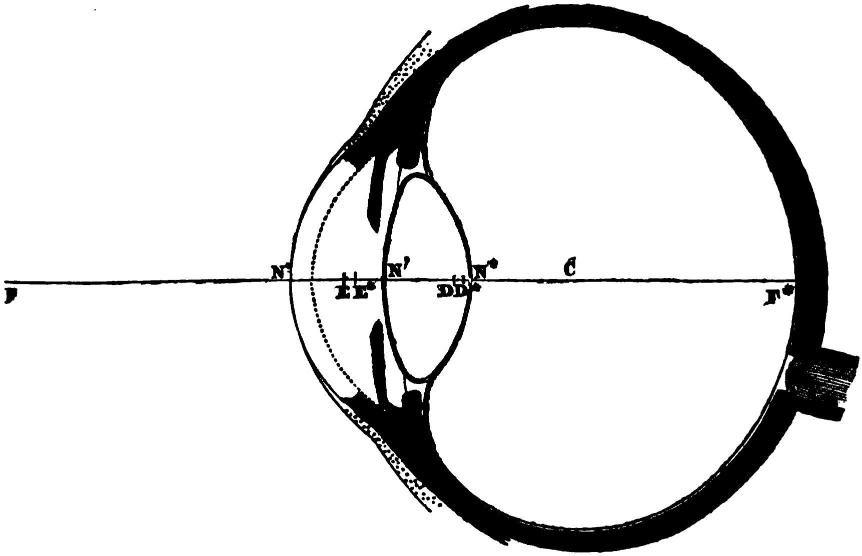

Fig. 1. From Listing in Wagner, l. c., Band iv., S. 492.

As already mentioned, the ideal eye of Listing is constructed on the supposition that the normal eye is accommodated for parallel rays when at rest. Should this supposition be hereafter found to be incorrect, some alterations would have to be made in the values assigned; thus, for example, Zehender* believes that the normal eye when at rest is accommodated only for a few feet (3–6 feet), or in other words, that the second principal focus is in front of the retina.

If this be the case, it will be necessary to modify Listing's ideal eye† either by slightly lengthening its axis, or, what is better, by diminishing the distance between the anterior surface of the lens and the cornea (thus, from 4 mm., the value assigned by Listing, to 3·5 mm.).

Helmholtz is inclined to consider, that the thickness of the lens (4 mm.) in Listing's eye would probably correspond only to that of an eye accommodated for near objects, and also that the value assigned for the distance between the lens and cornea (4 mm.) is too great. For the sake of comparison we append the values calculated by him for the optical constants and cardinal points of an ideal eye closely resembling the eyes of living persons which he examined, for two different conditions of the accommodation. The one adapted for distant objects differs from that of Listing only by the less thickness of the lens and depth of the anterior chamber. The indices of refraction are the same as those employed by Listing:—

	Eye accommodated for	
	distant objects.	near objects.
Radius of corneal curvature	8	8
„ anterior surface of lens	10	6
„ posterior surface of lens	6	5·5
Distance of the anterior surface of the lens from the anterior surface of the cornea	3·6	3·2
Distance of the posterior surface of the lens from the anterior surface of the cornea	7·2	7·2
Distance from anterior surface of cornea—		
of first focal point	—12·918	—11·241
second focal point	22·231	20·248
first principal point	1·9403	2·0330
second principal point	2·3563	2·4919
first knot-point	6·957	6·515
second knot-point	7·373	6·974

Admitting that this eye, when accommodated for distant objects, would bring parallel rays to a focus on the retina, the length of its axis would be 22·231 mm. measured from the anterior surface of the cornea to the retina; and the same eye, when accommodated (as above) for near objects, would bring to a focus rays proceeding from an object which is placed 130·09 mm. before the cornea. This would correspond with the range of accommodation possessed by the normal eye.

Reduced eye of Listing.—In all cases where extreme accuracy is not required, we may make a further simplification in the ideal eye; for owing to the extremely small distance between the two principal points, we can in most cases consider them as one; the same is of course the case with the knot-points; and there is thus formed what is called the reduced eye of Listing (*das reducirte Auge*). In this way we have only a single refracting surface and two media; the first being air with the index of refraction 1, and the second, vitreous humour with that of $\frac{103}{77}$. The radius of this imaginary refracting surface $= 5 \cdot 1248$ mm.; its vertex coincides with the single principal point, and its centre with the single knot-point.

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* Dioptrik des Auges, i. S. 190, § 33.

† Gräfe's Archiv für Ophthalm., Band. i. Abth. i. S. 133.

The distance from the vertex of the cornea to the*

First principal focus = − 12·8326 mm.
Vertex of the cornea = 0
Single principal point = + 2·3448 „
Single knot-point = + 7·4696 „
Centre of rotation = + 12
Second principal focus = + 22·6470 „

Optical centre of the eye.—All rays passing directly towards the centre of this spherical surface will strike the surface perpendicularly, and hence will not suffer any change in their direction after refraction. Thus the centre (single knot-point) of this spherical surface exactly corresponds to the "optical centre of the eye" of English authors, and to the "point of intersection of the axes" (*Kreuzungs-punkt der Richtungs-linien*) of German writers. The use of this optical centre is sufficiently clear from what we have already said about the knot-points: we may, however, mention that the angle at the optical centre, formed by the axes passing from the extremities of the object, is called the visual angle.

Circles of dissipation.—If we suppose the ideal eye accommodated for infinite distance, rays parallel to one another would come to a focus on the retina; divergent or convergent rays proceeding from any point would, however, come to a focus behind or in front of that membrane, and would thus form upon it what is called a circle of dissipation. The magnitude of this circle will depend partly on the distance of the object, and partly on the size of the pupil. Let $b\,c$ be the diameter of the pupil, and o its centre, a the luminous point, f the focus to which the rays are converging after refraction; let $b\,f$, $c\,f$, meet the retina in the points $d\,e$; $d\,e$ will be the diameter of the circle of dissipation, and i may be its centre: owing to the similar triangles $b\,f\,c$, $d\,f\,e$, we have $b\,c : o\,f :: d\,e : i\,f \therefore d\,e = \frac{i\,f. \cdot b\,c}{o\,f} = \frac{i\,f}{o\,i + i\,f} \cdot b\,c$, or in words, to find the diameter of the circle of dissipation, we must divide the distance of the image from the retina by the distance of the image from the centre of the pupil, and then multiply by the diameter of the pupil. This equation shows at the same time that $d\,e$, the diameter of dissipation, varies directly as $b\,c$, that of the pupil. Hence, other things being the same, the smaller the pupil, the less is the size of the circle of dissipation.

We may now trace the effects of a change in the position of the object, the ideal eye remaining adapted for parallel rays. Let us suppose that the object approaches the eye at some uniform rate of motion, its image will also move in the same direction—that is, it will be formed further and further behind the second focal plane; it will not, however, move at the same uniform rate: at first it scarcely changes its position; as the object continues to approach, it recedes with increased quickness, until at length the object, being close to the first focal point, it moves with infinite rapidity, so as to be situated at infinite distance, when the object reaches the first focal point. It is scarcely necessary to remark that the circle of dissipation and the distance of the image increase together, and that hence we may readily imagine how a distant object may pass over a great space without requiring any change (in the accommodation of the eye), whilst a very slight alteration in the position of a near object may necessitate a considerable increase in the power of refraction.

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* Fick, loc. cit. S. 269.

Owing to the compound nature of light, &c., a mathematically exact image is never formed on the retina; accordingly, we may readily understand that objects at different distances are seen with equal distinctness, provided their images are not attended by too large circles of dissipation. The eye is practically accommodated for a line and not for a point, a fact specially pointed out by Czermak, who has called it the line of accommodation; its length will vary inversely as the rate of increase of the circles of dissipation, the more slowly they increase, the longer it will be.

Listing* has calculated for the ideal eye the following table: in it l' is the distance of the luminous point from the first focal point, l'' the distance of the image from the retina, z the diameter of the circle of dissipation on the retina.

l'	l''	z
Infinite	0 mm.	0 mm.
65 mètres	0·005 mm.	0·0011 mm.
25 „	0·012 „	0·0027 „
12 „	0·025 „	0·0056 „
6 „	0·050 „	0·0112 „
3 „	0·100 „	0·0222 „
1·5 „	0·200 „	0·0443 „
0·75 „	0·400 „	0·0825 „
0·375 „	0·800 „	0·1616 „
0·188 „	1·600 „	0·3122 „
0·094 „	3·200 „	0·5768 „
0·088 „	3·42 „	0·6484 „

It is as yet unknown to what degree the circle of dissipation can attain in the normal eye, without interfering materially with vision;

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* Listing, loc. cit. p. 499. The formula for the distance of the image from the second focal point, when that of the object from the first focal point is given, is $\frac{F' F''}{l'} = l''$; $F' F''$ being the first and second principal focal lengths; l', the distance of the object from the first principal focus; and l'', that of the image from the second principal focus. In the ideal eye, $F' F'' = 301·26$ mm.; the distance of the pupil from the retina = 19·15 mm.; and its diameter is considered to be constant and = 4 mm.

very possibly this limit varies in different cases.[*] It is at all events clear that up to a certain degree it does not interfere at all with the distinctness of vision, and thus it can be scarcely doubted that an image having the diameter 0·0011 mm. would appear to us only as the image of a point;[†] and if we assume this to be the limit beyond which vision would suffer, the table shows that the ideal eye accommodated for infinite distance would see with equal distinctness an object at the distance of sixty-five metres: it is therefore in this sense accommodated for all objects placed between sixty-five metres and infinity, or in other words, its line of accommodation is of infinite length. Suppose again, that the retina be removed to 0·8 mm. behind the second focal plane; an object at the distance of 375 mm. from the first principal focus would now have its image formed exactly on it; objects further than 377 mm., or nearer than 373 mm., would form circles of dissipation greater than 0·0011. The line of accommodation would in this case be only 4 mm. long. It is also evident that this varies inversely in length as the size of the pupil, and thus that the less the latter, the longer will be the former. These deductions are perfectly consonant with our daily experience. Every one knows that two very distant objects can be seen with equal distinctness, and at the same time, although separated from one another by a great space; thus a star and a distant terrestrial object can be clearly seen at the same time; on the other hand, two objects placed near the observer will be seen with equal distinctness, only when they are very close to one another. We may also conclude that the changes of the eye necessary for the distinct vision of near objects will be very much greater than those required for objects at a distance. A very simple experiment may serve to enforce this fact: let a thin white thread be stretched horizontally before the eye and nearly in the line of the optic axis; if now, one eye being closed, some point of the thread, for example a point ten inches distant, is fixed with the other eye, the thread will appear single for a considerable distance on the further side, and for a short space on the nearer side of that point; elsewhere the thread appears indistinct or double. By fixing different points nearer and further off, we may readily convince ourselves of the existence of lines of accommodation. These facts—that the eye is able to see distinctly over a range of considerable length, independently of any change in its accommodation, and also that in many cases it can disregard even large circles of dissipation, should never be forgotten in estimating the range of accommodation in any given case; too much care cannot be exercised, for numerous errors have been already caused by a disregard of these points. Thus Gräfe[‡] says, with reference to the power of certain persons of employing circles of dissipation for the distinction of objects, "yet more strikingly is this the case with cataract cases which have undergone operation during youth, and have not employed sufficiently strong glasses. Such patients, although really without a trace of accommodation, may yet apparently possess a considerable range when examined by the ordinary methods with parallel lines of small print (at least, so far as refers to counting or distinguishing letters, and not as to distinctness of sight).

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[*] There are some interesting remarks on vision with circles of dissipation by Prof. A. von Gräfe, in the 'Archiv für Ophthalm.,' Band. ii. Abth. 1, S. 170–186. His observations show that there are great differences in this respect between cases; thus, that with equal circles of dissipation the power of recognising objects varies.

[†] The diameter of the bulbs at the macula lutea is from ·0025‴ to ·0030‴ = ·0011 mm. to ·0013 mm.

[‡] Archiv fur Ophth., Band vii. 2, 152.

It would here be the place to discuss the rays that pass to the more lateral portions of the retina; as yet, however, their course has not been very exactly determined. This is of the less importance, because the rays we have considered are those that strike the macula lutea, and it is only by means of them that we really see distinctly; the more lateral portions of the retina give only a very dull qualitative sensation.

Accommodation of the eye.—The eye when at rest is, owing to its structure, the curvature of the cornea, &c., adapted for a certain distance, which is commonly called the most distant point of distinct vision; the rays from an object placed at this distance would come to a focus on the rod-layer of the retina. By the exertion of a certain power we are able to see with distinctness objects placed at a much nearer point; this is *positive accommodation*, in which the eye is rendered more refracting; on the other hand, it is clearly possible that the eye may be rendered less refrangible than when at rest, and thus adapted for more distant objects, or for converging rays; this would be *negative accommodation*. As we shall hereafter have principally to refer to positive accommodation, we shall always understand it to be positive when we merely employ the term accommodation.

Positive accommodation.—For every eye there is a limit nearer than which an object cannot be brought without its becoming indistinct, a limit called the *nearest point of distinct vision*, and another given by the eye when at rest, the *furthest point of distinct vision:* throughout the space between them the eye can see distinctly. What are the changes in the eye then? Since Kepler first ventured an explanation, this question has constantly occupied both physicists and physiologists. At different times an altered position of the lens, elongation of the antero-posterior diameter of the eye, contraction of the pupil, change in the form of the lens, have been supposed to furnish a sufficient explanation, either separately or combined.

Sixty years ago, Thomas Young* convinced himself that the power of accommodation depended on a change in the shape of the lens. As an hypothesis it was already older; Young was, however, the first to advance positive proofs in its favour. The experiments of Cramer† and Helmholtz‡ have fully confirmed this opinion, and have caused a general agreement§ on this subject. Helmholtz|| says:—

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* Philos. Trans. 1801, vol. xcii. p. 23; or Works, vol. i. p. 12.

† Het accommodatie-vermogen, &c., Haarlem, 1853.

‡ Archiv fur Ophthal., Band i. Abth. 2; or Physiol. Optik.

§ The reader will find a further account of these researches in Prof. A. Thomson's remarks 'On the Focal Adjustment of the Eye,' &c., Glasgow Medical Journal, vol. v. p. 50. 1858.

|| Gräfe's Archiv, loc. cit., p. 63.

"The changes in the eye which I have observed during its accommodation for near objects, are the following:

"1. The pupil contracts.

"2. The pupillary margin of the iris moves forwards.

"3. The periphery of the iris is thrown backwards.

"4. The anterior surface of the lens becomes more convex, and its vertex passes more forwards.

"5. The posterior surface of the lens becomes also a little more convex, but does not perceptibly change its position. The middle of the lens therefore becomes thicker" (its axis longer).

From this it appears that the lens is the essential organ in accommodation for near objects; indeed, this part would be superfluous except for that special purpose; an increase of the corneal curvature, or even other alterations, would have done all, except providing the eye with a transparent body capable of undergoing change in its form.

Accommodation for near objects is effected by increasing the curves of the two surfaces of the crystalline lens—i.e., by diminishing their radii; the vertex of the posterior surface of the lens remains *in situ*, that of the anterior passes forwards, and thus the space between the two becomes greater.

From the accommodation essentially depending on a change in the form of the lens, it would seem certain that it would be entirely, or almost entirely, lost after the operation for cataract. This, however, appeared to be contradicted by the well-known fact, that cases occasionally occurred in which after the operation print could be read near at hand, and at the same time distant objects could be perceived. Thus Arlt* says:

"A decisive proof of the falsity of all hypotheses that the accommodation depends on a change in the position or form of the lens, is afforded by the fact, that we occasionally meet with patients who have been operated on for cataract, and who with one and the same spectacles can see distinctly both near and distant objects, and in whom by experiment we can prove that they possess a greater or less power of accommodation."

He goes on to say that such cases have been referred to errors of the observer, to regeneration of the lens, or to the change in the form of the anterior part of the vitreous body. He quotes such cases from Home, Maunoir, Stellwag von Carion, and his own observation. He denies that during life the anterior surface of the vitreous body becomes curved forwards, and does not believe in the regeneration of the lens. Thomas Young† made some experiments, from which he decided that no accommodation remained. He says:

"It is unnecessary to enumerate every particular experiment, but the universal result is, contrary to the expectation with which I entered on the inquiry, that in an eye deprived of the crystalline lens, the actual focal distance is totally unchangeable."

And he points out the cause:

"It is obvious that vision may be made distinct to any given extent, by means of an aperture sufficiently small, provided at the same time that a sufficient quantity of light be left, while the refractive powers of the eye remain unchanged.[*] And it is remarkable that in those experiments (Croonian lecture for 1794), when the comparison with the perfect eye was made, the aperture of the imperfect eye only was very considerably reduced."

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* Die Krankheiten des Auges, Band iii. S. 327. Prag, 1856.

† Loc. cit., p. 46.

Gräfe[†] considered that he found a small range of accommodation in some cases, at the same time he pointed out that such cases possess a great power of enduring circles of dissipation, and hence that the utmost care must be used in forming our conclusions from such data. Donders[‡] says, that what we find stated here and there about considerable ranges of accommodation in aphakia, only shows that the authors had no clear idea of the degree of distinct vision even in imperfect accommodation. His investigations have convinced him that in aphakia *there is not the slightest trace of any power of accommodating.* It is in old persons with impaired vision that there appears to be a certain range of accommodation; whilst in the young with perfectly clear pupil and great acuteness of vision, in whom we might hope for some little power being left, we may readily convince ourselves of its absence. Thus,[§] he found in a young and intelligent man, that when a distant luminous point was distinctly seen by means of a certain lens, the addition of a lens of $\frac{1}{180}$ or $-\frac{1}{180}$ (formed by the combination of $\frac{1}{60}$ with $-\frac{1}{36}$, or of $\frac{1}{36}$ with $-\frac{1}{60}$), caused a striking change in the image; the patient constantly stated, that by the $\frac{1}{180}$ the object appeared changed in the vertical, by the $-\frac{1}{180}$ in the horizontal direction into a short line. He afterwards found that even glasses of $\frac{1}{300}$ or $-\frac{1}{300}$, caused a distinct change of form, whilst in changes of convergence (of the eyes) and efforts of accommodation no difference appeared. He appends the practical and important remark, that the entire absence of the power of accommodating, and the high degree of hypermetropia, render it desirable that the pupil should have a small diameter. The power of vision then remains tolerably good even when the accommodation is imperfect, and without spectacles both persons and objects may be clearly distinguished, although for perfect vision glasses of $\frac{1}{3\frac{1}{4}}$, or even $\frac{1}{3}$ are required.

Mechanism of accommodation.—What is the mechanism by which the surfaces of the crystalline lens are caused to change their curvature?[‖] Cramer concluded from his experiments that the increased convexity of the anterior surface of the lens is caused by the action of the iris, both circular and radial fibres being contracted at the same time, compressing the periphery of the lens, and thus pushing forwards the central portions. Cramer considers the use of the ciliary muscle only to be to prevent the lens passing backwards when compressed by the iris, and also to protect the retina. This was opposed by the observation of Gräfe, that in mydriasis the power of accommodating may be either lost or not; in the former case the pupil may again become mobile, and yet the paralysis of accommodation continue; or inversely, the pupil may remain immovable, and yet the power of accommodating the eye return. Szontagh,[*] who has the power of voluntarily dilating the pupil, accommodated for his nearest point of distinct vision equally well with his pupil varying from 3·9 to 7·1 mm. in diameter. In him the accommodation is evidently independent of the size of the pupil. Ruete[†] found good accommodation in a case of congenital absence of the iris; and, finally, a case has been recorded,[‡] in which Professor von Gräfe removed the whole of the iris, yet the accommodation remained almost normal.

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[*] This remark of Young's may serve to explain a mistake into which J. Z. Laurence has fallen, by confounding distinct vision from accurate accommodation with that caused by cutting off the circles of dissipation. See his paper, 'The Influence of the Variation of the Size of the Pupil on the Accommodating Power of the Eye,' Glasgow Med. Journal, vol. viii. p. 268. 1860.

[†] Beobachtungen über Accommodation bei Linsendefekt, &c. in Archiv für Ophth. Band ii. 1, 187. 1855.

[‡] Loc. cit. 93; or Archiv für Ophth., Band vii. 1, 168.

[§] Loc. cit., p. 170.

[‖] Donders, from loc. cit., p. 12.

The fact that it may continue normal in cases of congenital or artificial coloboma of the iris, in anterior and posterior synechiæ, although worthy of consideration, was, *per se*, insufficient to prove the absence of any participation of the iris in the process. Professor Gräfe made some very interesting experiments with atropine on his case just mentioned. Ten minutes after the application of a four-grain solution there was no change either in ordinary vision or in optometrical trials (with + 6 and + 10). After fifteen minutes the nearest point had considerably receded, the furthest point a little. With + 10 the former is at 5″ (previously $2\frac{3}{4}$″), the latter 11″ (previously 8″); hence the range of accommodation is reduced to $\frac{1}{9\frac{1}{4}}$ (previously $\frac{1}{5\frac{1}{3}}$). After twenty minutes the nearest point had receded much further, the furthest point remained stationary. After twenty-five minutes the furthest point was a trifle nearer, about $10\frac{1}{2}$″ (with + 10), the range of accommodation was reduced to $\frac{1}{40}$. One hour after the application it was entirely lost, or at all events reduced to a scarcely appreciable minimum. The eye was adapted for slightly convergent light, the hypermetropia $\frac{1}{70}$ to $\frac{1}{80}$. This state continued from four to five days, and then gradually disappeared, to the great satisfaction of the patient. There had been previously a space between the ciliary processes and the lens, and no change in their position could be perceived. A. von Gräfe hence concludes, that not only the processes of accommodation, but also the action of atropine, are independent of the presence of the iris. In both respects the tensor choroideæ must be the single active agent. As to the manner of action, no further deduction can be drawn from the position of the ciliary processes than that exact contiguity to the equator of the lens is not necessary. Donders[§] endeavoured to explain the accommodation by the conjoined action of the iris and ciliary muscle. He was the first to assign an important part to the ciliary muscle, that of forming a fixed point for the action of the radial fibres of the iris; he also referred a certain amount to the action of the iris. Helmholtz imagined that the zonula Zinnii, when tense, flattened the lens, and that it became relaxed by contraction of the ciliary muscle. Heinrich Müller[‖] distinguished two sets of muscular fibres in the ciliary muscle, external or radial, springing from the inner wall of Schlemm's canal, and passing outwards and backwards, to be inserted into both sclerotic and choroid, and internal or circular, running parallel with the corneal margin, and principally situated in the antero-interior part of the muscle near the insertion of the iris. These fibres have also been described by van Reeken, Rouget, Arlt,* and Henke.† Müller considers that the circular fibres when contracted must exercise pressure on the ciliary processes, ciliary margin of the iris, and thus on the margin of the lens, and that the longitudinal (radial) fibres cause increased pressure in the vitreous body. The posterior surface of the lens is thus impeded from passing backwards, and the effect of the pressure exercised on its margin is essentially limited to the anterior surface. These changes he considered to be favoured by the iris and zonula of Zinnii. Mannhardt‡ holds that the posterior insertion of the muscle forms the fixed point, towards which it draws the anterior portion. He does not deny, however, that the posterior part is also carried a little forwards. According to him, the ciliary muscle is probably the single *causa movens* of the mechanism of accommodation, causing a difference in the hydrostatic pressure in the anterior and posterior parts of the eye. The change in the form of the lens is effected by a tension and altered direction of the zonula Zinnii, the very opposite of the view generally adopted.

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[*] Ludwig, loc. cit., i. 286.

[†] Loc. cit., vol. ii. p. 682.

[‡] Ophthalmic Hosp. Reports, vol. ii. p. 199; or at length in Archiv, vol. vii. 2, s. 150.

[§] Ametropie, p. 12.

[‖] Archiv für Ophth., Band iii., Abth. 1, S. 1.

In 1848, Max Langenbeck published some remarks on the mechanism of accommodation, under the title, 'Musculus compressor lentis accommodatorius.' He states the important fact, afterwards confirmed by Cramer and Helmholtz, that the anterior surface of the lens during accommodation for near objects becomes more convex, and also that only as a consequence of that occurrence its centre passes more forwards—a fact previously observed, but erroneously explained, by Hueck. He then gives a description of a ring-like muscle around the margin of the lens, and ascribes to it, as was afterwards done by Müller, the power of increasing the convexity of the outer surface of the lens by pressure on its margin. In regard to the radial fibres, he says:

> "An advance of the ciliary body and lens towards the iris cannot be induced by the action of these fibres; on the other hand, they draw the ciliary body outwards from the margin of the lens, and thus flatten the lens, so that this muscle may not unjustly be considered as the antagonist of the sphincter capsulæ (a name assigned by him to the circular fibres), and to correspond to the radial fibres of the iris, with which it would contract at the same time."

These remarks appear to have attracted little attention when published. Dr. W. Henke§ has, however, recently brought them forward again, and defended them in an interesting paper. He considers both extremities of the radial fibres to be practically fixed, and hence that only their centre is moveable (somewhat like the diaphragm, &c.); thus, by their contraction they would draw the ciliary body outwards and flatten the lens. He supports the view that the eye in a quiescent state is not adapted for its most distant point, and hence that there are two varieties of accommodation, for near and for distant objects; he holds that the former is effected by the circular fibres, the latter by the radial fibres of the ciliary muscle, just as in the iris the circular fibres act during accommodation for near, and the radial for distant objects.

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* Zur Anatomie des Auges; Archiv für Ophth., iii. 2.

† Archiv für Ophth., Band vii., Abth. 2, S. 53.

‡ Archiv für ophthal. iv. 1, s. 269. See also Müller's remarks in the same volume, Band ii. p. 277.

§ Gräfe's Archiv, vi. 2, 53.

As regards the external muscles of the eye, Ruete observed cases which he considered to prove that the accommodation did not depend essentially upon them. Prof. Arlt* replied to these observations, and vigorously defended the muscles. The following observations by Prof. Gräfe† appear decisive. He found that when muscular paralyses were not accompanied by any affection of the pupil, the power of accommodation proceeded with perfect regularity. H. D—— suffered from paralysis of the third, fourth, and sixth nerves on the left side, so that the globe was motionless, except on closure of the lids, when it was moved by the contraction of the orbicularis. The pupil was slow in acting, yet the iris showed distinct changes during accommodation. With the healthy although presbyopic (hypermetropic) eye, and with convex 10, he accommodated from 5″ to 18″, with the left eye and the same glass from 6″ to 24″, and thus furnished a brilliant example that the exterior muscles may be perfectly inactive, and yet the accommodation continue. Another case related by the same writer is even more striking: "A man, about forty years of age, presented himself to me with perfect paralysis of the whole twelve ocular muscles, caused by an intra-cranial affection—probably a tumour at the base of the skull. Both globes were perfectly motionless, except so far as contraction of the orbicularis somewhat displaced them. The visual axes were slightly divergent. The patient had been formerly troubled with diplopia; now he employed only one eye at a time. Vision and the intellect perfectly unaffected, circumstances rarely to be met with in cases of such extensive paralyses, whether caused by intra-cranial or orbital diseases. Accommodation perfectly normal; he read No. 3 of Jäger's specimens of print from 4 to 16 inches, and letters 2¾ inches high and 1½ inches distant from one another, at 70 feet distance. Examined with the author's optometer, and compared with his own (A. von Gräfe's) and Dr. Liebreich's healthy eyes, he gave the following results:

	G.	L.	Patient.
Nearest point with unassisted eye . . .	4⅓″	... 3¾″	... 4″
„ with concave 24	6¼″	... 4¾″	... 6½″
„ „ 16	7¾″	... 5⅓″	... 9″
„ „ 12	10″	... 7¼″	...11″
Limits of accommodation with convex 10	3½—9⅓″	... 2¾—9¼″	... 3½—9¾″
„ „ 6	2¾—5¾″	... 2¼—5¾″	... 2¾—6″
„ „ 3	1½—2½″	... 1¼—2⅓″	... 1½—2⅓″

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* Loc. cit., Band iii. p. 209.

† Archiv für Ophth., Band ii. 1, 190; and the same vol., Abth. 2, p. 299.

The pupil did not change at all according to the intensity of the light, but markedly according to the change of accommodation. It should also be remembered, that not only have we the muscles paralysed, and the accommodation good, but we also have the opposite condition—the accommodation paralysed, yet the muscles perfectly healthy.

We may conclude that the accommodation of the eye for near objects is not dependent on the action of either the iris or external muscles of the eye; that it is effected by the action of the ciliary muscle, whose circular fibres by their contraction must tend to diminish the transverse and to increase the antero-posterior diameter of the lens; that the action of the radial fibres is still open to doubt—the most plausible explanation perhaps being that their anterior extremity is the most fixed, and that by their contraction they render the choroid, &c., tense, increase the pressure in the vitreous space, prevent the lens receding, and at the same time relax the zonula Zinnii.

The nerves which set in action and regulate this apparatus are unknown. Gräfe's case, in which the branches of the oculo-motorius to the muscles of the eye and iris were paralysed, and yet in which the range of accommodation was very slightly diminished, renders it probable that this nerve is not the one. The rapidity with which this process is performed is not very great. If the eye after being adapted for a very distant object is suddenly accommodated for its nearest point, the change is sufficiently gradual to be very distinctly perceived.

Negative accommodation.—By this term, as already mentioned, we mean that the eye is rendered less powerfully refracting than when at rest; or, in other words, that it is accommodated for less divergent rays, or for more distant objects. The ideal eye, in a state of negative accommodation, would be fitted for only convergent rays—i.e., for visual images, placed at a greater or less distance behind the eye. On the other hand, if, as we often find in practice, the eye when at rest is accommodated only for objects placed at a certain finite distance, then the effect of negative accommodation would be to render more distant objects visible. The best reason for believing in this negative accommodation is the subjective sensation of exertion experienced when the eye is accommodated for its most distant point, or in many cases for visual images; the eye soon becomes fatigued, which would scarcely be the case if the eye were at rest when looking at such objects. This change is very possibly caused by the united action of the recti, obliqui, and orbicularis muscles: at least, in the effort to see very distant objects the brow is often seen to become wrinkled, and there is a feeling of general tension in the orbits.

Formula for the range of accommodation.—Supposing that the nearest and furthest points of distinct vision are discovered, it becomes of importance to express in some simple manner the range of accommodation, so that we may readily note it for future reference or comparison.

In the accommodation for near objects two changes might be imagined; either that the refracting power of the eye is increased, or that the optic axis is lengthened. In the latter case we should have to determine how much the lengthening amounted to; in the former, which is the conclusion to which we have previously arrived, we must determine the increase in refracting power. This may be done by our imagining that to the eye adapted for its most distant point, a positive lens of such a nature is applied, that rays from an object placed at the nearest point of distinct vision would be so refracted that they would seem to proceed from the furthest point. The focal power of this lens will be a measure of the accommodation, may be expressed in figures, and thus may be readily employed for purposes of comparison, &c. This focal power may be easily calculated by the formula $\frac{1}{a} = \frac{1}{p} + \frac{1}{r}$, wherein p indicates the distance of the nearest, and r that of the furthest point; it must, however, be remarked that r becomes negative if, as in the normal eye, both the nearest and furthest points are placed on the same side of the lens. Thus if both p and r are positive with reference to the eye, our formula will be $\frac{1}{p} - \frac{1}{r} = \frac{1}{a}$.* One or or two examples may render this clearer. Let the distance of the nearest point $p = 4''$, that of the furthest point $r = 12''$, then the range of accommodation A is

$$A = \tfrac{1}{4} - \tfrac{1}{12} = \tfrac{1}{6}.$$

If $r = \infty$, $p = 3''$, then we have

$$A = \tfrac{1}{3} - \frac{1}{\infty} = \tfrac{1}{3}.$$

The range of accommodation may also be expressed graphically by means of lines, the beginning and end of each line representing the nearest and furthest points of distinct vision, and their length that of the range of accommodation.

For this method of determining the range of accommodation we are really indebted to Professor Donders, who has pointed out its great value. Young† had, however, long since, made use of a similar method, as is pretty evident from the following extract: "The faculty of accommodating the eye to various distances, appears to exist in very different degrees in different individuals. The shortest distance of perfect vision in my eye is twenty-six tenths of an inch for horizontal, and twenty-nine for vertical rays. This power is equivalent to the addition of a lens of four inches focus. Dr. Wollaston can see at 7″ and with converging rays; the difference answering to 6″ focal length. Mr. Abernethy has perfect vision from 3″ to 30″, or a power equal to that of a lens 3⅓″ in focus," &c.

Relative range of accommodation.—It has long been noticed that there is an intimate relation between the convergence of the visual lines‡ and the refractive condition of the eye. Thus, in the vision of near objects, the visual lines converge, and this prevents the eye from accommodating for its most distant point. To accommodate for the most distant point of distinct vision, the visual lines must be parallel; and inversely, to accommodate for the nearest point, the eyes must forcibly converge. This relation is, however, far from absolute. Thus Donders* points out, that any one with normal eyes and good accommodation, can see singly and at the same time distinctly an object at the distance of a couple of feet with or without glasses of positive or negative nature, provided they are not too strong; without or with prismatic glasses, which must also not be too strong, and must have the refracting angle turned inwards: and yet in the latter case there must be a change in the degree of convergence, whilst the accommodation remains the same; in the former the accommodation changes, the convergence remains the same. The two are indisputably shown to be independent of one another at least to a certain extent, which indeed is not very great, as we shall soon see. For example, we can exert about one-third of our power of accommodating whilst the visual lines are parallel.

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* According to this supposition, p is the focus of origin, r that of refraction; the latter being a visual focus, must be designated by a negative sign, according to the convention previously mentioned.

† Loc. cit., p. 36.

‡ The visual line is that connecting the point fixed with the macula lutea. As it deviates to some extent from the optic axis, Helmholtz has given it a specific name.

Donders has determined for each degree of convergence of the visual lines the corresponding range of accommodation, which he calls the "relative range of accommodation." Fig. 2 shows the range of accommodation for each degree of convergence in the normal eye:

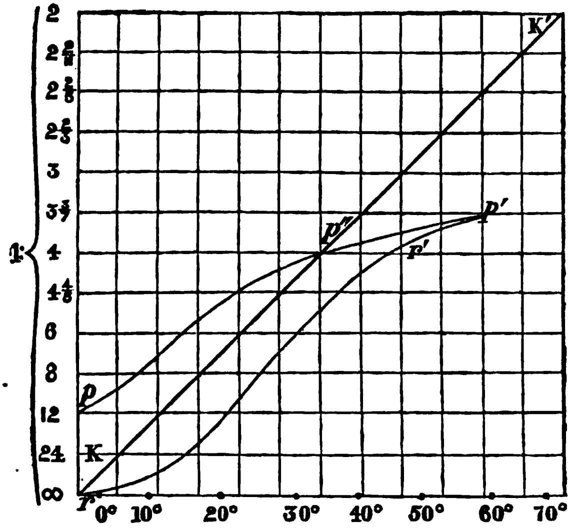

Fig. 2. From Donders' Ametropie, p. 37.

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* Archiv für Ophth., Band iv., Abth. 1, S. 303.

K K′ represents the convergence of the visual lines from 0° to 70°; *p p′*, the course of the nearest point of distinct vision; *r r′*, that of the furthest point; the figures 2, 3, 4, &c., placed at the side of the figure, are the distances (in Paris inches) at which the visual lines intersect before the eye (the distance between two horizontal lines $= \frac{1}{24}$ accommodation); the figures placed below indicate the angles of convergence of the visual lines corresponding to the distances, the distance of the eyes from one another being estimated at 28½ lines; the space between the lines *p p′* and *r r′* measured vertically indicates the (relative) range of accommodation for each degree of convergence. The portion of this space placed above the line K K′ is the positive, that below is the negative range of accommodation: the former shows how much nearer, the latter how much further, can be accommodated, than the point towards which the eyes converge; *p″* is the nearest point of distinct vision for both eyes together, consequently the nearest point of convergence at which distinct vision is possible; *p′* is the absolutely nearest point of distinct vision, which is almost always further from the eye than the corresponding point of convergence. At this point we have both the greatest possible convergence and at the same time the utmost accommodation, so that the lines *p p′*, *r r′*, here intersect, hence there is here positively no range of accommodation. On examining the figure, we see that in the normal eye, *p p′* and *r r′* ascend pretty regularly as far as an angle of twenty degrees, after which they both, but especially *p p,′* ascend more slowly. It must be remarked that this figure is a diagram planned from a number of individual observations.

The range of accommodation for both eyes extends from infinite distance to 4″, and hence amounts to ¼. Its absolute range is $\frac{1}{24}$ greater (for *p′* is separated from P″ by a space $= \frac{1}{24}$); it is $= \frac{1}{3\frac{1}{4}}$. When the angle of convergence = 0 (the visual lines parallel) the relative range $= \frac{1}{12}$ (for the furthest point is at ∞, the nearest at 12″, or in other words = ⅓ of the range of accommodation of the two eyes together). The relative range increases a little at 5°, 10°, and 15°, then again diminishes so that at an angle of 28° it amounts to $\frac{1}{12}$ again, and at 34° is reduced to $\frac{1}{20}$. From there it rapidly diminishes, until it = 0 at 60°. We should, besides, notice that the relative range of accommodation is at 0° of convergence entirely positive, is at 10° nearly equally divided into positive and negative; that this continues much the same to 22°, from which point, however, the positive A rapidly diminishes, becoming at 34° (= to a distance from the eye of 4″) entirely negative.

Influence of age on the accommodation and refraction of the eye.—As the age of man increases,* the eye undergoes a series of gradual changes: the cornea becomes less brilliant, the pupil smaller, the anterior chamber less deep, the arcus senilis appears, &c; anatomical examination shows various alterations in the internal membranes, such as those of the hyaloid, calcareous deposits in the sclera, pigmentary and other changes of the choroid, atrophy of the ciliary muscle, and increased solidity of the lens. Even before such changes can be discovered, the ophthalmoscope shows that the media become less transparent as the years advance—a fact especially noticeable on comparison with the beautifully clear fundus oculi in the child. The power of accommodating is much earlier diminished than that of refracting; the position of the most distant point of distinct vision long remains the same, whilst that of the nearest point becomes gradually more and more distant from the eye: the range of accommodation is diminished. A common error is to suppose that this change in the position of the nearest point only commences after the fortieth year. At this age, it first begins to interfere with vision, and attention is first attracted to it; in reality it has commenced during the years of youth, even before puberty.

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* Donders, loc. cit., 68; Gräfe's Archiv, Band vi., Abth. 2, S. 210.

This change affects all eyes; the myopic, although in some cases the progressive nature of the disease may prevent its being apparent, the hypermetropic, and the normal eye.

At first sight it appears strange that the power of accommodating, a muscular function, should begin to diminish even in youth, whilst all the other muscles are increasing in strength. Donders considers this to be most probably caused by a gradual increase in the solidity of the lens.

The refracting power of the eye also begins to gradually diminish, after the power of accommodating has been already much lessened. Hence the furthest point of distinct vision removes to a greater distance from the eye, and of course the second principal focus passes further backwards, even behind the retina. As already observed, this change in the power of refracting is observed only in advanced age. At forty years of age, it has not at all, or only just commenced: it is not until the fifty-fifth or sixtieth year that it is distinctly present; a positive glass then becomes necessary for seeing even distant objects. In this respect there are, however, great individual differences: the diminution of refracting power is often so slight, that it is only proved with difficulty; sometimes no change can be perceived, or, on the other hand, it may be very marked.

What is the cause of this change in the refracting power? It is probably owing to increased density of the cortex of the lens; the researches of Young, Senff, Listing, &c., have shown that a lens of homogeneous structure, even if it have the refracting power of the nucleus, will have a longer focal length than the normal one with its lamellated walls; besides, in advanced age the lens seems to become flatter. Donders has been unable to find that the cornea became less curved, or that the optic axis became shorter, and hence believes that this result must be entirely attributed to the lens. This view is favoured by the fact that ultimately the diminution of refracting power proceeds proportionately to that of the accommodation; thus appearing to point out a common cause for both. Fig. 3 shows the position of the nearest and furthest points of distinct vision, and of course at the same time the range of accommodation in the normal eye at various periods of life.

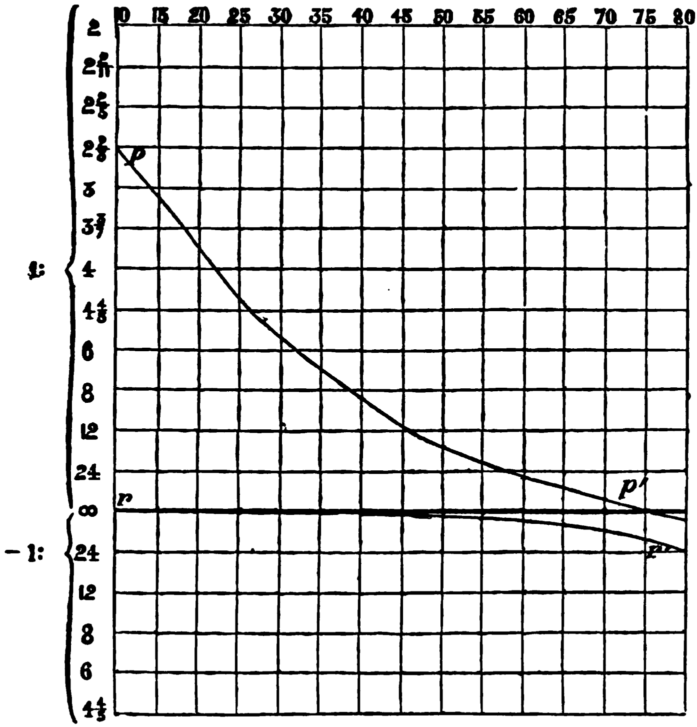

Fig. 3. From Donders' Ametropie, p. 63.

The numerals placed at its left side give the distance measured in Paris inches for which the eye is accommodated; those placed below ∞ have of course a negative value, for they give the distance at which the converging rays, for which the eye is accommodated, come to a focus behind the optical centre of the eye; *p p′* gives the course of the nearest point, *r r′* that of the most distant point; the numerals placed above the figure denote the years of life. Thus on the lines *p p′* and *r r′*, the nearest and furthest point of distinct vision can be found for each year of life, and the distance between these two lines at the same time indicates the range of accommodation; the distance between two horizontal lines is equal to $\frac{1}{24}$ range of accommodation. The figure shows that from the tenth year, the earliest period at which we can exactly determine the accommodation, the nearest point continually recedes from the eye, at first with tolerable regularity, at thirty years the range of accommodation being only about half of that at ten. From this period it seems to recede somewhat less rapidly, although continuously until the termination of life. The course of the most distant point is quite different.

Until the fortieth year it remains at the same distance from the eye, it then commences to very slowly recede, so that the eye originally normal has become somewhat hypermetropic at fifty years. From the seventieth to the eightieth year of life, the hypermetropia becomes nearly equal to $\frac{1}{24}$ accommodation; ultimately it becomes absolute,* so that the eye becomes unable to accommodate for even parallel rays.

On measuring and noting the accommodation.—No point in ophthalmology has been perhaps so generally misunderstood as this; and even in the most recent works we find little more than some fragmentary remarks on the subject.† The first effort of any importance was that of Porterfield, who founded on the experiment of Scheiner an optometer which was afterwards modified by Young, &c. All the instruments of this form have proved failures. At the utmost they show some of the points for which the eye can accommodate, and not the limits, the nearest and furthest points of distinct vision. Many practitioners, such as Stellwag, convinced of the worthlessness of these instruments, relied entirely upon print-tests; Gräfe and Donders introduced into more general use the application of convex lenses, which indeed had been employed already by Thomas Young for the same purpose, but which unfortunately had had the same fate as many of his other discoveries, that of being misunderstood or neglected.

A. von Gräfe made an important step in diffusing some knowledge of this subject, when, in the 'Archiv,'‡ he wrote, "For determining these points, I have abandoned the use of Stampfer's optometer, or of similar instruments founded on the principle of Scheiner's experiment, because, as already often pointed out, they are found in practice to be extremely defective. When an object is regarded through a closed tube, its distance cannot be appreciated, and when this *point d'appui* is wanting, it requires a great degree of delicacy or of practice in accommodating, to know whether the condition of refraction is too strong or too weak, a power not always possessed by healthy, much less by abnormal eyes. This may explain considerable errors and deviations in the results. Besides, any exact accommodation is always dependent upon the intention of perceiving the forms of objects, so that the judgment may thence obtain precise ideas; this design is, however, carried out in an extremely imperfect manner with the optometers just mentioned, for in their application the point in question is essentially different from recognising the forms of objects. There are also certain difficulties in the employment of these instruments in ignorant patients, arising especially from portions of the light which pass through the various splits of the ocular not all falling together into the pupil.

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* Donders has divided hypermetropia into three forms: facultative, where the range of accommodation is unusually large, the nearest point being at its normal place; relative, where the loss by the removal of the near point is equalized by the removal of the furthest point, so that the total range of accommodation remains normal; and absolute, where the eye is only able to accommodate for converging rays.

† Perhaps it may be well to quote an example. J. V. Solomon, on Myotomy of the Ciliary Muscle, Med. Times and Gaz., 1861, i. p. 54, &c., may serve as such, all the more because the paper having special reference to our subject, it is much lessened in value by the insufficient manner in which the cases related were examined. Mackenzie's large work may also be cited as another proof.

‡ Archiv für Ophthalmologie, Band ii. Abth. 1, S. 160. Berlin, 1855.

"Owing to the unsatisfactory results which proceed from the employment of these optometers in practice, some have abandoned their use altogether, restricting themselves entirely to type tests, and others have invented instruments founded on other principles than those on which Scheiner's experiment depends. I for my part prefer that the conditions of recognition in these trials should as closely as possible resemble those occurring in ordinary vision, that the patient should be perfectly conscious of the distance of the object, and that the circles of dissipation should not be suppressed. The two former conditions are best fulfilled by the ordinary reading tests; to the latter we may approximate by choosing as the tests very fine objects, placed close to one another, for then the circles of dissipation sooner interfere with vision than when the objects are of some size. For this reason the ordinary reading tests are accurate, inversely as their size; of course, owing to the smallness of the visual angle, the range of accommodation must be sufficiently approximated by means of convex glasses. This is best seen in presbyopic and hyperpresbyopic persons who can read considerably beyond their limits of accommodation, large or moderate, but never very fine print. Yet it cannot be denied, that even in the recognition of the finest print the circles of dissipation are to some extent suppressed, and that this power may be increased by exercise. For this reason I have adopted a plan founded on the last-mentioned principle (small visual angle of the objects, or more properly, of the spaces between them), the test object being somewhat changed. In a small frame of about an inch square, there are numerous threads as fine as possible, and placed at regular distances from one another, so that they present a delicately-striped appearance. A very important point is the fineness of the threads, which for the purpose of obtaining very exact results as to the limits of accommodation, should be formed of the finest metallic wire. When such an instrument is taken in the hand, and held before the wall of a house or the sky, it is only possible to see the threads distinctly when they are exactly in focus; so soon as the accommodation fails, the well-known coloured double images of the threads appear, and more or less fill the spaces between them, so that the distinctly-marked stripes are lost, and the whole image appears confused. This plan is pretty nearly the same as that used by Coccius (see his book on the ophthalmoscope), only that the wires are more delicate, and besides, the luminous background seems to me of importance, because it renders the double images more distinct. Such frames are moveable on a graduated rod, of which one end is provided with a plate and applied to the patient's forehead. The general agreement of the statements obtained from ignorant patients convinced me of the practical utility of this little instrument, which I have since employed in cases where there were most various affections of the accommodation."

By far the best account of the method of measuring the accommodation, is that given by Mac-Gillavry in his work already cited, a work unfortunately far too little known, and of which we shall make free use. Donders, Gräfe, Stellwag, Jäger, have all written more or less on this subject, and may be read with benefit.

Ordinary print has been much used as a test object; great care must, however, always be used to employ it as small as possible, and thus it must of course vary with the distance at which it is to be placed; for example, in the case of the normal eye unaided by glasses, we may use No. 1 of Jäger's print for the nearest point, and No. 18—20 for the most distant, say at twenty feet.

In this way we may determine both limits of distinct vision in very short-sighted persons; in the great mass, however, it is necessary to employ glasses to determine the furthest point, and they are not unfrequently of some use in determining the nearest point, at least as a further corroboration of statements. If the object be No. 18 or 20 of Jäger's specimens, placed at twenty feet distance, and it be seen with perfect distinctness, it is clear that the eye can accommodate for parallel rays;* it does not, however, show that it cannot accommodate for converging rays. This may be decided by weak convex glasses; if these, although very weak (+ 60 – + 80″) deteriorate the vision, it is evident that the eye cannot accommodate for any point more distant than ∞. Should† a positive lens be necessary for the distinct vision of distant objects, then the focal length of this lens, minus its distance from the anterior surface of the crystalline lens, gives the true point of accommodation. On the other hand, if negative glasses are necessary for seeing the same test objects, the distance of the glass from the crystalline lens must be added to the focal length of the lens to find the most distant point of distinct vision. The reason of this proceeding is, that parallel rays, after being refracted by a lens, converge or seem to diverge from its principal focus, and thus it is as if the object were placed at the same point, at least in the case of negative lenses.

There is greater difficulty in determining the nearest point; as already explained, the degree of convergence of the optic axes influences the refractive condition of the eye. Mac-Gillavry determined the furthest point when the optic axes were parallel; the nearest point in the myopic, normal, and slightly presbyopic eye without the use of any auxiliary lens, and thus with such a degree of convergence of the optic axes, that it was still possible to see distinctly; whilst presbyopic eyes were examined with positive glasses sufficiently strong to render the angle of convergence tolerably like that occurring in the average of normal eyes, in persons of the same age.

From the values obtained by means of glasses, the true ones may readily be calculated; so far as concerns distant objects having parallel rays, this has already been explained. In other cases we may find them by the following method:—

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* At least in a practical sense; the circle of dissipation would have a diameter of 0·0112 mm. in the ideal eye for this distance; of course, greater accuracy may be obtained by employing larger type at a greater distance.

† Mac-Gillavry, loc. cit., p. 9.

The focal length of the auxiliary glass . . . . $= f$.
The distance between the glass and the crystalline lens . . . . . $= k$.
„ „ object . . . . $= p$.
„ „ point from which the rays seem to proceed, or towards which they converge after refraction $= p'$.

f positive.	f negative.
$\frac{1}{p}+\frac{1}{p'}=\frac{1}{f}$	$\frac{1}{p}+\frac{1}{p'}=-\frac{1}{f}$
$p'=\frac{pf}{p-f}$	$p'=-\frac{pf}{p+f}$

If, in the case of using positive glasses, f is greater than p, then the value of p' becomes negative with reference to the lens, yet it remains positive in regard to the eye; if p is greater than f, the contrary takes place. Since k is always positive in regard to the eye, the value of k must in the former case be added to p', in the latter subtracted from p', in order to find the true point of accommodation. The negative glasses are rarely employed. It is advisable not to use too strong glasses; to the use of strong positive glasses there are two objections—one, that they magnify the object when it is placed between the lens and the principal focus, which is almost always the case with the nearest point of distinct vision, when, owing to the increased size of the image, the circles of dissipation are not so soon perceived; and the other, that the difficulty of exactly measuring the spaces is much increased. How readily results, more or less erroneous, may be found when powerful glasses are employed, will appear from comparing the following results of observations made by Prof. A. V. Gräfe on his own eye:*

With convex 10″ he found a range of $3\frac{1}{2}-9\frac{1}{2}$ = without glasses $5\frac{6}{13}-190''$
„ 6″ „ $2\frac{3}{4}-5\frac{3}{4}$ = „ $5\frac{1}{13}-138''$
„ 3″ „ $1\frac{1}{2}-2\frac{1}{2}$ = „ $3-15''$

Or, in the first case, a range of accommodation $=\frac{1}{5\frac{1}{2}}$ nearly.
„ second „ „ $=\frac{1}{5\frac{1}{4}}$ „
„ third „ „ $=\frac{1}{3\frac{3}{4}}$ „

Similarly, in a case of a normal eye which we recently examined without glasses, with + 10 and with + 6, we found a total range of accommodation of $\frac{1}{4\frac{1}{2}}$, of $\frac{1}{4}$ nearly, and of $\frac{1}{3\frac{3}{4}}$; it is scarcely necessary to mention that the first is the correct result. In practice, we find many hypermetropic cases which cannot entirely relax their accommodation —i.e., with parallel optic axes, they continue to exert a certain amount of their accommodation. In these cases we must, before determining the most distant point, paralyse the ciliary muscle by the use of a solution of atropine of sufficient strength (four grains to the ounce); it should be applied some time before the examination, for the first effect of the mydriatic is only to dilate the pupil; to paralyse the ciliary muscle takes a considerably longer period.

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* Archiv für Ophthalmologie, Band ii., Abth. 2, S. 301; the distance of the eye from the glass is not given.

The method we have found practically most convenient during the examination of a case is, after the usual objective examination, to determine the position of the near point without and with glasses, the minimum visibile, the field of vision, then to apply the atropine, and whilst it is acting to note the history of the case, &c.; finally, after a sufficient lapse of time, measure the most distant point, and, if necessary, employ the ophthalmoscope.

In the notes of cases, the following facts should always be given: How near and how far off the smallest type can be distinctly read with the unaided eye, the same points when provided with + 10, and the position of the furthest point of distinct vision after the use of atropine, or more generally, the position of the nearest and furthest points of distinct vision and the method used in their determination: there is always reason to doubt the accuracy of the observation, if the process of examination is not mentioned.

In conclusion, it may be well very shortly to recapitulate the following points:—

The object must be as small as possible, reference being made to the distance from the eye at which it is to be placed.

The illumination of the object should always be the same—ordinary daylight is the best.

Before determining the most distant point, it is generally advisable, and in many cases indispensable, to employ atropine.

The most distant point may be found by Jäger's print, Nos. 18—20, at twenty feet distance, only weak glasses being used; or by small print (Nos. 1—3), held in the patient's hand, and the use of a sufficiently strong glass (+ 10″).

The nearest point may be found by the unassisted eye, and No. 1 of Jäger's print held in the patient's hand, or by the use of glasses, positive or more rarely negative.

From the values obtained with glasses, we may, by calculating their conjugate foci, find values equivalent to those which would be obtained without glasses for the same points.

REVIEW II.

1. *Diphtheria: its Symptoms and Treatment.* By WILLIAM JENNER, M.D., Special Professor of Clinical Medicine, University College; Physician to University College Hospital, &c.—*London*, 1861. 12mo, pp. 107.
2. *On Diphtheria.* By EDWARD HEADLAM GREENHOW, M.D., Fellow of the Royal College of Physicians; Physician to the Western General Dispensary; and Lecturer on Public Health at St. Thomas's Hospital.—*London*, 1860. pp. 274.
3. *Second Report of the Medical Officer to the Privy Council.* 1859.
4. *De la Paralysie Diphthérique; Recherches Cliniques sur les Causes, la Nature, et le Traitement de cette Affection.* Par le Docteur V. P. A. MAINGAULT, Ancien Interne Lauréat des Hôpitaux de Paris, &c. —*Paris.* 8vo, pp. 161.

On Diphtheritic Paralysis. By Dr. MAINGAULT.

5. *A Paper on Diphtheria, read before the New York Academy of Medicine.* By JAMES WYNNE, M.D., Lecturer on Legal Medicine in the Medical Department of the University of the City of New York.—*New York*, 1861. pp. 32.
6. *Études Pratiques sur l'Angine Couenneuse.* Par le Dr. JUGAND.—*Paris*, 1861. pp. 100.

Practical Studies on Membranous Angina. By Dr. JUGAND.

THE four works which stand first on our list may be regarded as fairly representing the most recent advances in our knowledge of the disease to which they relate. In Dr. Jenner's lectures the essential characters of diphtheria are defined in clear and forcible language founded on cautious observation and shrewd discrimination of facts. M. Maingault has devoted himself to a special investigation relating to the remarkable functional disorders of the nervous system which assume so distinctive a development as consequences of the disease; while Dr. Greenhow has applied the large opportunities afforded by his local inquiries respecting the recent prevalence of diphtheria in various parts of England in the study of its etiology, mode of diffusion, and special epidemic characters. Dr. Greenhow and Dr. Jenner have approached the study of the same subject from widely different directions. The former has had before him diphtheria as a rural pestilence, sweeping away at one stroke the whole infantile population of an isolated moorland hamlet, or decimating a secluded and previously healthy village—so secluded, indeed, that every circumstance bearing on the mode of its invasion and progress could be easily ascertained. The latter has seen the disease, for the most part, in the great metropolis, where its epidemic character was lost and the mode of its diffusion could not be traced, owing to the complexity of the influences by which it was modified or determined. Notwithstanding this difference between the fields of observation in which these two writers have principally laboured, it is gratifying to find a remarkable agreement, both as to fact and inference, between them. Of this we shall best enable the reader to judge by putting him in possession of what our authors have to say on the most important questions relating to diphtheria which have been mooted since its first appearance in 1856.

Nosological Definition of Diphtheria.—Both author's insist on the importance of regarding diphtheria as a disease having a twofold aspect, general and local; its essence being made up not merely of those anatomical changes from which it derives its name, but of a specific constitutional state, the characters of which are as peculiar to the disease as the morbid lesions with which they are associated, and cannot be disregarded in forming a conception of it without rendering that conception incomplete and inadequate.

As might be expected from a teacher of practical medicine, Dr. Jenner defines the disease more dogmatically, and therefore more satisfactorily to the general reader who seeks instruction, than Dr. Greenhow. He tells us that "diphtheria is a general disease, having exudative inflammation of the pharyngeal mucous membrane for its anatomical character." Passing by for the present the criticism which this definition suggests—viz., that the pharyngeal lesion, although the most frequent anatomical manifestation of diphtheria, is by no means the only one—instance those cases in which the mucous membranes remain unaffected, the "exudative inflammation" being confined to the skin—we proceed to his further exposition of the two "diseases" in which the one infection manifests itself: "The general disease varies in its character from sthenic febrile to typhoïd febrile, but always has a tendency to assume an asthenic type. . . . The local nasal, pharyngeal, and laryngeal disease is inflammatory in nature, the inflammation varying in character from sthenic to asthenic, but always showing a tendency to become asthenic." These words, in addition to the clear distinction they contain between the "fever" and "inflammation" of which we are taught that the disease is constituted, point to another character as belonging to it in both its aspects. Both the general morbid condition and the local phlegmasia are subject to marked variations; and these variations are, as regards both, in one and the same direction—namely, from sthenic to asthenic or typhoïd—in accordance with which variations the cases of diphtheria may be arranged in a series, at one end of which would be found cases marked by the general and local signs of adynamia, at the other, cases rather distinguished by the absence of these signs than by the presence of any opposite tendency.

In a former review of works on diphtheria (January, 1860), we endeavoured to enforce the necessity of bearing in mind, as a special character of the disease, the inconstancy of its development, as affording the only explanation of the apparent contradictions which present themselves in the descriptions and definitions even of the most exact and trustworthy observers. We found that every physician whose experience of diphtheria had been large, was led sooner or later to distinguish various modifications of the disease, differing from each other not merely in their relative severity and the degree of danger attending them, but in the mode of attack, the progress of the symptoms, and the direction from which danger was to be apprehended. In the writings of Isambert, Trousseau, Becquerel, and others, we found plain indications of the existence of two types of diphtheria, which may be properly distinguished as septic and membranous; the one of which, in its most marked expression, is characterized by its tendency to rapid extension and repeated reproduction of the exudation, by the integrity of the subjacent mucous membrane, and by the mildness of the constitutional symptoms; the other locally, by the softening and disintegration of the submucous tissue—constitutionally, by all the symptoms of septic fever. As, however, these two forms are by no means specific, and are capable of being combined in all possible proportions, it will depend on accidental circumstances, and particularly on the characters which have happened to present themselves most frequently within the range of each observer's experience, whether he is led to distinguish two or a greater number of typical forms, the important fact being, that every classification is based on the presence or absence of the so-called typhoïd condition, and each case takes its place in the series according to the intensity assumed by the group of symptoms of which that condition is made up. Thus we find Dr. Jenner dividing his cases in respect of the general morbid diathesis into two categories, including in the first all the varieties of diphtheria in which danger is to be apprehended from the existence of exudative inflammation affecting the larynx; and in the second, those in which "the patient, when the disease proves fatal, dies from the general disease—'asthenic diphtheria.'" The first section admits of further distinction into "inflammatory diphtheria," and other forms—viz., the "insidious," the "nasal," and the "primary laryngeal," which occupy, as regards the general state of the patient, an intermediate position between the inflammatory and the asthenic forms, while they are distinguished from each other entirely by differences in the seat and extent of the local lesion. We will endeavour to place before the reader the precise grounds on which these distinctions are founded.

Inflammatory Diphtheria.—The disease resembles in its mode of attack the ordinary form of tonsillitis—the cynanche tonsillaris of authors. There is "redness and swelling of the mucous membrane covering the arches of the palate, the uvula, and the tonsils. The redness is in some cases vivid and in others dusky;" and the membrane has "that peculiar gelatinous aspect which indicates submucous serous effusion." Along with this there is febrile disturbance, which "may be extreme or moderate. The pulse is frequent, but soon becomes weak; there is considerable sense of weakness and of illness. From twelve to forty-eight hours from the first symptoms of throat affection, a layer, more or less extensive, of tough lymph coats the inflamed surface, and when death follows, it does so from the extension of the exudative inflammation to the larynx, trachea, &c."

Thus the inflammatory form of diphtheria is clearly enough characterized by its anatomical peculiarities, by the intensity of the previously existing signs of inflammation, by the toughness of the membranous concretion, and by its tendency to extend rapidly from the pharyngeal to the respiratory mucous membrane. But the general symptoms are rather negative than positive. There is an absence throughout of marked constitutional disturbance, the slight and transitory pyrexia observed at the commencement being scarcely sufficient to justify the term "sthenic febrile" applied to these cases by our author.

Asthenic Diphtheria is characterized by Dr. Jenner as follows:

> "In this form the disease begins sometimes with general and local symptoms of moderate severity. Soon, however, the pulse is rapid and feeble, the sense of weakness and illness extreme; the skin is not very hot, but there is a peculiar feverish pungency in its heat as appreciated by the touch. The complexion has that dirty-looking pallid and opaque aspect which we see in so many general diseases. In some cases, from an early period of the disease, the brown tongue, the sordes on the teeth, &c., and the muttering delirium, which are so characteristic of the so-called typhoïd condition, are present. On examining the throat, more or less lymph is seen on the pharyngeal mucous membrane. The lymph, in these cases, has always, in my experience, been of the granular pulpy or softer form. The patient may swallow with perfect facility, and the throat-symptoms be trivial in degree, and this even when the pharyngeal mucous membrane is covered with lymph. In other cases, the pain in deglutition is extreme. The extension of the exudative inflammation to the larynx, when it occurs, is shown by a little huskiness and want of power in the voice, and imperfectly marked laryngeal breathing. The patient usually dies in about ten or twelve days, death being the result, not of apnœa, but of asthenia. It is the failure of the heart's action, and not want of breath, that causes death." (p. 30.)

Every one who has seen much of diphtheria as it has prevailed in this country, will recognise the accuracy of this comprehensive picture of its development and mode of termination. There can be no doubt this is the form of the disease which has most generally prevailed throughout the rural districts of England. For however valueless such an investigation as that undertaken by the Privy Council may be on matters of exact pathological research, the information obtained by it—embodying, as it does, the experience of a large number of medical men—is perfectly satisfactory in relation to the *progress* and *mode of termination* of a disease. In respect of these points, the cases and observations contributed in so disinterested a manner by rural practitioners coincided completely with the "asthenic type" of Dr. Jenner.

The passive yielding of the energies of life to a general poisoning, which is characteristic of this common form of diphtheria, contrasts strikingly enough with the violent struggle implied in every symptom of those exceptional cases which Dr. Jenner designates as "febrile sthenic." On the other hand, it is to be distinguished from another class of cases referred to by all authors—e.g., by Isambert, under the name of angina diphtheritica maligna; by Becquerel, under that of angina gangrenosa; and by others under that of septic diphtheria, to which Dr. Jenner does not fail to assign a place in his classification.

> "There is yet another set of cases in which death appears to result from the evils consequent on the absorption of fœtid matters from the pharyngeal tissues. The pharynx is covered with lymph, the mucous membrane below sloughs, the breath is very offensive, the glands about the angles of the jaw swell extremely, the cellular tissue in which they are embedded is the seat of the effusion of serosity, the skin assumes that dirty-yellowish tint which it has in septicæmia, the mind wanders, and the patient rapidly sinks." (p. 35.)

This most malignant form of diphtheria has been already graphically described by M. Perrochaud in Boulogne, and by Trousseau as it appeared in Paris a few years later, being fatal to Valleix and Blache. In England the examples of it have been rare, but it has been occasionally met with in Lincolnshire, Kent, and other rural districts.

Pathological anatomy.—Dr. Jenner distinguishes two varieties of diphtheritic "exudation," one of which is "very tough and elastic, and as much as one-eighth of an inch in thickness, resembling wash-leather;" the other, grey, pulpy, or creamy; the former, consisting for the most part of "such fibres as we see in the buffy coat of blood coagula;" the latter, pus, pyoïd corpuscles of Lebert, and other smaller and larger granular corpuscles, epithelium, and oleo-protein granules. Dr. Jenner teaches that these two forms of concretion are severally related, the latter with the asthenic, the former with the so-called inflammatory types of the general disease. This relation has been already elucidated by Dr. Sanderson in the pages of this journal; it is an example of the general truth in pathology, that an exudation, while retaining its own specific identity, may be so modified by the constitutional state of the subject as to exhibit in different cases great differences of aspect.

Dr. Jenner abandons the doctrine which has been so often repeated after Bretonneau by authors, that the mucous membrane remains intact in diphtheria, and refers repeatedly to ulceration and sloughing, particularly of the tonsils and uvula. Neither he nor any other writer appears to have observed ulceration in the larynx. This is the more remarkable, as it constitutes the distinctive feature of that disease which is described as diphtheria by German authors. In the work of Dr. Rühle, we are told that—

> "The anatomical difference between laryngeal diphtheritis and croup lies in the fact that the exudation is deposited not on the free surface only, but also in the tissue of the mucous membrane, that it is thrown off in the form of dry sloughs, and leaves behind it loss of substance, &c."

This description, which corresponds with those of other German writers, is so entirely inapplicable to diphtheria as it has commonly been met with in this country and in France, that it is clear that it applies to another disease, and that, in fact, up to a recent period, diphtheria was as little known to the pathologists of Germany as it was a few years ago to ourselves.

Albuminuria.—Dr. Jenner does not attach much importance to the condition of the kidney in diphtheria. "The albuminous urine," he thinks, "probably indicates rather an abnormal state of the blood than disease of the kidney." After death he has "never seen more than *congestion*," and this, he tells us in another place, is a condition "induced mechanically by the state of the lungs." This view is inconsistent with the facts recorded by Mr. Simon, Dr. Bristowe, and other observers, which prove the existence of exudation in both the fibrinous and granular tubes of the cortical substance of the kidney, as well as with the observations of Dr. Sanderson and others, as to the occurrence of fibrinous and more rarely of granular casts in the urine.

Pulmonary complications.—The manner in which that form of pneumonia which is associated with the extension of the concretion from the larynx into the bronchial tubes, is developed, is well illustrated by Dr. Jenner in the following descriptive enumeration of the pulmonary lesions observed after death by diphtheria:

> "Acute pulmonary vesicular emphysema, the result of the obstacle to expiration produced by the imperfect occlusion of the larynx and trachea; collapse of lung-tissue from the combined effects of lymph or mucus in the smaller bronchial tubes leading to the collapsed tissue, and of the impediment to deep coughing offered by the state of the larynx." (p. 7.)

On this condition of collapse "pneumonic consolidation" follows, exhibiting that "scattered" distribution which is characteristic of pneumonia of this kind.

Mode of epidemic diffusion.—Dr. Greenhow, whose inquiries have been specially directed to the question whether or not diphtheria is communicable, expresses himself in the following guarded manner on the subject:

> "Many facts have fallen under my notice which convince me that the disease is in some way or other communicable. . . . . It is evident, however, that diphtheria is much less contagious than either scarlet fever or small-pox, inasmuch as I have seen many instances where only one member of a family has suffered from the disease." (pp. 138 and 148.)

The kind of evidence on which this conclusion is founded is stated as follows:

> "If, soon after the arrival of a patient from an infected district, diphtheria should break out in a place where it did not previously exist, and attack persons who have been in direct communication with the invalid, and especially if it attack only such persons, then have we the strongest presumptive evidence of its being a contagious disease." (p. 138.)

To this presumption we venture to object. The statement on which it is based is, in our judgment, more general and absolute than the facts themselves justify, and if it were limited so as to be conterminous with them, it would no longer support the conclusion. Thus Dr. Greenhow relates that a young lady returned to her country home from school, suffering from diphtheria; that a week after her arrival her sister was attacked, and two others subsequently. Again: a little girl arrived at Folkestone from Boulogne on the evening of July 2nd, 1856, in an advanced stage of diphtheria, and died the day after. On the 6th her sister was attacked, and another case occurred three days after, in the same house, both terminating fatally. Several other cases are related, all of which are reducible to this formula: An individual suffering from diphtheria is introduced into a healthy household; all, or the greater number of the children, and some of the adults of the household, are attacked, but no one not residing in the locality, even though in communication with the invalids. On the other hand, Dr. Sanderson relates several very striking instances in which healthy children, or even adults, introduced into infected households, contracted diphtheria a very short time after their arrival. This fact tallies with the other, and both point to the inference that diphtheria is a disease which attaches itself rather to localities than to persons, and that the instances which at first sight seem referable to personal intercourse, are rather to be regarded as household epidemics, dependent on the same occult cause as the general prevalence of the disease. This view of the question is clearly supported by the numerous instances in which diphtheria has ravaged a village or hamlet for a lengthened period without attacking a single individual in the surrounding country.

Local and personal predisposing causes.—As regards the indifference of diphtheria to local conditions, our authors strikingly agree. According to Jenner, "the infection-element does not require for its development any of the ordinarily considered anti-hygienic conditions. It is very doubtful even if any of these conditions favour its development or give to it a more untoward course when it occurs." (p. 51.)

In like manner Dr. Greenhow appears to have met with nothing in the whole course of his inquiries which indicated a relation between diphtheria and local sanitary defects, although his attention was specially directed to the question in his instructions; he expresses his opinion on the subject as follows: "My inquiries and personal observation have entirely failed to connect its occurrence with the defective construction of houses, or with uncleanliness of dwelling, imperfect drainage, or any other cause of offensive effluvia." (p. 125.)

Climatic causes.—As regards the influence of climatic causes, Dr. Greenhow is disposed to attribute some importance to dampness of the soil, whether arising from the absence of natural or artificial drainage, or the impermeability of the subsoil. On the other hand, Dr. Gull is of opinion that "an exposed locality is as liable, and perhaps more liable than sheltered spots."* Both of these inferences, though apparently somewhat contradictory, seem to be borne out by the history of diphtheria both in England and France—the former by the broad fact that the disease has prevailed most in the marsh counties—viz., Lincolnshire, Kent, and Essex; the latter by the observation that the individual localities invaded have not been the villages on the marshes, but those in more elevated situations overlooking them. Thus, in Kent, the immunity of the low-lying district was no less remarkable than the severity with which villages enjoying apparently salubrious situations on the chalk hills were ravaged.

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* Second Report of the Medical Officers of the Privy Council, p. 297.

Whatever importance we may be disposed to attribute to these considerations, no general inference can be drawn from them. They aid us little in explaining how it happens that diphtheria prevails at certain periods and localities to the exclusion of others. The utmost that we can assume as to the nature of the cause of diphtheria is, that it is a material virus having little tendency to originate *de novo*. If this be admitted, the diffusion of the disease may obviously be accounted for by assigning to its virus certain properties; it must not only be a highly diffusible substance, but it must be capable of maintaining its existence and activity for lengthened periods, and probably of reproducing itself independently of the subjects of its action.

The functional disorder of the nervous system consecutive to diphtheria with which experience has rendered us so familiar during the last few years in this country, was till a recent period scarcely recognised by nosologists. No allusion to it is, we believe, made in any of the most recent general works, either in our own or in the French language. Although described by some early writers—as e.g., by Chomel, in his relation of the epidemic of malignant sore-throat in Paris in 1749—it is to M. Maingault that we owe the first complete account of it. In 1854, M. Maingault defended an inaugural thesis in Paris, in which he described the pharyngeal paralysis which is so common a consequence of the disease. In his recent work he has extended his former observations, and has been led to regard this affection as a mere manifestation of a general neurosis, to which he has applied the term of "paralysie diphthérique."

The outline which M. Maingault has sketched of the disease coincides entirely with its characters as they have presented themselves in this country. Thus, he has observed that the accession of the first symptoms usually takes place two or three weeks after the cessation of the primary local disease and the complete establishment of convalescence. In those cases in which the affection is about to attain its full development, the patient, instead of recovering his strength, begins gradually to become weaker; he experiences formications in his extremities, usually first in the feet and legs, attended with or followed by varying degrees of numbness and insensibility, and gradually becomes unable to walk. As the paralysis attacks the upper limbs, and the disease progresses, vision is impaired or lost, the articulation becomes indistinct, and the voice nasal and weak; the constitutional state being marked by the absence of pyrexia, the feebleness of the pulse, the pallor of the countenance, and the general characters of anæmia. There is often complete anorexia, but occasionally the appetite is preserved. The duration of this condition is various, it usually diminishes gradually, the occasionally fatal result being dependent either on gradual prostration or sudden asphyxia.

Thus we observe that the symptoms may be divided into two categories, according as they are referable to abnormal conditions of sensibility or of motility. The experience of M. Maingault agrees entirely with our own as regards the fact, that with the exception of the paralysis of the velum palati, the affections of the former class are by far the most frequent. In a large number of cases, the sensation of tingling radiating from the toes and fingers towards the knees and forearms is the only symptom complained of. But he finds that this is usually accompanied either with a certain obtuseness of tactile sensibility, the patients being unable to feel the ground or to judge correctly of the form and size of objects handled; or more rarely by complete anæsthesia.

M. Maingault finds that the impairment of vision comes next in order of frequency after the affection of the soft palate, and is usually concurrent with the disorders of common sensibility already referred to. It varies in duration "from a few days to six months," and in degree "from the mere inability to read small print to perfect blindness." In explanation of its nature, the examination of the eye with the ophthalmoscope yields no information; the only fact observed by M. Follin, in a case of great intensity, being a "slight indistinctness of the edge of the pupil, such as is often observed in anæmic persons." Not only the absence of anatomical change, but the sudden accession and rapid disappearance of this affection leads us to regard it as purely neurotic. Other circumstances show that all its peculiarities may be accounted for as dependent on loss of adjusting power—in fact, a temporary presbyopia. Cases are related by Dr. Greenhow and Dr. Sanderson, in which children, convalescent from diphtheria and unable to read the largest print unassisted, could read even the smallest with the aid of presbyoptic glasses.

The paralysis of the velum palati and pharyngeal arches is regarded by our author as an affection apart from the general muscular paralysis. It is not only exceedingly common, while the other is comparatively rare, but occurs at a much earlier period in the disease. We regret that space will not allow us to follow our author in his minute and masterly analysis of its phenomena, which is no less interesting to the physiologist than to the physician. He has minutely described the peculiar voice, and pointed out the distinction between its character (*voix nasillarde*) and the nasal quality of voice produced by obstruction of the nares, and has carefully investigated the mechanical action of the faucial arches in relation to the effects of its perversion, and has satisfactorily accounted for the incapacity of the diphtheritic patient to blow or expand his cheeks, to suck, to smoke tobacco, to gargle, or to expel mucus from the fauces, and his liability to regurgitation through the nostrils in the act of swallowing liquids.

The general paralysis of the muscles of the limbs and trunk has been met with by M. Maingault in a more intense form than in this country. It is marked at its onset and in its slightest degree by tremor, uncertainty of movement, and loss of muscular power as estimated by the dynamometer. The muscles of the lower limbs are usually first attacked, then the upper, and here the progress of the affection is frequently arrested. In other cases, however, it extends to the neck and trunk, the movements of which are abolished. The general attitude is altered, the lumbar portion of the vertebral column being thrown forwards and the shoulders backwards, while the head, no longer supported by the muscles of the neck and back, falls forward and rolls on the chest. This state of things is often accompanied by obstinate constipation, and in some instances there is complete loss of power of the *sphincter ani*, the patient being incapable of making any effort either of relaxation or retention. In rare instances urine is passed involuntarily, and occasionally complete anaphrodisia has existed in vigorous men for several months. Such symptoms, especially when associated with impairment of the articulation and tremor of the tongue, have an alarming resemblance to more serious forms of disease; on which account it is of great importance that the medical practitioner should be so familiar with them as to be able to express with confidence that favourable prognosis which experience justifies.

The epidemic which forms the subject of M. Jugand's essay, ravaged the town of Issoudun, situated in the midst of an alluvial district, and exposed to malaria. The type of the disease was the asthenic, or, as M. Jugand calls it, that of "*diphthérite humide*." Both the primary symptoms and the consecutive disorders are clearly described, and largely illustrated by cases which, although they do not differ materially from those recorded by others, are valuable additions to our knowledge. As might be expected from an observer residing in the midst of a marshy district, he attributes importance to malaria, not only as influencing the character of the disease, but as a determining cause of its prevalence.

The pamphlet of Dr. Wynne is for the most part a compilation from European literature. It contains little information as to the nosological characters or mode of diffusion of diphtheria in the New World, excepting so far as to show that its progress was not continuous, but that it appeared simultaneously in places immensely distant from each other.

REVIEW III.

1. *On Infantile Mortality, as illustrated by Private Practice, with Suggestions for future Inquiries.* (Reprinted from the 'Edinburgh Medical Journal,' November, 1860.) By W. T. GAIRDNER, M.D. pp. 12.
2. *On the Expediency of Founding an Hospital for the Diseases of Children in Edinburgh, with Notes on Continental Children's Hospitals.* By CHARLES WILSON, M.D.—*Edinburgh*, 1859. pp. 31.
3. *Annual Reports of the Hospital for Sick Children, Great Ormond-street, London, from its Foundation to* 1860.
4. *Annual Report by the Directors of the Edinburgh Hospital for Sick Children.* January, 1861.
5. *Infant-Feeding and its Influence on Life, or the Causes and Prevention of Infant Mortality.* By C. H. F. ROUTH, M.D., &c.—*London*, 1860. pp. 379.
6. *On Infantile Death-Rates in their Bearing on Sanitary and Social Science.* By W. T. GAIRDNER, M.D. ('Transactions of the National Association for the Promotion of Social Science for 1860.'—*London*, 1861. p. 632.)
7. *On the Excessive Infantile Mortality occurring in Cities and large Towns.* By JAMES FRAZER, M.D. ('Transactions,' *ut supra*, p. 648.)
8. *Address on Public Education and the Relation of Moral and Physical Forces in Civilization.* By Sir JAMES KAY SHUTTLEWORTH, Bart. ('Transactions,' *ut supra*, p. 79.)
9. *Address on Public Health.* By EDWIN CHADWICK, C.B., Vice-President of the Department of Public Health of the National Association for the Promotion of Social Science. ('Transactions,' *ut supra*, p. 574.)
10. *Public Health. The Right Use of Records founded on Local Facts,* Being Two Papers read before the National Association for the Promotion of Social Science at Bradford, in October, 1859; with an Account of subsequent Proceedings, further Remarks, and Evidence on the main Subject, and Replies to certain Objectors. By HENRY WILDBORE RUMSEY.—*London*, 1860. pp. 63.

THERE are no doubt persons who may be deterred by the titles alone of some of the memoirs placed at the head of this article from reading more of their contents, or of our observations upon them. One-half of such will regard their subject-matter as probably of an useful kind, but unquestionably of too dry and repulsive a character to make pleasant reading. The other moiety will, along with the dryness, maintain its deceptiveness in respect of practical results, if not its complete inutility. But such sceptics certainly ought not to be found amongst the members of the medical profession. The statistical and sanitary data of social science undoubtedly will not afford quite that kind of excitement which is attainable from the perusal, *e.g.*, of the 'Woman in White,' nor will a false idea of their capabilities, or an exaggerated view of their shortcomings, tend to make statistics a profitable study. But we may observe, *en passant*, that some very thrilling episodes in the writings of what has not been inaptly termed "the spasmodic school," have actually been founded upon some of the results of statistical inquirers. Nevertheless, to many the subject in question is and will no doubt continue to be void of much interest, seeing that, as Mr. Chadwick has remarked, such persons will regard mischief but coldly, if only it occur in an accustomed routine, if there be no visibly violent destruction, no smashing of limbs, nothing to excite the imagination with spectacles of blood, nor with manifestations of passion, to create an individual and tragical effect.

> "The whole community are excited by the dramatic interest connected with the murder of one child by violence, whilst the Registrar-General's returns of such annual numbers as eighteen hundred children burnt or scalded to death, included in upwards of thirteen thousand annual deaths from violence, amongst which are upwards of five thousand deaths from fractures and contusions reported as 'due to the absence of precaution which the new mechanical agency of steam has introduced,' being divested of manifest passion or individual interest, are read and passed by with vacant apathy or worse."

To such unmoved individuals, of course, statitians, who produce Blue-books about preventible causes of evil will appear to be only a dreadful set of bores. To some the social investigator is even worse than tiresome: he is an impertinent nuisance. We have been told that

> "He searches for only one class of objects, the disfigurements of our social system. Set down in a magnificent town, he forthwith proceeds to inquire where the drunkards mostly congregate, in what street the wife-beaters are found in greatest numbers, what proportion of the juvenile community is addicted to blasphemy and theft, and in what quarters the sewers are most offensive."

In fine, our sanitary legislators are to them simply members of a great "stink committee." To persons of such opinions we have here nothing to say; our writing upon this occasion of what *ought* to be a popular subject is addressed *non ad populum, sed ad clerum.* The majority of the profession is certainly fully alive to the importance of statistics and of sanitary reform. Indeed it is mainly through their exertions that a great department of our social economics has attained to that precision and value (however yet limited), which it is widely admitted to possess. It has been hard, uphill work, no doubt, and will continue to be so; but as purpose and method come to overrule more and more these particular investigations, the labour will become more hopeful and remunerative. It must be, however, admitted that there are yet some in our ranks who demur to this. They talk of "sanitary science" and "statistics" as being the cants of the day. The former, we are told, is to be found in the physiological principles which we are ordinarily taught in the schools, and the application of these principles is but a matter of method and handicraft, not of science. The latter mainly consist, it is said, in the multiplication of unmeaning returns. Whether this "matter of method" can or cannot be legitimately put forward as a science we will not stop to discuss, but simply affirm that it is worthy of our most anxious exertions to complete and carry out, seeing that it is not only revolutionizing the whole art of medicine, but is writing the hitherto missing chapters in the Pandect for the government of social life. That the multiplication of unmeaning returns is a miserable craft we do not deny, but only that it constitutes statistics. That facilities for deception are afforded by statistics cannot be gainsaid, though this is not a defect for which they are peculiar. Moreover, like all other fallacies, they are open to scrutiny and exposure, though the latter perhaps may be often more than ordinarily difficult. But hence arises the need rather for more caution in accepting some of their deductions than for an extreme flippancy in rejecting them all. There are perturbing causes which come into play in prosecuting statistical inquiries, which no unprejudiced person can refuse to admit.* The connexion, for example, between political and religious theories and certain investigations will lead men, in the collection of data, to a partial and deceptive statement of asserted facts in order to support the results of preconceived opinions. But here reliance is not to be more readily placed than in other departments of knowledge based upon statements of facts and on numbers without what must be deemed ample support of evidence, and inferences from such data can only be legitimately admitted according to the rules by which all sound reasoning is governed. But whilst advance in many of the physical sciences from the careful examination of facts is rapid, in social science, from an indifference to the facts, progress is comparatively very slow. Thus, to take an illustration from Mr. Chadwick:

> "A member of the House of Commons cited without question from any one, the amount of illegitimacy as a test of the state of education, without knowing or caring to learn what proportion of our increasing numbers are crowded, both sexes together, in single rooms, and often in single beds, as displayed in respect to this city in papers read before the present meeting. If he had inquired, he might have learned that in some urban districts, between sixty and seventy per cent. of the population have only one single living-room for a whole family, in which one room young unmarried men and women lodgers are commonly taken, in which one room they live and sleep, and births, sickness, and death take place, and the dead are retained amidst the living until interment can be obtained. . . . . I might ask your consideration of the effects on the morals of these adverse physical elements: 1st, the depressing effect of the foul air, in provoking an appetite for alcoholic stimuli to withstand it; 2nd, the high wages, in furnishing those means of relief and of indulgence which blind the judgment and excite passion; 3rd, the overcrowding, the massing both sexes together under circumstances of powerful provocation and of the entire abolition of moral restraints, to the destruction of moral as well as physical health. Such elements are as capable of analytical observation as those of inorganic substances by the chemist."

Now, it is the elucidation and systematic exposition of the effects of physical conditions upon health, morals, and habits, as *primary elements*, which the statitian and sanitary reformer bring forward in a code of doctrine as a *science.* As regards statistics in particular, it may be observed that whilst one party has demanded of them too much, another school has refused their assistance. Both errors have arisen altogether from a misconception of the kind of truths statistical inferences unfold. These, it ought to be carefully remembered, are *probabilities* and *approximate* generalizations, but still probabilities and approximations under the government of laws; for under corresponding circumstances, their mean averages are maintained. And if the approximate proposition be not the ultimatum of scientific knowledge, it is often the only one of it available for practical use. This has been well pointed out by Mr. Mill, who remarks:

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* We propose, ere long, to offer to our readers a communication specially treating of the application of statistics to medical science.—ED.

> "A general average should only be applied to cases which are neither known nor can be presumed to be other than average cases. Such averages, therefore, are commonly of little use for the practical guidance of any affairs but those which concern large numbers. Tables of the chances of life are useful to insurance offices, but they go a very little way towards informing any one of the chances of his own life or any other life in which he is interested, since almost every life is either better or worse than the average, such averages can only be considered as supplying the first term in a series of approximations; the subsequent terms proceeding on an appreciation of the circumstances belonging to the particular case."
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> "He can get on well enough with approximate generalizations on human nature, since what is true approximately of all individuals is true absolutely of all masses. And even when the operations of individual men have a part to play in his deductions, as when he is reasoning of kings or other single rulers, still, as he is providing for indefinite duration, involving an indefinite succession of such individuals, he must in general both reason and act as if what is true of most persons were true of all."*

This did not escape the Prince Consort, who, in his address delivered at the opening of the Statistical Congress in 1860, observed:

> "It is the essence of statistical science that it only makes apparent general laws, but that those laws are inapplicable to any special case; that therefore which is proved to be law in general is uncertain in particular."

More than approximate generalizations, then, the department of knowledge in question cannot be expected to afford, but these it certainly can, whether we regard moral or physical vital phenomena. No doubt many are staggered by the steady march of scientific research into the most sacred sanctuaries of life, and shrink from a large quantity of the statitian's data, either refusing to admit them as evidence or vehemently combating the conclusion that they exhibit the moral constitution of man and society as subject to law in any degree coordinate with that of the material world. Sir James Kay Shuttleworth allows that,

> "Neither moralists nor physicians were, however, prepared for the results of the researches of M. Quetelet, who ascertained that the same probabilities could from experience be predicated respecting the recurrence of phenomena involving in a much greater degree the moral constitution of man. Thus, during the same forms of government, education, and social condition, there is

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* A System of Logic, &c., vol. ii. pp. 126, 131, third edition.

a tendency to the recurrence of similar events, usages, and crimes. In like manner we can predicate that if the circumstances, physical or moral, were gravely changed, the sum of each class of probabilities would undergo a corresponding variation."

In so recent a work as Mr. Kingsley's Inaugural Lecture,[*] the possibility of a science of society is more than simply demurred to:

"How, I ask," says Mr. Kingsley, "are we to make calculations about such a species as man? Many modern men of science wish to draw the normal laws of human life from the average of humanity; I question whether they can do so; because I do not believe the average man to be the normal man exhibiting the normal laws, but a very abnormal man diseased and crippled; but even if their method were correct, it could work in practice only if the destinies of men were always to be decided by majorities; and granting that the majority of men have common sense, are the minority of fools to count for nothing? Are they powerless? Have they had no influence on history?"

Now here we have the professor, as his critic in the 'Westminster Review (April, 1861, p. 319) points out, refusing to hear of averages, for the extraordinary reason that what is true of the average is not true of every individual unit of the sum from which the average is struck! Mr. Kingsley has quite forgotten that the very idea of an average implies such inequalities and irregularities.

"If a man falls into the water, are we or are we not safe in predicting that he will try to get to shore? At all events, however perverse an individual might be, it is absolutely certain what a ship's crew would do. When we have so high a degree of certainty there is surely the possibility of a science. Mr. Kingsley would admit that it is highly improbable that any life insurance society will ever be ruined by all its members cutting their throats. But his metaphysics will not allow him to affirm that it is impossible. 'Any individual man,' he would say, 'can cut his throat; and I have yet to learn why whole insurance societies, whole nations, why all mankind may not use the same prudential power.' That is the amount of uncertainty which, in his eyes, makes a science of human nature impossible! . . . . The possibility of a science of society of course depends on the possibility of a science of individual man. Unless the acts of each individual are the necessary results of certain causes, the phenomena of society, which is made up of many individuals, must be radically incapable of scientific explanation. It does not, however, follow that because we cannot lay down rules of any particular nature for predicting the conduct of individuals, therefore the data to which sociologic laws are to be applied are equally inaccessible to us. On the contrary, much of the uncertainty attendant on speculations respecting the individual vanishes when we come to consider large masses of mankind; the actions of the individual being, perhaps, determined mainly by peculiarities in his constitution or circumstances, while the phenomena of society result from such influences as are most general and universal, and, therefore, most capable of being investigated."[†]

Whatever it may be that statistics can perform, the result is only to be attained, of course, when the facts upon which the influences are based be of a valid character. From false data the generalizations must be as erroneous here as in any other department of investigation.

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[*] The Limits of Exact Science as applied to History. An Inaugural Lecture delivered before the University of Cambridge by the Rev. Charles Kingsley, Professor of Modern History in the University of Cambridge, &c. London, 1860.

[†] Westminster Review, *ut supra.*

That the former is peculiarly open to certain kinds of false facts we have already admitted, and that we have them in no small quantity we must also allow. If, e.g., we take that branch of statistical inquiry which establishes the death rates from particular diseases, and in which, as medical men, we are considerably interested, it will require no great amount of acumen to perceive that the character of many of the assumed facts from which the inferences must be drawn are, to say the least, of a highly suspicious kind. To Mr. Rumsey, of Cheltenham, great credit is due for the earnestness and acuteness he has shown in endeavouring to improve this branch of vital and social economics. Would it be going too far to say that, taking the whole mass of "causes of death" returned annually to the Registrar-General, we can therefrom attain to only a *faint guess* at the true causes of death relating to those of a primary or of a secondary character? Are not certificates given when the diagnosis has been wrong, either from the imperfect state of medical science, or, it may be, simply from want of knowledge upon the part of the attending practitioner? Are they not also accorded when the medical attendance of the certifier has scarcely dated beyond the last few days of existence, or even only of the agony, and when the absence of post-mortem inquiry has added to the obscurity? Are not certificates well known to be given by unqualified and ignorant persons, and to be accepted by the registrars? Lastly, do not those public servants whose duty it is to collect and methodize material fit to be employed for sanitary purposes, really take but little or no interest in the matter? Do they, at any rate, exhibit much more useful curiosity about what they register than does a clerk at a booking-office about what he enters for his accustomed fee? If we look at the Registrar-General's Report for 1858, we shall find that not less than 449,656 persons are known to have died in England and Wales during the year. Of these the nature of the causes of death was in only eighty per cent. described by men professionally qualified to give, it is assumed, correct information. Twenty per cent. was made up of those either not at all, or ostensibly insufficiently certificated. It is not to be wondered at, then, that those who regard this important item of medical and social economy should be desirous of clearing the way before them of all removable hindrances to the perfectioning of their work. Mr. Rumsey very properly puts an important point in a striking light:

> "Most erroneous I deem the assumption that the last phenomena of mortal disease may be correctly reported as the 'cause' of death. They are in general but the penultimate effects of the real cause, or at most the last link of a chain of secondary causes. A public registration of sickness would provide the natural and obvious means of correcting mere statements of apparent results, often certified as the 'cause' on a cursory view of the dying or the dead. In crowded manufacturing districts deaths are continually occurring to which the medical certifier is summoned barely in time to witness the Hippocratic face, the cold drops on the livid forehead, the last agonies of life. In many a case the disease, or rather the series of disorders, has been treated only by some bold druggist or ignorant herbalist, or some wretched quack. Worse still, it is too often the mere expression of the very treatment—regular or irregular—to which the patient has been subjected. In thousands—especially among the

infants of factory workers—it is the direct consequence of maternal neglect, if not of slow alcoholic or narcotic poisoning. Violence, crime, intemperance, privation, congenital infirmity, syphilization, and that mysterious spring of evil, hereditary taint, are more frequently the real causes of a mortality which is hastily attributed to various secondary complaints. If these causes may be referred directly to certain abnormal conditions of society, so may the fatal diseases themselves be frequently traced to over-crowding, to dwellings of unspeakable foulness, to sites most pestiferous—ulterior causes, which need never have existed, or might long ago have been removed, and which having been tolerated, have prostrated each victim as surely as if his unshielded breast had been struck by the murderer's knife. Do the Reports of the Registrar-General display the frightful agency of these social wrongs, or leave on record a trace of their origin? A score or two of commonly certified 'causes of death' might easily be cited to show that they mean nothing more than the *modes* of death, affording hardly a clue to the real nature of the primary disease." (p. xiii.)

Further, it must be admitted with the writer whom we have just quoted, that the inherent difficulties of the nomenclature of disease become greatly increased by a recondite nosology. A classification such as that of the Registrar-General must frequently lead to great perplexities, and tend to vitiate statistical conclusions, so long as indifferent unscientific registrars record the statements of careless or uninformed certifying medical men. The preventive rather than the corrective remedy for avoidable mistakes would not unlikely lie in the improvement proposed by Mr. Rumsey—viz., that complete records of sickness and mortality should be compiled and published in the several registration districts by a legally-constituted order of men of superior education and large medical experience habituated to scientific processes, and in respectable position. Thus,

> "Any serious misuse of evidence, any deliberate concealment or perversion of facts would be next to impossible, while the number of recorders and the universality of their jurisdictions would furnish the necessary corrections for occasional or individual errors. Under such a system, certificates would be no longer accepted from unqualified practitioners, for the medical superintendent would possess just that information with regard to professional qualifications which the present registrars are neither compelled nor assisted to obtain.
>
> "We should no longer be able to quote a medical certificate that somebody's death was caused by 'want of vitality,' or another's by a 'worn-out stomach.' [*Facts.*] There would be far less probability that diseases of the epidemic or zymotic class would be confounded with diseases of particular organs, or that the equally important distinction between constitutional or blood disorders and those directly caused by external agencies would be ignored. A case of pneumonia would rarely be certified as typhus, or a death from scarlatina as the effect of a secondary dropsy; scrofula would often escape the euphemism of simple 'abscess' or 'ulcer.' Disease of the heart—a most uncertain term—would less commonly stand for the rheumatic fever on which it depended." (p. xii.)

But with all the drawbacks accompanying our present method, it must be frankly allowed that the mortuary registrations existing amongst the principal States of Europe result in statistics inferior, medically speaking, to those of our Registrar-General. We decline, therefore, to join Mr. Aspland* in looking upon the return annually presented by the officer simply as an "elaborate romance," whose unfaithfulness alone is proved whenever its verification is attempted. We feel still, as we always have done, very thankful for such useful approximate generalizations as it has on many points attained to. To look back upon the nature of the information which we possessed before the year 1837 concerning those important items dealt with by the modern Registration Act, and then to contemplate the various reports of the public officer alluded to, recalling to mind the admirable summing-up letter of Dr. Farr, is to make us feel that this act was really the birth of a promising and vigorous offspring of social science. Whilst, then, we would go hand in hand with Messrs. Rumsey, Aspland, Price, and others, in arguing for improvements in carrying out General Registration, we do not choose to forego any information and advantages for which we have been indebted to the method as it has for some time been carried on.

Whatever doubts may exist as to the value of the reported causes of mortality, it must be granted that the *general death-rate* has been pretty closely approximated to. This is 21·87 as a general average for England and Wales, or to express it in round and popular terms, we may say that out of one thousand persons living at all ages, twenty die in the course of a year. It is true that some births escape registration, some deaths are never known, some infants are surreptitiously placed in the coffins of adults, &c., and thus the exact truth is, and so far as we can judge, ever will be beyond our attainment. Still, as we have said, the general death-rate, as at present attained to, must be regarded as very closely approximate. When, however, we attempt to deduce the death-rates of particular places from the details from which the general death-rate is worked out, we find ourselves open to chances of very probable error. Many persons, *e.g.*, advanced in fatal disease, go and die in well-known healthy localities, and hospitals, asylums, unions, &c., will cause a local death-rate to rise much above the general average when the true rate may be discovered to be actually below it. As Mr. Rumsey observes:

> "The mere proportion of deaths to population in some town or building within a brief period is again and again put forward as a proof of the favourable or unfavourable condition of health in that place, without reference to the ages, habits, and employments of its inhabitants, without distinguishing residents from casual immigrants, regardless of vicissitudes of climate and seasons, calamitous events, or any other modifying circumstances. . . . . I have elsewhere exposed the common fallacy of assuming that a comparatively high average death-rate in any town or district is an absolute proof even of excessive mortality in its proper population. The alleged excess has to be examined; the facts have to be analysed by competent investigators—men versed in statistical researches. The deaths of recent immigrants have to be separated; the mortality has also to be checked by the sickness and infirmity not ending fatally, and by the proportion of 'effectives' among the inhabitants." (pp. xxiv., xxx.)

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* On the Statistics of Paris, and the Mode of obtaining Facts for Mortuary Tables. Paper read before the Manchester Statistical Society, Jan. 14th, 1861, by Alfred Aspland F.R.C S.

The necessity of such corrections was forcibly dwelt upon in the discussion which took place at Bradford in 1859, and will be found alluded to in the pamphlet from which we have just quoted. It must be further borne in mind that the mere number of deaths occurring in any locality does not bear a constant and scarcely an approximate rate to the real amount of unhealthiness prevailing there; or if it does, its expression will often be a very different one to what is usually expected.

"There are," says Mr. Rumsey, "grounds for the belief that while the standard of hygienic observances, personal and public, remains at its present level, and while the average age of the population is gradually increasing, especially in towns, *a diminution in the rate of mortality will be found to co-exist generally with an augmentation of the rate of sickness.* The very triumphs of advancing medical art are probably attended by an average prolongation of the helpless and infirm conditions of life." (p. xxiii.)

In comparing local rates of mortality, it has been demonstrated by Dr. Farr that one of the most essential points to attend to is the analysis of the deaths *according to age.* To Dr. Gairdner we are now indebted for carrying this point of analysis an important step further —viz., in analysing the death-rates of young children so as to indicate the favourable or the unfavourable position of a community. His proposition is—

"Given a community in which the infants die with extreme rapidity and in which the general death-rate is also high, you have in the fact of the high infant mortality not only a corroborative proof of defective sanitary conditions operating on the entire population, but in proportion as the infant mortality is higher than the average of places having the same general death-rate, you have proof of defective sanitary conditions operating specially on the young life in all probability through the neglect or vice or ignorance of the parents, and through their failure to fulfil the necessary conditions of a sound domestic relation. If, on the other hand, it were possible to find a district where the general death-rate is low, while the infantile death-rate is much above the average of such places, you would have, notwithstanding the good general sanitary condition of such a population a culminating instance of proved unfaithfulness on the part of the parents to their trust, proved neglect on a large scale of the duties of domestic life and the care of a family." (p. 633.)

Both Mr. Simon* and Mr. Chadwick† have laid much stress on the mortality of children, as almost necessarily denoting a high local prevalence of those causes which determine a degeneration of race. The death of children within one year of their birth is the test least affected by occupation or by immigration or migration, as also by aerial impurity. In attempting to lay down some of the laws of infant mortality, Dr. Gairdner has wisely chosen the period of one year in preference to the more usual one under five years, as representing the special death-rate of the earliest period of life. For the reasons which have guided him to this we must refer to his paper, p. 633. In working out the problem of the relation which the infantile bears to the general death-rate, he has availed himself of the laborious calculations appended to the Registrar-General's Ninth Annual Report, there being

* Preface to Dr. Greenhow's Papers. † Op. cit.

no similarly minute and elaborate series of calculations for any later period than that report refers to. On considering broadly the death-rate of very young infants, as compared with that of the general population, it is found to be immensely different, insomuch that it is rather a moderate statement of the case to say that where twenty represents the general death-rate, one hundred and fifty will be the death-rate of infants less than a year old; or in other words, the infantile is seven and a half times the general death-rate. This is undoubtedly the case so frequently, that it may be said to be in one sense of the word a normal fact, though, according to Dr. Gairdner—

> "It is not always a normal fact for the infantile death-rate to be seven and a half times the general death-rate. By a further consideration of the returns of the Registrar-General, and by a calculation from them in a great number of instances of the proportion which the infantile bears to the general death-rate, it appears that, as these death-rates themselves rise or fall, their proportion to one another commonly rises or falls also. Thus, when the general death-rate is so low as sixteen in a thousand, it is probably normal for the infantile death-rate not to exceed six and a half times the general death-rate; and when the general death-rate is so high as twenty-two in a thousand, it is probably so common as to be a normal though, of course, not a desirable result for the infantile death-rate to be eight and a half times the general death-rate. Thus it appears, upon a careful and extended consideration of the details of the inquiry, that the proportion or ratio of the two death-rates to one another varies with the amount of the death-rates themselves. And this curious fact tells a tale of some importance as regards the tenure (so to speak) of infant life—the conditions on which the young infant lives, moves, and has its being; for the enlargement of the ratio between the infantile and general death-rate, according as the rates themselves increase, shows nothing less than this—that, generally speaking, the causes which produce a high rate of general mortality, have a still greater tendency to produce a high rate of infant mortality, and operate upon the infant life to a far greater degree. In other words, the infant life is not only more largely sacrificed than the general life of a population under ordinary circumstances, but it is far more keenly sensitive to those causes of increased mortality which produce exceptionally high death-rates." (p. 635.)

The inquiry, it will be seen, becomes rather a complex one, necessitating the examination and comparison of a very large number of individual instances. As it seemed impossible to develope the full results of the comparison of infantile and general death-rates without the investigation being made upon a very extensive scale, Dr. Gairdner extended his inquiries over a very large surface, inquiring into the normal relations existing between the two rates, and the limits of variation of the ratio between the one and the other. This inquiry he has carried on

> "1st. By determining the ratio of the infantile to the general death-rate in all the divisions and counties in England and in a large number of individual districts and groups of districts. 2nd. By placing these in series according to certain pre-determined rules of general arrangement, and reducing them to mean values. 3rd. By arranging the mean values in the form of a scale or table, to be used as a guide in the rest of the inquiry before us." (p. 536.)

This table the author very truthfully says, is the result of no small amount of labour and calculation, and is a forcible illustration of the importance of large numbers, and of the collation of many individual facts in reducing to order a chaos of apparently random variations. We shall now lay before the reader very cursorily some of the results attained by Dr. Gairdner through the aid of his tables. In the first place, the districts and counties having very low death-rates, both infantile and general, and consequently a low ratio one to the other. are found to be, as is natural to suppose, mostly *rural*, often to a great extent pastoral, in character. The majority of these localities are in Wales, Cornwall, and Devonshire; though Glendale, in Northumberland, has the lowest of all infantile death-rates—viz., 7·702 in 100, male and female, under one year of age. There are some places, however, which, though on the whole very healthy, have an infantile death-rate higher than it ought to be. The causes of this rise have as yet to be inquired into. 2nd. The districts having moderately, but not extremely, low death-rates, are, speaking generally, those in which agriculture assumes a prominent feature as a staple industry. But we are

> "startled to find in the great corn-growing counties of England evidences of a flaw in the well-being of the infant population, which must necessarily exert a deleterious influence on the health of those counties, and through them on the English race in general. Not only is the infantile death-rate in many of them high (absolutely higher, for instance, than that of the country at large), but in many cases where this is not so, the infantile death-rate is much higher than it ought to be, considering the eminently rural character of the population, the small size of the towns, and the small number of persons to each acre of surface." (p. 640).

In Huntingdonshire, Cambridgeshire, Bedfordshire, Lincolnshire, and Norfolk the infantile death-rate reaches its maximum, being considerably above that of England and Wales; and the proportion between the infantile and the general death-rate is more than 18 per cent. instead of 14 per cent., the normal amount in the circumstances according to the principles of the tables. There are, however, many difficulties in the way of arriving at just conclusions in regard to this point. As one explanation of the high rate of mortality in such districts as the above, we may refer to Dr. Headlam Greenhow's statement as to the danger frequently accruing to the health of the female population and of the children in rural places from the occupations of lace-making, straw-plait weaving, straw-bonnet making, &c. We may also recal to the recollection of the reader how much painful interest was excited last year by the distress among the ribbon-weavers of the city of Coventry. Now, the registrar of the Holy Trinity of this city tells us that, notwithstanding the unprecedented suffering which prevailed there in consequence of the prostration of the trade, the rate of mortality was extremely low, "there being only 67 deaths against 132 deaths in the corresponding quarter of last year, 98 in 1858, and 100 in 1857." The Registrar-General remarks upon this: "The care of the mothers of Coventry has, it would seem, counteracted some of the effects of privation, so that neglect of their homes by mothers at work in the manufactories is apparently more fatal than starvation." 3rdly. The districts having the highest death-rates are to be found in the manufacturing districts, the potteries of Staffordshire, many of our seaports, in fact, in all the worst parts of our great centres of population. In some of these localities it would appear, assuming the details to be correct, that much more than 1 in 4 of those living under one year of age perish annually. 4thly. From particular circumstances, well-known and succinctly alluded to by Dr. Gairdner (p. 644), the annual mortality of London as recorded from week to week by no means represents the mortality of the numbers included in the census, and particularly as regards what is called "the West End." These circumstances help to give a general death-rate to certain metropolitan localities much below the general average of some districts. In St. George's, Hanover square, only 18 in 1000 die annually; in St. James's, Westminster, 21 die; in Marylebone under 23 die; whilst in St. Saviour's and St. Olave's we reach the high rate of 28·46 per 1000; in St. George's in the East, 28·87; and in Whitechapel the culminating point is arrived at—viz., 29·03 per 1000 living.

> "Now we might naturally expect that in these different districts the infantile mortality would bear some appreciable proportion to that of all ages. But the fact is far otherwise, insomuch that it is absolutely impossible to extract from the death-rates of London any trace of such a series of proportions as is shown in Table I. to exist in the counties of England taken as a whole. . . . . These facts hardly admit as yet of being reduced to any general form of expression. But a careful consideration of them has led me to the discovery of a phenomenon which lies indeed on the surface of the Registrar-General's returns, but which I do not remember to have seen stated in the distinct form in which I shall now bring it under your attention. It is this: that all the West End districts of London, without exception, are fatal to children in a proportion which is really enormous when we consider the favourable state of the general death-rate and the many advantages which these districts have over the others. . . . . It results from this table, that the group of districts which I have marked on this map has a position inferior to all the others except two—the two in question being simply a collection of the most crowded and among the most neglected districts in London. In other words, the group of districts which encloses all that is best and noblest, and, in one sense, healthiest and most vigorous in London, is about as murderous to infants under a year old as the districts of Shoreditch, Bethnal Green, and Whitechapel taken together; while even the sailors around the docks and on the Surrey shore, and the tradesmen and artisans of the Strand and City districts, may boast that their contribution to the infant mortality of London is small compared with that of the rich, prosperous, and polished West End." (p. 644).

Two of Dr. Gairdner's conclusions—to say nothing of the others—may well be termed both novel and startling; the one which shows the influence of large agricultural populations on the infantile death-rate, the other the influence of the West End of London upon the same.*

We shall now accompany Dr. Routh, who advances the analysis† a step onward in his examination into the truth of the doctrine that the highest mortality amongst infants is that which occurs in foundling hospitals, particularly in those where artificial feeding entirely displaces suckling at the breast. In prosecuting his inquiry, Dr. Routh has been compelled to use the French returns. Late writers have generally satisfied themselves with the conclusions of M. Villermé, who has shown that in Lyons and Parthenay, where the children are suckled at the breast, the mortality is respectively 33·7 and 35 per cent.; and at Paris, Rheims, and X——, where artificial feeding is either extensively or very generally employed, the mortality rises to 50·3, 63·9, and 80 per cent.; the per-centage of children from 0 to 1 year in Paris generally being (according to Benoiston de Chateauneuf and Quetelet) 21·287, and in France, 23·248. A more recent investigator—M. de Watteville—quoted by the author, gives the following *résumé* respective of all France:

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* Vide 'Lancet' for October 5, 1861, p. 334, for some further observations upon this latter point.

† *Quoad* the infantile death-rate.

> "In comparing the deaths of *enfants trouvés*, whether with the totality of their number or that of the *expositions*, this is the result obtained. One dies out of seven from 1 day to 12 years, or about 14 per cent., and the mortality of such children in the first year of their existence is 50 per cent."
>
> "There is but one foundling [adds Dr. Routh] exposed in every 39 births in France, while the number of foundlings in institutions is 1 to every 353 inhabitants. Again, the number of foundlings exposed is one-fourth the entire number of foundlings actually existing in institutions; whence it would follow that the mean duration of life of foundlings is four years. Fortunately, of late years this mortality has been diminishing. Thus for all France it was for children from one to twelve:

Year.		Per cent.	Year.		Per cent.	Year.		Per cent.
1838	...	14·02	1841	...	13·30	1844	...	11·33
1839	...	13·37	1842	...	12·60	1845	...	11 30."
1840	...	13·25	1843	...	11·35			(p. 5.)

Dr. Routh attempts to prove that if we take Ireland, selecting indifferently different years, the returns show an extraordinary high rate of mortality, and this tends to explain in some measure the high rate common to foundling hospitals. But as a late reviewer* of our author's conclusions has pointed out, some modification of his inferences in respect to Ireland must be accepted. A modification so important, indeed, as to show that the real problem for investigation is "how, under all the apparent disadvantages they are exposed to, a greater proportion of children survive the dangers of the first year of life in Ireland than in any other European country." As among all town children of tender age the mortality is greater (speaking generally, and with the qualifications previously pointed out) than among rural children, it is important to distinguish between foundlings living in the country and those living in towns. The rate for each class is deduced by our author from some figures given in the 'General Statistics of Foundlings in France,' published by the authority of the Government. But says Dr. Routh:

> "Here, as in the former case, the data being insufficient, I am unable to obtain more than an approximative result. The relative mortality, however,

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* The 'London Medical Review,' July, 1861, p. 33.

between town hospital foundlings and those placed in the country thus comes out more strikingly than we might have supposed. Thus in five years,

Out of 52,883 town hospital foundlings the mortality was 72·2 per cent.

Out of 122,110 country ditto the mortality was 11·5 per cent.

This conclusion proves that foundling hospitals, if established at all, should always be placed in the country." (p. 15.)

What, it may be asked, are the chief causes of the general high rate of infantile mortality in cities and large towns, and of the still higher rate of foundlings in particular? In reference to the first point, we may refer to Dr. Frazer, according to whom the following special agencies, placed in their order of importance, most contribute to the excessive infantile mortality in our large communities.

1. Overcrowding and vitiated air, imperfect drainage, and deficient supply of light.
2. Deficient nutrition.
3. Want of hospitals for the sick children of the poor.
4. Too early marriages.
5. Neglect of illegitimate children.

To these of course have to be added other causes which are more general in their operation, and which, though common to country and town districts, yet acquire an immense increase of power in the latter, and possess, as Dr. Frazer points out, "a marked influence in increasing the fatality in the young of the middle and upper classes by giving increased intensity to contagious emanations, and spreading their lowering influence far beyond the localities in which they are generated." Mr. Chadwick—including in his views children above five years of age—points out with so much force the aggravation of all children's ailments, and the fatal effects arising from an excess of bodily constraint and the neglect of physical training when combined with imperfect ventilation and deficient warmth and light, that we cannot refrain from making the following quotation:

"If we observe young children in a state of nature, their peculiar mobility during periods of growth, their incessant changes and activity for muscular exertion—changes short at first and longer as growth advances, excited by quickly varying objects of mental attention, with manifestations of pleasure when allowed free scope, of pain when long restrained—if we ask to what these changes subserve, we receive for answer from the physiologist that they serve to stimulate the whole nervous and muscular system, and to promote healthy bodily assimilation and development. The theory and the common practice of school instruction is of five, at the least, or six hours and more of quietude and muscular inactivity, with intervals of three hours each with only occasional variations of position, and during this bodily inactivity continued attention and mental labour by very young children, say from six or seven to ten years old and upwards. To ensure this bodily inactivity and enforce continued mental attention and labour (during periods in which it is difficult to sustain it, and injurious to exceed it even for adults), the service of the school-teacher is made to be one of severe repression to keep little children still whilst every muscle is often aching from suppressed activity. I have the warranty of Professor Owen and other physiologists for saying that the resistances of children are for the most part natural vindications of the laws of physiology, and I am prepared to show elsewhere, on the evidence of some of the most experienced and successful school-teachers in the kingdom, that they are violations of the laws of psychology, and injurious mentally. The evil effects of the common bodily constraints during long hours in school are seriously manifested on girls, and especially on girls of the middle classes." (p. 593.)

We return to Dr. Routh to assist us as to the causes of the high mortality of foundlings. It has been usual to assign the want of breast-milk and its substitution by artificial food as the main cause of it; and the different rates of the great French hospitals, as they follow one or other method of suckling or feeding the children, are appealed to as forcible illustrations of the truth of the theory. To this doctrine Dr. Routh unhesitatingly objects. He asserts want of breast-milk to be only *one*—though a powerful one—of several causes. The arguments he follows are, in our opinion, sufficiently satisfactory. The causes in operation may be thus expressed. In the first place, the removal of the foundling from a greater or less distance to the hospital, and its consequent exposure, is a source of fatality to all weakly children, and if done with want of due care and with neglect, even to healthy infants. This cause operates more powerfully in the cold seasons, though for children *in* the institutions spring would appear to be the most fatal quarter of the year. In the second place, the recumbent posture and want of movement, so well commented upon by M. Hervieux, give rise to cold and to hypostatic congestion of the lungs, and are almost necessarily found associated with what must be called—starvation. In explanation of this we must refer to page 37 of Dr. Routh's work, where will be found an abstract of M. Hervieux's paper. In the third place, the impure air of the wards of foundling hospitals, and the endemic contagious diseases well known to be common in them, are important causes of fatality. Fourthly, to those foundlings who are suckled, the strange nurses' milk is of itself a source of sickness and death. M. Benoiston de Chateauneuf has shown that the mere substitution of a hired wet-nurse's for a mother's milk increases the mortality 10·64 per annum—i.e., from 18·36 to 29 per cent. Most of the vicious elements involved in the foundling system may bear more or less upon the children external to their walls or government, but it is in association with this system that they operate so intensely.

For the treatment of the main evils which are the causes of the high mortality of children, we may say with Dr. Frazer, that they will be found to consist chiefly in

> "improvements in the dwellings of the poor; the erection of hospitals for sick children, supplemented by some modification of the Scottish parochial system, by which sickness as well as pauperism on the part of the young would be made a condition of entrance into our poorhouses; the correction of the more prevalent errors in regard to the management of infancy and childhood; and by giving the rudiments of physiology a place in general education, the introduction of some method which, without removing responsibility or the penalty of error, would yet save the lives of illegitimate children and the extension to Scotland of the English Compulsory Vaccination Act." (p. 654.)

Having reviewed some of the causes of the mortality of foundlings, and sought to prove that it is by no means due so much to want of breast-milk alone as to other circumstances co-operating with it, Dr. Routh next proceeds to consider the advantages which evidently attend the use of this natural food over all other kinds of diet. If from some inevitable and urgent reason a mother cannot suckle her own child, a *properly* selected wet-nurse will be the best succedaneum. In selecting one, however, we are surrounded by some great difficulties. It is well known to be a very common practice to make choice amongst "fallen women." This practice the author strenuously opposes as one fraught with danger to the infant, the household, and to general society. We do not propose here to consider the *pros* and *cons* of the subject; we will here only observe that the reasons adduced for this objection by our author are many and forcible, and will well repay the consideration of the medical adviser. The wet-nurse ought to be, it is affirmed, a married woman, and "should be chosen amongst mothers of many children" (p. 87), as if not so chosen, her experience in the management of infants will not be greater than that of a woman after her "first fall"—a person who constitutes the *beau ideal* of a wet-nurse in the opinion of many. So great are the difficulties, indeed, attending this method, that Dr. Routh talks of "leaving the employment of a wet-nurse as a *pis aller*." If the mother

> "be not able to wet-nurse the child at all, certain principles ought to be observed in feeding it, whether the artificial food given be animal milks or something more distinctly artificial . . . . no treatment can be safely recommended in these cases which can bear any comparison with that which experience has proved to be most successful in other countries: I allude to the direct suckling of the child from the breast of some other animal, as, for instance, of the goat, to which I have already referred (pp. 141 and 156). Besides, it is the most natural. This itself is no small advantage. But it also does away with the necessity of an experienced nurse to prepare the child's food *secundum artem*, so that it shall not disagree. Lastly, no improper practices of the animal are likely to endanger the safety of the child, which, after suckling a short time, it will come to love and protect as its own offspring." (p. 307.)

But it is evident that such a plan can be only of limited adoption; what, then, is the next better and more successful method of artificial feeding? In early months the food should be exclusively animal, and milk obtained from a cow at grass will be more likely to be wholesome. This should be given in a diluted state, of the proportion of one or two pints of water to one of milk, according to the age of the child, the amount of water being diminished as the child becomes older. To this diluted milk sugar should be added in the proportion of one or two drachms to every pint. All admixtures of vegetable matters in early periods as contrary to nature, except in disease, should be avoided or given only as correctives of bad milk. When the child gets some teeth, it is an indication that those physiological changes which are essential to the digestion of vegetable material have taken place; then vegetable aliment may be usefully combined with the food which is given to the child:

> "Of these, several preparations have been from time to time recommended and used with advantage. Thus we hear of 'Hard's farinaceous food,' of

baked flour, tops and bottoms, biscuit powder, and a variety of other aliments of that kind. In my own practice, without necessarily denying that with some children these substances will prove occasionally very useful, I have generally limited myself to the employment of three substances: 'Mrs. Well's vegeto-animal food,' 'Robb's biscuits,' and 'lentil powder.'" (p. 344.)

In feeding or bringing up a child by hand, circumstances will frequently arise which will necessitate a change being made in the food usually employed, or the adoption of some additional article like wine, raw meat, whey, &c.; upon the use of such agents much interesting and valuable information may be found in Dr. Routh's little treatise. He has subjoined, we may add, in conclusion, an appendix to it, in which he discusses the question as to the possibility of mental influences being transmitted through the milk of a wet nurse. He deems that the whole analogy of nature proves that it is possible to put that into the infant which shall contaminate the life of the man—taint his whole constitution and influence his psychical power.

The public have much reason to thank Dr. Routh for the painstaking inquiries he has made respecting the bringing up of young children; the more so, as it must have been quite apparent to him that —as the reviewer of Wertheimber,* in a recent number of the 'Journal für Kinderkrankheiten,' observed—such a study promised but little reward, honour, or profit, whereas, could he discover a new cystic entozoon, or a queerly-tailed cell, renown would freely be accorded to him by "Young Medicine."

REVIEW IV.

Principes de la Doctrine et de la Méthode en Médecine. Introduction à l'Étude de la Pathologie et de la Thérapeutique. Par J. DELIOUX DE SAVIGNAC, Professeur de Clinique Médicale à l'Ecole de Medicine Navale de Toulon, &c. &c.—*Paris*, 1861. 8vo, pp. 834.

Principles of Doctrine and Method in Medicine. An Introduction to the Study of Pathology and Therapeutics. By J. DELIOUX DE SAVIGNAC, Professor of Clinical Medicine in the Naval School of Toulon, &c. &c.

M. DE SAVIGNAC is of opinion that the treatises we possess on general pathology, and still more those on general therapeutics, are insufficient for their end, because none of them embrace a philosophical exposition of the general truths of medical science. However this may be, we think that there was abundant room for a new work on the more scholastic and theoretical departments of medicine, and that M. de Savignac has produced one which will be found in some respects of great utility to the medical student. It appears to us, however, that he would have done better if he had confined himself to a review of the labours of preceding writers, with such modifications and additions as might have been needful to bring the subject up to the present state of knowledge. This object he has indeed accomplished in a manner which shows extensive and accurate information, and, in some instances, considerable soundness of judgment; but in going further than this, and attempting a new classification of diseases and of therapeutical agents, he has, we think, been eminently unsuccessful.

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* Diätetik der Neugeborenen und Säuglinge. München. 1860.

He divides his work into two books. The first contains a review of medical doctrines, and their influence on practice from the earliest times to our own. We consider this as by far the most valuable portion of the work. The author has succeeded very happily in catching the leading ideas and characteristic peculiarities of each successive sect and system, and especially in enucleating those views by the persistence in, or the subsequent recurrence to which, the science of medicine as it now exists has been gradually built up. And this is no small praise; for though we have several so-called histories of medicine, and some of them replete with learning, they can scarcely be regarded as histories in the truest sense of the term, the most important object of all history being to illustrate the present by the past. We regret that our limits will not allow us to present our readers with any specimens of this portion of M. de Savignac's work.

The second book is on pathology and therapeutics, and contains much instructive disquisition on the general bearings of these subjects, which we are obliged to pass by, because our limits would not allow us to do it any sort of justice. M. de Savignac is very full upon the subject of nosology, and is greatly in favour of a natural method, which, according to him, is one presenting "a systematization of all the morbid facts, logically grouped into classes, orders, genera, and species, according to their natural relations." (p. 500.) We may suggest, in passing, that a writer who, like M. de Savignac, makes no inconsiderable display of logic, should be careful not to use such absurdly inaccurate expressions as "morbid facts." If facts be subject to disease, we should decidedly be for rejecting all but the healthy ones. We entirely differ from our author as to the superior advantages of what is called a natural system of nosology. The natural relations of diseases necessarily involve the intimate nature and the causes of the morbid actions; but concerning these there has been, and is, an endless diversity of opinion; and it appears to us that the attempt to found the definition and classification of diseases on such uncertain and fluctuating data has contributed, more than anything else, to give rise to the serious question whether nosology has promoted or retarded the progress of medicine. This question, however, in as far, at least, as relates to the *definition* of diseases, appears to admit but of one rational answer—namely, that medicine, like all other arts and sciences, must have some language of its own, in which, as far as possible, the same things shall be designated by the same terms. In this view, the most useful, or rather the only useful method, would seem to be an artificial one, founded merely on symptoms and external appearances, to which may be added what are called "physical signs," where these exist and are sufficiently certain and uniform. It is true that no just parallel holds between the definition of an ordinary object of natural history, as a plant or a mineral, founded on its external characters, and the definition of a disease founded on its symptoms. In the one case we have to deal with a tangible object marked by characters in a great measure constant; in the other we have to deal with an abstraction, derived from an assemblage of phenomena subject to great variation in different cases and at different times. Nevertheless, if we cannot attain to precise definitions of diseases, we must take up with such as may yet serve the main purpose of coupling intelligible general notions with given modes of expression. Thus, although we cannot give such a definition of *hysteria* as shall embrace even the leading phenomena of every case, we may easily give such a definition as that no one shall suppose us to mean thereby smallpox or the gout, and such as shall in general apply with reasonable accuracy to the disease intended to be designated. If symptoms, signs, and external appearances can afford us this amount of accuracy, they will do for us, we conceive, all that is to be expected in the definition of disease, and more than can be obtained from the application of any so-called natural method.

With respect to classification, again, any attempt at a comprehensive arrangement of diseases according to their natural or true relations and dependencies must necessarily fail, as requiring an amount of knowledge which we neither possess, nor seem likely soon to attain; and the best recommendation of any nosological arrangement would perhaps be the negative one of doing the least possible violence to what we know of the natural affinities of disease. On the other hand, if a natural classification fail us, we are left without an alternative, as any artificial principle would here evidently be altogether inapplicable; so that, on the whole, the attempt to frame nosological classifications would appear, in the existing state of knowledge, little better than labour thrown away. True, we have long possessed a few extensive groups, formed by a sort of instinctive recognition of the natural affinities of disease. Thus we speak of febrile diseases, inflammatory diseases, pestilential diseases, spasmodic diseases, &c.; and, in as far as individual maladies can be fairly brought under such general heads, they may have a place assigned them, but where they cannot, they had better, we think, be left to shift for themselves. According to our view of the matter, then, nosology is of indispensable use in reference to the definition of diseases, but at present, of very little in reference to their classification.

M. de Savignac takes a widely different view of the subject, and has been at the trouble of framing a new nosological arrangement founded on the "elements" of disease. The doctrine of morbid elements is one to which he attaches the highest importance. He speaks of it as

> "that great doctrine of elements which is coeval with medicine, the cultivation of which is perpetuated in the school of Montpellier, which justly boasts of it. A doctrine elsewhere ignored or forgotten; submerged by the wave of systems which the last half-century has raised, in spite of the precepts and protestations of those who remain faithful to it. The greater part of modern treatises on pathology do not even deign to make mention of it. To become acquainted with it, one must read Barthez and Dumas, who, taking up and restoring all the details of a plan on which the school of Montpellier has worked since the nosology of Sauvages, formed definitively, out of this doctrine, the code of diagnosis as applied to treatment. If any one shrink from the laborious reading of these old masters, he may at least derive an idea of this important subject from the remarkable article 'Eléments,' in the 'Dictionnaire

des Sciences Médicales,' which bears the signature of an illustrious disciple of the school of Montpellier, Frederick Bérard." (p. 542.)

It would not be difficult to show that the modern doctrine of elements in relation to disease differs very widely from the ancient, and that even the term "element" is used in a totally different acceptation. But this would be unprofitable. The subject of morbid elements belongs properly to scholastic medicine, and not to that inductive science which we now cultivate by observation and experiment; it is, therefore, worthy of attention only as a part of the history of medicine.

But let us see how M. de Savignac applies his favourite doctrine to the definition and classification of diseases. Take, for example, the "rheumatic element." This, it appears, is at the bottom of a class of diseases which he names "Rheumatalgiæ" (*rheumatalgies*), and which includes three genera—namely, rheumatism, gout, and neuralgiæ.

The Rheumatalgiæ are thus defined:

> "Diseases essentially painful, of a congestive rather than an inflammatory nature, affecting particularly membranous and fibrinous organs, localizing themselves especially in the articulations and the course of the nervous cords, changeable and erratic in their course, accompanied in some cases with various degrees of alteration of the organic fluids (superabundant fibrine, excess of uric acid, alkaline urates), and occasioning anomalous secretions (sweats, catarrhal flux, herpetic eruptions, articular concretions.)" (p. 605.)

Now, admitting that this definition applies with tolerable accuracy to gout and rheumatism, and to neuralgia, as dependent on the gouty or rheumatic diathesis, it is applicable to neuralgia arising from any other cause in no particulars except those of pain, and location in the nervous fibre. Nor is the relation of neuralgia to rheumatism rendered at all more intelligible by what M. de Savignac calls a "complementary" species of neuralgia, which he characterizes as "the most complex of all," belonging as much to rheumatism as to neuralgia, or rather best showing the resemblance of these two states—namely, "general neuropathy, in which the hyperæsthesia and pain radiate through all parts of the nervous system." (p. 606.)

But, setting all this aside, what useful purpose is here answered by the introduction of the "rheumatic element?" We can see nothing in this so-called element but a vague general expression for the phenomena of diseases supposed to be akin to rheumatism, or a still more vague expression for some unknown cause of such phenomena. A system thus based upon "elements"—in other words, upon abstract notions hatched in the brain of the nosologist—would appear to lead to nothing but an endless controversy as to what the elements might be; one disputant contending for an inflammatory, another for a congestive, another for a neurotic, another for an asthenic, another for a caco-chemical element, and so on *ad infinitum*, according to the particular theoretical views of each. But the worst of all is, that M. de Savignac does not stick to his own text. For example, on his fourth class, or "Pyrexiæ," he makes the following remark:

> "I consider this class as altogether temporary, and destined to disappear from nosology before the progress of pathology. Maladies cannot be diffe-

rentiated by the presence of an element so general as fever, which accompanies the most dissimilar diseases. Fever is not a disease in itself, it is only an element of disease." (p. 611.)

Thus, after making elements the basis of his whole system, he objects to fever because it is "only an element," and further objects to it because it is too general an element, which would seem as much as to say, that the more extensively a principle will apply, the less is it worthy of adoption. We can understand that an element too general for a class might be promoted to a division; but that its more general application should be made a ground for expelling it altogether, is something that we cannot reconcile with the logical conditions under which our author professes to construct his classification. On the whole, it must be confessed that the highly complex doctrine of elements is wonderfully simplified in its application by M. de Savignac; for his practice is merely to subjoin the word "element" to an adjective formed from the name of each class of diseases. Thus, for the neuroses we have a neurosic element, for virulent diseases a virulent element, for exanthematous diseases an exanthematous element, and so forth. To all this, however, the objection is, that it enunciates nothing, explains nothing, and in fact means nothing.

As we take so great exception to M. de Savignac's nosological system, we feel that it is but justice to give a general view of it, so that it may be enabled to speak for itself.

CLASS I. NEUROSES (Neurosic element).*

ORDER I. NEUROSES OF SENSIBILITY.

GENERA, founded on the distinction of the nerves affected, or of the organs of which the innervation is perverted, as neuroses of the acoustic nerve, morbid acuteness of hearing, nervous deafness, &c.; neuroses of the stomach—anorexia, dyspepsia, &c.

ORDER II. NEUROSES OF MOTIVITY.

GENERA.—1. Tonic convulsions. 2. Clonic convulsions. 3. Acynesiæ (paralyses of motion).

CLASS II. RHEUMATALGIÆ (Rheumatic element).

GENERA.—1. Rheumatism. 2. Gout. 3. Neuralgiæ.

CLASS III. DYSCRASIÆ (Dyscrasic element.)

ORDER I. ALTERATIONS OF THE BLOOD, CONSISTING IN MALPROPORTION OF ITS CONSTITUENT ELEMENTS.

GENERA.—1. Increase of fibrine. 2. Diminution of fibrine. 3. Increase of albumen. 4. Diminution of albumen. 5. Increase of red corpuscles. 6. Diminution of red corpuscles. 7. Increase of white corpuscles. 8. Increase of water. 9. Diminution of water. 10. Increase of salts. 11. Diminution of salts.

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* We beg to say that we do not hold ourselves responsible for the etymological propriety of the terms used in this nosological sketch; we have merely adapted them, as we best might, from the Gallicised forms employed by M. de Savignac.

ORDER II. ALTERATIONS OF THE BLOOD BY EXCESS OF ACCESSORY PRINCIPLES, OR THOSE WHICH EXIST IN IT ONLY IN MINIMUM PROPORTIONS.

GENERA.—1. Fatty matters. 2. Uric products. 3. Colouring matters of the bile. 4. Glycose.

ORDER III. ALTERATIONS OF THE BLOOD ARISING FROM THE PRESENCE OF AN ABNORMAL PRINCIPLE.

GENERA.—1. Caseine. 2. Products of morbid secretion. 3. Hæmatozoa. 4. Gaseous products.

CLASS IV. PYREXIÆ (Pyrexial or febrile element).

SPECIES.—1. Ephemeral fever. 2. Periodical fevers not of miasmatic origin. 3. Inflammatory fever, or synocha. 4. Slow nervous fever. 5. Hectic fever. 6. Adynamic fever of the aged.

CLASS V. ATHERMIÆ (Athermic element).

SPECIES.—1. Sclerema, or œdema algidum. 2. Progressive algidity of the newly-born. 3. Asphyxia from cold.

CLASS VI. INTOXICATIONS (Toxical element).

GENERA.

1. *Miasmatic Diseases* (Miasmatic element).
2. *Typhous Diseases* (Typhous element).
3. *Exanthematous Diseases* (Exanthematous element).
4. *Virulent Diseases* (Virulent element).
   Group 1. Species of animal origin—Vaccinia, hydrophobia, glanders, farcy, malignant pustule, carbuncle.
   Group 2. Species of human origin—Variola, varioloid diseases, varicella, syphilis, hospital gangrene, diphtheria.
   Group 3. Contagious blenorrhagia not syphilitic, ophthalmic blennorrhagia, endemic or epidemic purulent ophthalmia.
5. *Venomous Diseases* (Venomous element).
   Group 1. Poisoning by venomous serpents.
   Group 2. Poisoning by venomous arachnida.
   Group 3. Poisoning by venomous insects.

To which groups are added two special sections, of which the first includes poisoning by the ornithorynchus, the only venomous quadruped; and the second, poisoning by humours secreted by certain animals, as toads, tritons, and salamanders.

6. *Poisonous Diseases* (Poisonous element).
   Group 1. Mineral poisons.
   Group 2. Vegetable poisons.
   Group 3. Animal poisons.

Class VII. Congestions (Congestive element).

Genera.—1. Hyperæmiæ, or sanguine congestions. 2. Serous congestions.

Class VIII. Phlegmasiæ (Inflammatory element).

Orders.—Formed according to the tissue affected—as cellular, nervous, vascular, cutaneous, mucous, serous, &c.

Class IX. Hæmorrhagiæ (Hæmorrhagic element).

Arranged as affecting the nervous centres, serous membranes, skin and mucous membranes.

Class X. Dyscriniæ (Dyscrinic element).

ORDER I. HETEROCRINEÆ.

Genera.—1. Of the skin. 2. Of the respiratory organs. 3. Of the digestive organs. 4. Of the genito-urinary organs.

ORDER II. DROPSIES.

Genera.—1. Of the organs of innervation. 2. Of the organs of circulation. 3. Of the organs of respiration. 4. Of the digestive organs. 5. Of the genito-urinary organs. 6. Of the locomotive organs.

ORDER III. GASEOUS HYPERCRINEÆ, OR PNEUMATOSES.

Genera.—1. Affecting the cellular system—emphysemata. 2. Affecting the digestive organs—gastric and intestinal pneumatoses.

ORDER IV. ACRINIÆ.

Diminished or suppressed secretions, generally symptomatic.

Class XI. Anomotrophiæ (Anomotrophic element).

ORDERS.—1. HYPERTROPHY. 2. ATROPHY. 3. INDURATION. 4. SOFTENING. 5. GANGRENE. 6. DILATATION. 7. CONTRACTION. 8. OBLITERATION.

Class XII. Anomoplastica (Anomoplastic element).

ORDER I. HOMŒOMORPHIÆ.

Genera.—1. *Simple products*—Fibrous and fibro-plastic tumours, nævus, erectile tumours, cartilaginous products, osteo-sarcoma, spina ventosa, enchondroma, ossifications, calcifications, polysarcia, fatty liver, heart, lung, and kidney, hæmatoma, epithelioma, &c. 2. *Compound products*—Dermoïd cysts of various composition, cystic cysts, also of various composition, sebaceous tumours, melanosis.

ORDER II. HETEROMORPHIÆ.

Genera.—1. Tubercle. 2. Cancer. 3. Scrofula. 4. Esthiominous diseases. 5. Cheloïd tumours. 6. Frambœsia. 7. Elephantiasis.

CLASS XIII. HERPETOSES (herpetic element).

ORDER I. ACCOMPANIED WITH SECRETION.

GENERA.—1. Vesiculæ. 2. Bullæ. 3. Pustulæ.

ORDER II. DRY.

GENERA.—1. Papulæ. 2. Squamæ.

CLASS XIV. PARASITOSES (Parasitic element).

ORDER I. ANIMAL PARASITES.

GENERA.—1. External, or ectozoary. 2. Internal, or entozoary.

ORDER II. VEGETABLE PARASITES.

GENERA.—1. Trichophytic and onychophytic as *tricophyton*, producing herpes circinnatus, sycosis, &c.; *achorion*, producing favus; and *microsporon*, producing porrigo decalvans. 2. Epidermophytic, as *microsporon furfur*, producing pityriasis versicolor. 3. Epitheliophytic, as *oidium albicans*, the fungus of aphtha.

In the formation of his orders, genera, and groups, M. de Savignac seems to be guided by no fixed principle of any kind. Sometimes they are founded on general anatomy, or on physiology; sometimes on theoretical views of the nature of morbid actions; sometimes on the more obvious pathological changes; sometimes on the exciting causes of disease; sometimes on its locality; and sometimes on zoological distinctions.

Taken altogether, his nosology appears to us to afford a striking example of the inapplicability of the natural method which he so warmly advocates. It may be observed, however, that it is valuable in one respect—namely, as affording a very full enumeration of diseases, and including several morbid states which are not generally familiar, or which have been only recently described.

M. de Savignac's classification of medicinal agents is, on the whole, a chemical one, though botanical and therapeutical elements enter into the formation of several of the sections.

We may, perhaps, be rather old-fashioned in our views on this subject, but it certainly appears to us that a classification of therapeutical agents founded on their actions upon the living body, is not only the most obvious, easy, and practical, but the only one that has any real applicability. We may, indeed, arrange vegetable articles of the materia medica according to their botanical relations, and articles belonging to the animal and mineral kingdoms according to their zoological affinities or chemical constitution; but this is not to consider them as therapeutical agents, but merely to place them in one of three points of view, all entirely irrelevant to the matter in hand. M. de Savignac objects to the classification of medicines according to their effects, that the same substance may have several different actions on the animal economy; thus, says he, sulphate of zinc is an astringent, but it is also an irritant and an emetic. But this is an illogical objection: we are not considering the substance in question in respect merely to itself, but in relation to its therapeutic actions, and if it have several distinct actions, there is no reason why it should not be introduced on different occasions in its several distinct capacities. Julius Cæsar was a man; but he was also a statesman, a general, an historian, and an orator; and who would maintain that he ought not to be mentioned under each and all of these heads, because he was only one individual, referrible, in a natural history point of view, to the class Mammalia, and order Bimana? M. de Savignac objects, also, that the nature of the morbid state causes a variation in the physiological effects of many medicines.

> "Thus," says he, "belladonna and hemlock, which are generally placed among the narcotics, act as alterants or resolvents in cancer, according to those who believe them to be efficacious against this formidable anomoplasia; iodine is also an alterative and a resolvent; but in amenorrhœa, it acts as an emmenagogue." (p. 719.)

There is more plausibility in this objection than in the former, but still it is not really valid. The ordinary agency of the substance is modified, under particular circumstances, by certain morbid actions; so that the exception—for such it is—belongs rather to the study of disease than to the classification of remedial agents. Various articles of diet are not the less justly considered to possess nutritive, or healthfully stimulating properties, because, in some disordered states of the digestive organs, they may excite vomiting, or act as irritants.

M. de Savignac's classification of therapeutic agents is too voluminous to admit of our presenting any abstract of it.

We are sorry that, in our notice of this work, we have been compelled, by the nature of the subject, to dwell more on its defects than on its merits: we would not, however, be supposed to be by any means insensible to the latter. The historical portion is excellent; the critical portion learned, and sometimes acute; but it will easily be inferred from what we have already said, that we think the systematic portion had much better have been altogether omitted.

REVIEW V.

1. *Dictionnaire Général des Eaux Minérales et d'Hydrologie Médicale, &c.* Par MM. DURAND-FARDEL, EUGENE LE BRET, et JULES LEFORT, avec la collaboration de M. JULES FRANÇOIS. Two Vols. —*Paris*, 1860. pp. 1664.

A General Dictionary of Mineral Waters and Medical Hydrology, &c. By MM. DURAND-FARDEL, EUGENE LE BRET, and JULES LEFORT, with the assistance of M. JULES FRANÇOIS.

2. *Ueber die Wirkung der Sitz-bäder, der Brause, und der Nassen Einwickelung auf den Ausscheidungsprocess.* Von Dr. BÖCKER, in Bonn. (Moleschott's 'Untersuchungen zur Naturlehre,' Band vi. Heft 1. 1859.)

On the Influence of Hip-baths, Shower-baths, and the Wet Sheet upon the Process of Excretion. By Dr. BÖCKER, of Bonn.

3. *Physiologische Bemerkungen über das See-baden, mit besonderer Rücksicht auf Misdroy.* Von RUD. VIRCHOW. ('Archiv für path. Anat.,' Band xv. S. 70.)

Physiological Observations on Sea-bathing, with especial reference to Misdroy. By RUD. VIRCHOW.

4. *Das Nordseebad; eine Kurze Darstellung seiner Wirkung und seines Zweckmässigsten Gebrauchs, mit besonderen Bezug auf Norderney.* Von Dr. A. WIEDASCH, Praktischen Arzt auf Norderney.—*Hannover*, 1858. pp. 52.

Bathing in the North Sea; a brief Exposition of its Operation and of its most judicious Employment, with special reference to Norderney. By Dr. A. WIEDASCH.

5. *De l'Influence sur quelques Maladies de l'Air et de l'Eau de Mer, d'après leur Degré Réciproque de Température.* Par Dr. P. M. MESS, Médecin-Directeur aux Établissements des Bains de Mer à Schéveninque. Avec trois tableaux météorologiques.—*La Haye*, 1859. pp. 30.

On the Influence of Sea Air and Water upon certain Diseases, according to their Relative Temperature. By Dr. P. M. MESS.

6. *Thérapeutique Respiratoire; Traité Théorique et Pratique des Salles de Respiration Nouvelles à l'Eau Minérale Pulvérisée pour le traitement des Maladies de Poitrine.* Par le Docteur SALES-GIRONS, Médecin-Inspecteur des Eaux Sulfureuses de Pierrefonds. 1858. pp. 315.

Respiratory Therapeutics; a Practical and Theoretical Treatise on the New Chambers for Respiring Pulverised Mineral Water in the Treatment of Diseases of the Chest. By Dr. SALES-GIRONS.

7. *Bains à l'Hydrofère; Expériences Physiologiques et Observations Cliniques faites à l'Hôpital Saint Louis.* Par M. HARDY, Médecin de cet Hôpital, Professeur Agrégé à la Faculté de Médecine de Paris, &c.—*Paris*. pp. 84.

Physiological Experiments and Clinical Observations made on the Hydrofere Baths at the Hospital St. Louis. By M. HARDY.

8. *Bains à l'Hydrofère; Résumé des Principales Observations recueillies*

sur 200 Malades soumis aux soins de 54 Médecins de Paris. Par C. Tampier, Ex-médecin-Inspecteur des Eaux de Condillac, Chargé de l'Inspection des divers Établissements de Bains à l'Hydrofère. —*Paris.* pp. 52.

An Abstract of the Principal Observations made on the Hydrofere Baths, collected from 200 *cases under the care of* 54 *Medical Men in Paris.* By C. Tampier, &c.

At the present time, when science is extending itself so rapidly in all departments that it is only possible to keep pace with its progress by a methodical arrangement of the materials with which its votaries supply us, it is becoming more and more incumbent upon us periodically to gather up the stray threads of investigation which are scattered about on all sides, and to weave them into connexion with one another, and with the web of our previous knowledge. In the science of medicine such an occasional "taking of stock" is especially necessary in those fields of research which lie a little out of the beaten track of every-day routine, and which, from their being not very accessible, or not very attractive to the great bulk of the practitioners of the healing art, are left to the cultivation of those whom accident or inclination may impel to the task. In the so-called "practical" branches of the profession the case is different; there every one is at home, and each feels himself interested in any contribution that is made to the common stock. And the reason of this is obvious; the principles by which the practice of these latter is regulated are so well established, that they readily serve as standards to test, and as rallying-points around which to collect the waifs and strays which the tide of exploration is continually washing to our feet. But where the molecules of fact have as yet only imperfectly crystallized into the outline of principle and law, or where their bearing upon the necessities of practice is not very evident, the offspring of scientific discovery are apt to fall still-born to the earth, and to pass into the "long night" of isolation or oblivion, unless some *vates sacer* attend to chronicle their birth, and to introduce them to the notice of the busy world.

It is with the view of performing to some extent this humble but useful office, as well as of forwarding the cause of a department of medical practice which we believe to have been most unwisely neglected in this country, that we propose to lay before our readers a brief outline of some of the more important contributions which have of late been made to the theory and practice of balneology. Many of them will doubtless remember the able *résumé* on the same subject which appeared in our pages three years ago,* and may have probably been sufficiently interested by it to feel desirous of learning what progress has been made in this department of medicine during the interval. And although the results which we may have to present to them may not be so brilliant in their promise, or so obvious in their application, as some of those with which medicine has within the same period been enriched, we trust that we shall be able to show that they are eminently deserving of the attention of the profession generally, and are in many cases susceptible of adaptation to important therapeutic ends.

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* See "On the Action of Baths," in British and Foreign Medico-Chirurgical Review for January, 1859.

Of the special propriety of reviewing at the present time the additions which have lately been made to the literature of balneology, there can be little doubt. To make use of the language of the learned translator of Aretæus,* whose loss to medical science will be appreciated by all who know how great a light the history of the past often throws upon the labours of the present, "in no other respect within our recollection has so great a change come over the practice of medicine in this country, whether for the purposes of hygiene or therapeutics, as in the usage of baths." Whether we seek evidence of this fact in the more general extension of the familiar British institution of "tubbing," which is as much the Englishman's palladium of health as the Habeas Corpus Act is of his liberties; or in the erection of the admirable baths and washhouses by which the cultivation of cleanliness, and with it, in no small degree, the preservation of health, is brought home to the very doors of our lower classes; in the development of the so-called hydropathic establishments in all parts of the kingdom; in the spasmodic furor for the hot-air bath which has lately seized upon the public; or in the introduction into use of special modes of balneation—it is clear that "the bath" is destined ere long to assume with us an importance, both as a preservative of health and as a remedy in disease, not very far inferior to that which it possessed in the eyes of the ancients. The interest which the subject has thus excited in the popular mind, and the temptation which is thereby presented to the publication of *brochures* whose object is rather *ad captandum vulgus* than the enrichment of science, makes it the more necessary jealously to scrutinize every contribution to a study so young in this country as that of balneology. On the continent, where its claims upon the scientific world have long been recognised, where special journals are devoted to its objects, special societies record its progress, and an organized system of inspection controls its practice, the pretensions of any new forms of balneological treatment, and the value of any new contributions to balneological science, are more readily estimated than they are with us. To what causes this greater attention on the part of our continental brethren to a most important branch of therapeutics may be due, it is beside our purpose here to inquire; any one who doubts the fact may easily satisfy himself that such is the case by referring to the voluminous 'Dictionary of Mineral Waters and Medical Hydrology,' whose title we have given above, and by then asking himself what work in any way comparable with it is to be found in the English language? These two bulky volumes of upwards of 1600 pages, which discuss at length the mechanical construction, physiological action, and therapeutic uses of almost every variety of bath; the chemical composition of most of the leading mineral springs both in Europe and other parts of the world, and the indications for their employment; and which form a perfect repertorium of information on all subjects relating to medical hydrology, are a real contribution to medical literature, and do full credit to the reputation of M. Durand-Fardel and the able *collaborateurs* by whom he has been assisted. We strongly recommend any one who wishes for a complete hand-book of reference on these subjects to provide himself with this work. That an undertaking of such magnitude should not have been accomplished without leaving an opening here and there to hostile criticism, is only what might have been expected; into those points, however, we will not enter; we prefer to speak of the work on its general merits, and we can only express our surprise that, considering the natural difficulties of their task, and the superadded ones which have been entailed by the purely artificial method of treating their subject which they have adopted, its authors should have succeeded so well as they have.

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* Is the Turkish Bath the same as the Antient Roman Bath? By Francis Adams, M.D. (Med. Times and Gazette, Feb. 2nd, 1861.)

If the results of scientific investigation were always commensurate with the labour bestowed upon them, there are few subjects with regard to which our knowledge ought to be more complete than the action of baths, for there are few which have received attention from an earlier period in the history of medicine, or upon which more able and praiseworthy researches have at different times been made. But, unfortunately, although the problems to be determined in our inquiries on this subject would appear at first sight so simple as to be easily susceptible of solution, it will be found upon a closer examination of them, that the phenomena they involve are so complex, the causes which disturb the uniformity of result so numerous, and the difficulty of establishing precisely similar conditions during the investigation so great as to render the present condition of our knowledge on this subject one rather of approximation to the possession of *probable*, than to that of *actual truth*. Take, for instance, the influence of sea-bathing upon the economy. What would seem more easy than to determine, in the first place, whether a course of sea-bathing does produce any definite effects at all upon the body; and in the second, supposing that it does so, to estimate at their respective values the influence which the several elements of the process exert? It might be supposed that the only conditions necessary to be observed for the attainment of the above objects are to take a certain number of baths, and to estimate during, and for a short period subsequent to the time of their employment, the increase or diminution in the weight of the body, and in the excretions generally, as well as the immediate effect upon the centres of circulation, respiration, and nervous action. Now, although it might be very easy to arrive, with a little care, at a tangible result on each of these points, we shall soon see how difficult it would be to estimate the real value of those results when we had obtained them, and to be sure that they were really due to the agency experimented on. Let us reckon up some of the principal conditions whose influence it would be necessary to determine before doing so. 1st. There is the *state of health of the individual on whom the experi-*

ments are made, the susceptibility of the economy to the action of external influences being, as a rule, much greater in most states of disease than in health. The more we diverged from the type of health, the more numerous and conflicting are the conditions under which alone we could expect uniformity of result. 2nd. *The time of day at which the baths are taken.* The activity of the organism, and consequently the amount of its excretions, and the power with which it reacts against external influences, vary greatly during the twenty-four hours. The researches of Kaupp, Draper, Dr. Edward Smith, and others, have shown that there is a sensible and moderately regular fluctuation of the circulation and excretions within the twenty-four hours, and even between each meal. It will therefore be necessary for our experimenter to take his baths at the same hour on each day, and if possible to institute several series of researches, each at a different hour, so that he may be able fairly to take the average of the several members of each series, and to compare the several series with one another. 3rd. It will, for the same reason, be necessary for him to record the periods at which his meals are taken, and the *time that has elapsed between each bath and its preceding meal.* 4th. But even when he has done this, he must still remain in doubt as to whether the period which he has selected for his observations is a fair sample of the ordinary condition of his economy or not. It seems to be now pretty well established by the researches of Dr. Smith, as was surmised by Professors Parkes, Radicke, and Vierordt, that there is a cyclical variation in the amount of the excretions, and that they exhibit often a pretty uniform "wave" of two days (Radicke), which is probably accompanied by others of a much longer duration. It would appear as if the activity of the organism fluctuated in a definite and rhythmical manner within certain secular periods, in dependence probably upon the great law of secular vibration of which we see illustrations in the sleep of plants and animals, in the pulsations of the heart, the appearance of the catamenia, &c., and which may be ultimately referable to the variations of solar heat experienced by the earth in its orbit. How, then, is our investigator to be certain, even when he has complied with the conditions above mentioned, that his results have been obtained in an *average period* of bodily activity, and that they do not represent either the excess of the top of a "wave," or the deficiency existing during the interval between two waves? On this point he can obviously only satisfy himself by accurately observing the activity of his economy for a considerable period under the same conditions as those which would exist during the employment of the baths; by noting whether there are any, and what amount of fluctuation in the results obtained, and by using the conclusions drawn from these results to check the records of the bathing investigation. Should he find evidence of the existence of a definite period of fluctuation in the activity of his economy, he will then be able either to make the bathing period correspond to it in duration, or to allow for any variation from it. 5th. It would also be necessary for him to observe a uniform and moderate dietary and general course of life during the investigation, every deviation from which would proportionately vitiate the accuracy of his conclusions.* 6th. He would have to note the amount of exercise taken previous to and during the time of his bath—*e.g.*, the distance he had to walk to the sea; whether, whilst bathing, he swam about, or confined himself to the passive enjoyment of a plunge; and what was the amount of exercise taken immediately after leaving the water. 7th. In connexion with this point he should state the condition of the sea generally, during the whole of his investigation, as well as at the time of the individual baths, the observations of several German observers appearing to show that the action of the waves (Wellenschlag) has a decided influence on the physiological effects which bathing produces. 8th. He would also have to record the temperature both of the sea and air at each bath, as well as that of his own body, both with reference to the immediate effect of the temperature of the water upon the circulation, and to its secondary influence on tissue-metamorphosis. 9th. Of course he would make the duration of each bath as uniform as possible, and would use his judgment in so adjusting it to the conditions of temperature, bodily strength, &c., that it should not, on the one hand, be too short to allow of the full effect being produced, or, on the other, too long to interfere with the development of that reaction which is so essential a part of the bathing process. Finally, he should complement his observations by such a statement of his general nervous susceptibility as would allow those who might consult his researches to estimate the influence which an excessively irritable or easily exhausted nervous system might have had in unduly intensifying the action of external agencies.

Having thus far complied with the requirements of the physiologist, and arrived by the comparison of his observations before and during the bathing period at results which, we will suppose, indicate that the bathing either increases or diminishes the activity of the whole, or of some portion, of the economy, our investigator might be led to fancy that his results would now receive the stamp of general approbation, and be recognised as positive additions to the material of science. But this would be far from being necessarily the case, for having passed the ordeal of the physiologists, and satisfied them that he had considered and made allowance for the influence of various collateral agencies which would have otherwise vitiated the trustworthiness of his observations, he might have the bad luck to fall into the hands of the mathematicians, who would jealously scrutinize the relations of his figures, and who might inform him that, although his conclusions might *possibly* be correct, the number of his observations was too small in reference to the fluctuations between the figures obtained from them to allow of more than a problematical value being attached to his inferences.

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* He would also require to be scrupulous in the amount of liquid he consumed, so that the quantities taken on the several bathing days should be uniform, and not excessive, in amount; the effect of large quantities of fluid ingesta in favouring the metamorphosis of tissue rendering it impossible to compare observations made under these cirumstances with those made fasting. See Lehmann's reply to Boecker in Moleschott's Untersuchungen zur Naturlehre, Band vii. Heft 2. 1860.

We have thus indicated some of the traps and pitfalls into which any one who sets himself the task of determining some of those questions which lie at the root of our acquaintance with the influence of balneological agencies must be prepared to fall, unless he warily avoid them. And we have done so, not with the view of discouraging any one who may feel inclined to devote himself to this branch of inquiry, or of fostering that pyrrhonistic indifference to the solution of such questions which the difficulties to be encountered in so doing engender in a certain class of minds, but that we might corroborate the assertion which we made just now,—viz., that the knowledge we have acquired as the result of many investigations in this direction must be regarded as *problematically* rather than as *certainly* correct. How great, indeed, are the obstacles in the way of determining the influence upon the economy even of the commonest of the physical agencies, and how conflicting are the statements often made with regard to them, is well exhibited in the discussion which has lately been carried on between three or four of the most distinguished German hydrologists. Into the merits of the questions at issue between the various parties to it we are unable to enter here, from want of space, although they are in some respects of very general interest;* all that it is necessary to say with regard to the general bearings of the controversy is, that any one who impartially peruses it cannot fail to come to two conclusions: the first is, that in attempting to determine the influence of any agency upon the economy, too much caution cannot be exercised in taking into consideration every possible condition which may disturb the accuracy of the results obtained; the second is, that a large number of past researches will require to be gone entirely over again before they will justify the reliance at present placed on them. This may be very discouraging, but it is better to incur the labour of constructing the edifice of science entirely anew than that it should rest upon rotten foundations.

There is one point, however, in this discussion on balneological matters to which this seems an appropriate opportunity for referring, and that is the attention which it has drawn to the importance of the *mathematical relations* of the numbers obtained in any given series of researches. In the conditions above mentioned, as necessary to be observed in undertaking any investigation into the influence of sea-bathing, no question was raised as to the number of observations requisite to constitute a reliable ground for drawing any inference from the figures obtained; yet this is the most important consideration of all. What must be the number of the observations to justify our placing confidence in the numbers deduced from them? Or, to put the question in another way, how are we to estimate the amount of probable truth which any given result, founded upon numerical processes, contains? According to the ordinary method of conducting such an investigation as the above, two series of researches are made; the one under the influence of the agency to be examined (in this case sea-bathing), and the other in its absence. The mean number of each series is then taken as its representative, and by a comparison of the means with one another, we conclude that the agency in question exerts a positive, negative, or neutral influence upon the body generally, or on the special portion of it under observation. Now, this process consists of two distinct stages: the first is the determination of the mean number of each series; and the second, the comparison of the two means with one another. And the considerations involved in these two processes are, firstly, to what extent can the mean of any series of numbers be looked upon as really representing the average value of the whole series; and secondly, does the result which we obtain by comparing the two mean numbers invariably indicate the real difference in the relations of the two series?

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* The reader who feels interested in this subject may refer to the papers by Prof. Radicke, of Bonn, entitled "Die Bedeutung und der Werth Arithmetischer Mittel," &c., in the Archiv für Physiolog. Heilk., Band ii. Heft 2, 1858; and in Moleschott's Untersuchungen, &c., Band vi. Heft 4, 1859; which we are glad to see the New Sydenham Society has recently placed before its subscribers in an English dress: also to the papers of Dr. L. Lehmann, in Moleschott, Band vi. Heft 2, and Band vii. Heft 2, entitled "Zur Wurdigung der Physiologischen Wirkung der Sitzbäder;" to Dr. Beneke's reply to Radicke in the Archiv für Physiolog. Heilk., Band ii. Heft 4, 1858; and to the paper of Boecker, quoted at the head of this article.

Both of these questions are discussed in a most able manner by Professor Radicke in the papers to which we have alluded; and without entering into the details of the subject, or the bearings which it has upon medical statistics generally, which we think had better be reserved until the translation of the Sydenham Society makes its appearance, we shall content ourselves with briefly stating the conclusions at which he has arrived.

The mean of any series of numbers whose fluctuations depend, as in the case we are now considering, partly upon errors of observation, and partly upon the influence of unknown disturbing causes, may be regarded as containing a certain amount of truth, vitiated by a certain amount of error, both of which may be approximatively determined by recognised mathematical processes. As the mean itself represents, as it were, the line on each side of which the numbers of the whole series fluctuate, so the amount of error in the mean represents the extent to which the several errors of excess on the one side, and of deficiency on the other, have balanced one another in the general result. Now, by a natural law of numbers, the longer any series of numerical observations of this description is continued, the more do the errors of excess and deficiency tend to compensate one another, and the smaller does the residual amount of error in the mean result become.

Hence the question, How long must any series of observations be continued to allow of our placing any confidence in the mean result obtained from them? can only be answered in this way: A result in which the amount of error equals that of truth (i.e., = ½), is positively unreliable; but further than this no definite answer can be given, as each person must determine for himself what degree of confidence he will place, under the circumstances, in the mere numerical relations existing between the truth and the error in any given mean result.

All that we can demand is, that in stating such result the observer shall also state numerically the amount of probable error it contains, and by that statement our reliance on his conclusions must be governed.

With regard to the second of the above questions, the conclusion to which Professor Radicke comes is this: We may assume that the *difference* between the means of any two series of observations represents the probable (not the actual) effect of the additional agency brought into play in one of them, *if that difference exceed the sum of the errors in each of the means.* We may make this condition clear by a simple illustration. Suppose that the mean of one series is 50, with a *possible* error of 10 (i.e., the real mean varies between $50 - 10 = 40$ and $50 + 10 = 60$), and that the mean of the other series is 70 with a *possible* error of 20 (i.e., fluctuating between 50 and 90). According to the ordinary method of proceeding in researches of the description we have been contemplating, the difference between these means (i.e., $70 - 50 = 20$) would be taken as the estimate of the effect of the agency introduced in one of the series; but according to Radicke, it would indicate nothing at all, because it would be less than the sum of the errors in each of the means (i.e., $10 + 20 = 30$). If the first mean had had a possible error of only 5, and the second mean a possible error of only 10, then the difference of the means (20) would have exceeded the sum of the possible errors (15) by 5, and the reliability of the result would have been in the proportion of 20 to 15. Of course, the more the difference between the means preponderates over the greatest possible error (i.e., the *sum* of the errors of the means), the greater will be the confidence which we shall place in the result.

We have referred at some length to this element of all statistical inquiries which are involved in determining the influence of any given agency upon the economy, for two reasons; firstly, because its right apprehension is intimately related to the progress, not only of balneological, but of all branches of medical science, in which the establishment of facts depends upon the comparison of numerous uncertain and fluctuating phenomena; and secondly, because both the papers of Professor Radicke and the subject itself appear to have attracted much less attention in this country than they deserve.* We hope to be able, *à propos* of the forthcoming English translation of them, to bring the question more thoroughly under the notice of the profession, and that it will lead to more exactness in the way of conducting numerical researches than is too often the case at present. We shall now proceed briefly to lay before our readers some of the contributions to the theory and practice of balneology which have appeared since our last notice of this subject.

Although Boecker's paper on the action of hip and shower baths, and of the wet sheet, has been now published some time, yet as it has not been noticed in this journal, and as it has some practical import, we shall give a short outline of its contents. Those of our readers who perused the article 'On the Action of Baths,' to which we have alluded, will remember that Dr. L. Lehmann, of Oeynhausen, arrived,* as the result of his researches on hip-baths, at the conclusions: *firstly*, that they lessen the action of the pulse; *secondly*, that they increase the amount of the urine generally, and especially of its water, urea, uric acid, and fixed salts; *thirdly*, that they increase the insensible perspiration: and *fourthly*, as a consequence of these effects, that they promote the metamorphosis of tissue. These conclusions were sufficiently important to attract general attention, and were open to sufficient objections to require further confirmation. With the view of testing their accuracy, Boecker and one of his assistants, named Lampe, instituted upon their own persons a series of researches, the results of which must be regarded as considerably modifying the conclusions of Lehmann. Lampe's investigation we may dismiss at once, as it only consisted of a series of four comparative observations, which are too few to entitle his conclusions to any confidence, notwithstanding that the numbers he obtained corroborate, so far as they go, the results at which Lehmann arrived. Boecker's investigation consisted of two sets of observations of ten days each; the first without baths, and with an interval of two days between two of the observations; and the second with a hip-bath on each day of from twenty to forty minutes' duration, and with an interval of one day in the series. The observations appear to have been carefully made, and all the conditions noted which would have interfered with the accuracy of the results. The conclusion to which they lead, so far as they go, is, that there is no proof that hip-baths at a temperature of 50°–63° Fah. produce, *within a period of three hours after taking them*, on a healthy person, any effect either on the quantity of the urine, or on the amount of urea and chlorides contained in it. The limitation which we have italicised is important, as Boecker's examination of the urine did not extend beyond that period, which he himself admits was too short to settle the point definitively. With regard to their influence upon the pulse, the number of beats of which Boecker never took for less than eight consecutive minutes, and on one occasion for forty, he could not detect that any marked diminution of it occurred, but rather that it was increased on first entering the bath, though it speedily subsided to its normal number. Some observations, however, which Lampe made upon a third person showed that the effect of a hip-bath of 60°—68° Fah. was uniformly to diminish the pulse. This corroborates the statement of Lehmann, and agrees with the conclusions to which H. Johnson and Petri came; but the value of the evidence of these latter observers is much diminished by the fact that they only counted the pulse once before entering the bath, an omission which, considering the fluctuations that frequently occur in it from one minute to another, may have led them into some error. The more accurate researches of Virchow, to which we shall presently allude, prove that the effect of sea-bathing, at least, upon the pulse is very variable, and it is highly probable that the different results to which different observers have come on this subject are due more to various degrees of susceptibility to the action of the bath upon the part of the individual, than to any other cause. Indeed, as Petri remarks,* the effect upon the pulse may be entirely dependent upon the idiosyncrasy of the person experimented on. And this leads us to note that in weighing the conflicting evidence which is often given with regard to the action of various agencies upon the system, we must not be too hasty to conclude that because the statements of two observers are at variance with one another, one of them must, therefore, be necessarily untrue. These discrepancies are frequently, without doubt, due to the different way in which the organism reacts in different persons to the same stimulus. What acts as an excitant in one case may act as a depressant in another, as every tyro in physiology knows. And Boecker expressly guards against the inference being drawn from his researches, that because *he* could not detect that any effect was produced upon his own person by hip-baths, therefore they are without influence altogether. This principle he also applies to the criticism of Lehmann's conclusions; for, as he justly observes, even granting that they accurately represent the effect of hip-baths on himself, they do so of necessity *only for himself*, and for no one else, until repeated experiment shall have shown that a similar effect is produced by them upon other persons. Indeed, the objection which Boecker here urges is one that strikes at the root of too many of the fabrics which are being continually raised by the premature speculations of experimentalists. How often does not one see inductions of the widest character drawn from observations made upon a single individual; inductions which, it may be, are accurate enough for that person, which are true for the circumstances under which the experiments were made, but which require to be confirmed on dozens, or it may be, scores of different individuals, before they can be assumed to represent indisputable laws of the economy. But Boecker brings another objection against Lehmann's conclusions in the fact that they were drawn from observations made on his own person at periods so distant from one another that they cannot be fairly said to have been made under uniform conditions. In reply to which Lehmann shows that, so far as the weight of his body is evidence in point, his state of health was so equable throughout the whole period embraced by his researches, that in that respect they were made under uniform conditions. But a graver doubt still is cast upon Lehmann's conclusions by the analysis which Boecker gives of the numerical relations of the figures he obtained, in which he shows that in all cases the sum of the errors exceeds the difference between the means of his series of comparative observations, and consequently that they are insufficient to establish any positive conclusion at all. We may here mention an amusing instance which Boecker gives of the light in which some people regard medical statistics. He says that when he offered his paper, 'On the Action of Sarsaparilla,' to one of the German journals, its insertion was promised on the condition that "only the numbers expressing the *averages* should be printed, and not the series from which they were deduced," as the expense of printing the entire tables would be great, and "*there was no necessity for printing the original numbers when the means deduced from them were given.*" The individual who made this condition certainly had, as Boecker ironically remarks, a fine idea of the value of averages!

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* With the exception of the article "On the Action of Baths," by Dr. Parkes, above quoted, and the work 'On the Urine,' by the same author, we do not remember to have seen Radicke's name quoted in any English publication. We hope shortly to make our readers more conversant with his labours.—Ed.

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* Archiv des Vereins für Gemeinschaft. Arbeit., Band i. S. 515, and Band ii. S. 1.

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* Wissenschaftlichen Begründung der Wassercur, p. 174. Coblentz, 1853.

We have devoted so much space to Boecker's researches on the influence of cold hip-baths, that we must notice very briefly the results at which he arrived with regard to that of cold shower-baths, and the wet sheet. To test the operation of the power of these agencies, sixteen observations were made with a shower-bath of water at 52° F., and having a fall of forty-six feet; each observation lasting seven minutes. The numbers obtained when compared with those given by a series made without the use of shower-baths, show that in Boecker's case the shower-baths appear to have produced no effect whatever *within a period of three hours after taking them*, either on the general weight of the body, the quantity of urine, or the amount of urea or chloride of sodium. He allows, however, that in this, as in the case of the hip-baths, three hours was hardly long enough to exclude the possibility of any effects having been produced, and promises to institute another series of observations in which the points involved shall be examined during a longer period. But even if a second and more protracted investigation should give a similar result, it will by no means follow from it that the cold shower-bath has no influence at all upon the system. It would require a very numerous and coincident series of observations to establish this in the face of the abundant evidence in favour of its therapeutic efficacy which every clinical observer must have collected for himself. And it would almost appear, from the negative results at which Boecker has arrived in both these cases, as if either his system were singularly unimpressionable to these agencies, or else, which we much suspect, that we must look for the effect of their action in some other direction than that in which he has sought it. At any rate it is somewhat singular that, notwithstanding his assertion that he could not detect any influence of the shower-baths upon the urine or the weight of his body, his own numbers show that at the end of the three weeks during which the experiments were made he had gained more than two pounds in weight. But after all, even if we admit the accuracy of his negative conclusion, it only shows how difficult it is to determine what may be the therapeutic value of any given agent by simple observations on its physiological action. Here is an agent, whose influence in pathological cases is undoubted, and yet no evidence can be obtained of its producing any effect whatever upon the healthy organism. This is one of the many facts which go to prove that our best hope of establishing a rational classification of therapeutic agents is rather by means of careful and exact clinical observation than by experiments on the lower animals, or on man in a state of health.

With regard to the wet sheet, twelve observations undertaken by Lampe upon himself appear to show that this application *probably* increases the metamorphosis of tissue, since the loss of weight was sensibly, though not considerably greater, whilst the wet sheet was employed, than under ordinary circumstances. There was, however, no increase either in the amount of the urine or the fæces, but a diminution in the alkaline phosphates of the former. The products of increased metamorphosis must therefore have escaped by the lungs and skin; in corroboration of which supposition Boecker states that profuse perspiration during the use of the wet sheet is very common. Both this statement, and that of the increased loss of weight by the body generally, are in direct conflict with the assertions of Howard Johnson, who found that during the use of the wet sheet the body maintains its normal weight, and that sweating is *never* produced: but they agree with the observations of Wundt,* who states that rapid loss of weight occurs during the use of the wet sheet. It must be regretted that observations undertaken with such praiseworthy care and diligence as those of Boecker have not led to more positive results. There is certainly no other science but physiology in which so much skilled labour and time are expended without leading, often, to any tangible result. Let us hope that such conscientious labours find their reward in the satisfaction of the *besoin de travailler* which is the necessity of some men's lives, if in no other way.

The paper by Professor Virchow 'On the Action of Sea-bathing at Misdroy,' is a most valuable contribution to our knowledge, and is in every way worthy of the author's high reputation for scientific accuracy. Misdroy is situated in the North Sea, and is one of the two islands which form the delta of the Oder. It is much noted for its sea-bathing, and is greatly frequented for that purpose in summer by the inhabitants from the mainland of Prussia. Much information is contained in the earlier portion of Virchow's paper on its climate, prevailing winds, the character of its sea, and other points of interest to those who may purpose visiting it. The special object with which the author's researches were made, was to determine the influence of sea-bathing upon the temperature of the body, the circulation, and the respiration. The experiments were made in autumn, on his own person, and the first set lasted from the 17th to the 29th of August; the second from the 8th to the 13th of September (in both cases inclusive), so that altogether nineteen are recorded. Previous to making them, the author had bathed for a few days; he was also in good health, and lived liberally on a mixed diet. To register the variations of the temperature of the body, one of Geissler's delicate thermometers was used: on two occasions the observation was made in the axilla, but on the others in the mouth and the palm of the hand. Full tabular records are given of the results of the observations, the points noted in them being, the temperature in the house, in the open air, and in the bathing machine; of the sea, and the amount of motion in it (indicated roughly): the temperature of the mouth and hand before and after bathing, as well as the number of the respirations and pulsations under the same circumstances; and the duration of the bath, together with general information as to food, occupation, &c., during the time that the researches were continued. Throughout the whole period the sea only varied 3°·2 C., and the air only 5°·1 C. in temperature, a uniformity which adds greatly to the reliability of Virchow's conclusions. The average temperature of the mouth was 36°·3 C., and that of the axilla corresponded exactly with it. In the house the temperature of the hand agreed with that of the mouth, but directly after leaving the house the hand began to cool, and a marked difference between the two became perceptible. It is very interesting to find that this difference diminished as the observations were continued: on the ninth of August the temperature of the hand in the bathing machine was 33°·2 C.; but on the latter days of the investigation it was on an average 35°·5 C.; showing that the effect of bathing was *to permanently increase the activity of the peripheral circulation.* The author also notes that the sensation of heat in his hand in no way corresponded with the real temperature.

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* Archiv für Wissen. Heilk., Band iii. S. 35. It must be noted with regard to Wundt's cases, which were only two in number, that they were both of them women, and both of them suffering under hysteria. Still, the coincidence between the great increase of the urine, urea, and chloride of sodium in them (nearly double that excreted during a corresponding period without the sheet), hardly leaves a doubt that a portion of that increase is due to that agency.

The bath always caused a diminution of temperature in the body (notwithstanding the exertions he made while in the sea and in walking down to it), which varied from 1° to 2° C. Virchow sometimes stayed in the water as long as half an hour, and generally found that the longer he remained in it the more the temperature of his body was lowered. Although the diminution of temperature was comparatively so slight in the mouth, it was much greater in the palm of the hand, amounting on one occasion to 11°·4 C., and on an average to 8°·02 C. In reference to this point, the author points out that the influence of the air as a cooling agent is much greater than that of the water, and that the maximum of cold was reached in September, the minimum in August. This agrees to some extent with the statement of Currie,* who found, however, that the diminution of temperature in the mouth was as much as 4°·5 C. after half an hour's exposure in a bath, the temperature of which was, it must be stated, as low as 5° to 7° C.; and that the fall was even greater when the bather was exposed for a short time on coming out of the water to a sharp breeze. The observations of Esmarch, Aubert, and Forster, Kahltor,† Fleury,‡ and Hjelt§ also confirm those of Virchow, though those of the three latter are in many respects so imperfectly recorded as greatly to diminish their value. We may observe that the accuracy of Virchow's statements as to the temperature of his mouth is greatly increased by his having kept it steadily closed whilst bathing.

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* On the Effect of Cold and Warm Water in the Treatment of Fever. Liverpool, 1798.

† Uber die Zweckmässrige Anwendung der Haus- und Fluss-bäder. Wien, 1822.

‡ Practisch-Kritische Abhandlung über die Wasserheilkunde. Stettin, 1853.

§ Bildrag till laran om det Kalla vattnet säsom läkemedel. Helsingfors, 1855.

The conclusion at which Virchow arrived with regard to the effect of bathing upon the pulse and respiration was, that whilst the pulse was increased by motion in the morning air, and the respirations slightly diminished, especially when the air was cold, the bath increased the number of respirations without definitely affecting that of the pulse, its beats being sometimes increased, and sometimes diminished. Hence the *ratio* of the pulsations to the respirations was diminished in the proportion of 3·8 : 1 to 2·9 : 1. But although his own observations enabled him to draw no decided conclusion as to the influence of bathing on the pulse, Virchow believes, from a comparison of special circumstances, that we are justified in assuming that, under certain conditions, cold bathing does cause a diminution in the activity of the heart—a fact of which we think that there can be no doubt. Although there is some discrepancy in the statements of observers with regard to this point, in the main they coincide in asserting that a greater or less amount of diminution of the pulse is produced by bathing.

Wiedasch, it is true, in the work to which we shall presently refer, states that out of 39 cases, the pulse was increased by bathing in the sea in 27, and he attributes the diminution in the 12 to pathological causes, and even goes so far as to assert that such an increase is the test of the beneficial action of sea-bathing. But Wiedasch's observations were not made upon himself, and there is great reason to doubt whether he has not mistaken the temporary effect of emotional excitement upon the heart in persons of a susceptible nervous organization for the decided physiological action of the bath. Guastetta,* Harse,† Poitevin, Marteau, Currie, Sieveking, and a host of others, who have written on the subject, all agree as to the sedative influence of cold bathing upon the heart. Falconer,‡ indeed, states that the *immediate* effect of instantaneous cold immersion is increase of the pulse; but he adds, that a prolonged cold bath permanently depresses it. Macard found that, with few exceptions, all baths under 35° C. diminished the pulse, and the more so the longer the bath lasted, or the higher the pulse was before taking it. This latter assertion quite agrees with the result of some observations of our own, both upon the cold shower, and in an opposite direction, on the hot-air bath; for we have always found that the depressing effect of the former was always greatest when the pulse was high, whilst the stimulating effect of the latter was greatest when the pulse was low before taking the bath. With regard to this point, it may be observed that many of the above-quoted observers leave us in doubt as to what amount of exercise they took immediately before or after the bath, so that we cannot tell whether the diminution relates to an accidentally heightened state of the pulse by walking to the bath, undressing, &c., or to its normal standard. We believe that any discrepancy in the statements of the above authorities as to the influence of the bath on the pulse is explicable by the different circumstances under which the observations were made; and that the sedative influence of cold baths is proportionate, not only to the lowness of their temperature, but also to the amount of surface which is exposed to them, and to the natural fluctuations of the pulse exhibited by the individual experimented upon. Avicenna long ago remarked, that when a cold bath exerted its influence upon the internal parts of the economy, it depressed the heart's action; but when it only affected the external parts, the pulse became fuller and more rapid.

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* Studii Medici Sull' acque di Mare. Milano, 1842.

† Schmidt's Jahrbuch., Band xiv. S. 147.

‡ Observations respecting the Pulse. 1796.

Virchow discusses at some length the question of the cause of this diminution in the activity of the heart. Is it caused by the peripheral cooling of the body by the cold water acting through the cutaneous nerves,—or through the blood? And if by the latter, does the cooled blood act as a direct sedative upon the heart, or indirectly through the vagus? The illustrious Harvey* remarked, that direct application of cold to the embryo of the fowl caused a diminution of pulsations in the fœtal heart; and Calliburces found that by plunging the pulsating heart of a frog into water at 10° C., he could cause its immediate quiescence, and that he was able to restore its activity by introducing it into water at 50° C. On the other hand, the researches of Valentin, Wundt, and others, show that section of one or both vagi causes notable diminution of the temperature of the body; but it remains to be shown whether application of cold to the peripheral extremities of the vagus has any effect on the heart or not. We have applied cold (ice) directly to the vagus of a cat in the neck, but without producing any definite results on the activity of the heart.

With regard to the action of bathing upon the other organs of the body, Virchow offers no certain information; but he quotes an observation of Kahltor which, in his opinion, proves conclusively that the skin does absorb water. A strong man, aged twenty-six, remained for an hour in a bath at 40° R., during which time the weight of his body diminished by 6 lbs. 3 ozs.; but when placed for an hour in a bath at 2° R., he gained 5lbs. in weight! This, though the amounts are improbably large, coincides with the results which Duriau† obtained; but the whole question of the absorbing power of the skin is in such an unsatisfactory state, that isolated experiments like this can do little or nothing towards clearing up the discrepancies of various observers with regard to it.

The primary effects of sea-bathing are summed up by Virchow as follows:—The blood is propelled from the peripheral to the central parts of the circulating system; and, as a consequence, a corresponding disturbance of the functions of the central organs takes place. Loss of energy is felt in the motor, and of sensibility in the sensory nerves; and even the muscles themselves are less easily excited than usual. The secretions of the skin and internal organs are at this time probably reduced to a minimum, and the body is in much the same state as that of an animal whose skin is covered with varnish. The diminution of the bodily temperature which takes place is probably as much due to a diminished production of heat as to an absolute abstraction of it. The secondary effects Virchow found to be—the temperature of the mouth generally rose in half an hour after the bath above its height on entering the sea, and during the middle of the day and afternoon continued some degrees above its normal standard; whilst in the evening it sunk again, but without ever reaching the temperature of the morning. In the hand and other peripheral portions of the body it was two or three hours before the normal degree of temperature was regained, but eventually it equalled that of the mouth. As might be expected from the increase of temperature noted in the body generally, the pulse and respiratory movements resumed their normal frequency in from half to one hour, and somewhat exceeded it during the latter part of the day. From these facts Virchow *infers* that the metamorphosis of tissue is increased by bathing, and he finds evidence in favour of this assumption in the more vigorous appetite which he enjoyed during the bathing period, as well as in the augmented weight of his body which followed. Into this and other more debatable portions of his paper, however, we refrain from entering here, both on account of our having already exceeded the space which we originally proposed to allot to it, and because we prefer to give our readers only the positive results which Virchow's observations have established, and to postpone drawing any inference from them until they have been confirmed by the researches of others.

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* Exercitationes de Generatione Animalium. Amstelod. 1651.

† Archiv Gen. de Méd. Fév. 1856.

The work of Dr. Wiedasch is ostensibly intended as a handbook for the bathers who frequent the island of Norderney, but it is in reality a treatise on the effects and uses of sea-bathing in general. And although it contains few positive additions to our knowledge on the subject, and is rather designed for the non-medical than the medical public, there is much in it that will well repay the perusal of any one who may desire to obtain a clear and succinct account of the physiological relations of, and therapeutic indications for, a course of sea-bathing. Dr. Wiedasch divides his work into two parts; in the first, he discusses the effect of sea-air, and in the second that of sea-water upon the organism. Under the former head he treats of the influence of the increased density of the atmosphere at the sea level, its richness in saline particles and in watery vapour, and of the greater amount of ozone which it contains, upon persons coming to it from an inland district. Under the latter, he considers more particularly the action upon the economy of the temperature and chemical composition of the sea, as well as of the shock of its waves (Wellenschlag). Indeed, the importance which most German writers attribute to this latter element of sea-bathing seems rather exaggerated to English readers, and is probably explicable by the more exposed position of the bathing places on the North and Baltic seas, and by the greater roughness of the water at them, compared with the more favourite resorts on our own coasts. Dr. Wiedasch describes very carefully the nature of the reaction which follows the judicious employment of the cold bath, and lays down some useful rules for testing whether bathing is beneficial or not. Although the work is on the whole very intelligibly and practically written, it exhibits an occasional tinge of that speculative pedantry, without which it would appear that but few German works could be complete. Thus, the author devotes some space to the discussion of the questions whether the fluids excreted by the mollusca, or the adhesion of the stinging weapons of the Radiata to the skin (*wie es von einzelnen erwiesen ist!*) have much to do with the beneficial effects of bathing or not; and he also dilates upon the probable existence of important electrical relations between the bather and the sea, but this he gravely allows to be "ein schwieriges problem!"

Dr. Mess has been attached for some years as medical director to the large bathing establishment at Schéveningen, a place much resorted to by bathers, especially from the Hague, within three miles of which it is situated; he therefore speaks *ex cathedrâ* on the value of sea air and water as curative agents. The object of his work is more especially to point out the importance of studying the relative temperatures of the sea and air during the different months of the bathing season, with the view of employing the information so obtained in meeting the requirements of individual cases more completely than is done at present. For this purpose he examined the temperature of the sea and air daily from the 1st of June to the 30th of September in the years 1855, 6, and 7, at 6 A.M., and at mid-day. The results of his observations are given in three large diagrammatic tables, and they are, as far as we are aware, the only ones on record extending over so long a period. Although they contain several points of interest, their value is chiefly of a local kind, as they only represent the fluctuations of atmospheric and marine temperature at one given spot. They will, however, be useful not only as enabling practitioners to form an opinion as to the climate of Schéveningen, and its fitness for individual cases, but also as a means of comparison with that of other places. And we would point out to those of our medical brethren who may be residing at the sea-side, that they would greatly advance the science of meteorology by undertaking a series of observations of an analogous nature to those of Dr. Mess. An accurate knowledge of the relative variations of temperature in the air and sea on different parts of our own coast is much wanted; and full information on this point would often enable a medical man to form an opinion on the relative merits of various watering-places for individual cases on better grounds than he at present possesses. The importance of so doing is not nearly so much appreciated as it ought to be. To persons in good health it perhaps matters little whether in selecting the time of the year, or the hour of the day, at which they will bathe, they do more than consult their own inclinations; but for invalids, or those who though not invalids are very sensitive to variations of temperature, much care is often requisite in determining the hour and the season when sea-bathing will prove most beneficial to them. The want of discrimination shown in this respect is, we believe, the most common cause of the unsatisfactory results often obtained by invalids from a course of sea-bathing, and Dr.

Mess mentions several striking cases in corroboration of this view. The existence of such records of temperature as we have suggested would do much to remove any difficulty which medical men living inland may feel in giving advice on this subject, and Dr. Mess deserves well of the profession for the example he has set in instituting them. The results of his own experience have shown him, in opposition to the general opinion, that the early months of the season—viz., June, July, and the first weeks of August, are much better adapted for the majority of invalids than the latter ones. The reasons which he assigns for this superiority of therapeutic influence are, that the sea during the early months is much rougher, and that on this account, as well as from the temperature being less elevated, bathing is more bracing then than at a later period; and also that the greater length of the days during these months enables the patient to enjoy a much longer exposure to the sea air than during the month of September or the latter part of August. Of the rules generally which Dr. Mess lays down for the various cases in the treatment of which recourse is usually had to bathing, it is unnecessary for us to say anything, as they will be best studied by a reference to the work itself. We will only refer to two, which, from their being of some practical importance, as well as from their not being commonly recognised, are deserving of individual notice. The first is, that whilst anæmic patients are for the most part greatly benefited by sea-bathing, and that at almost any season of the year, those who suffer from chlorosis rarely derive any good from it, or, if they do, it is only when the time at which the baths are taken is most judiciously selected. The distinction here laid down between anæmia and chlorosis has received a certain amount of attention, but is deserving of much more than is generally given to it, not only on account of the theoretical considerations with regard to the ætiology of these two states which it involves, but also because, as in this case, their treatment often requires to be conducted on very different principles. The second rule which Dr. Mess lays down, and which we think worthy of consideration, is that whilst pregnant women of a robust constitution should not be allowed to bathe at all, or only with very great caution; those who have suffered from previous abortions, which have enfeebled their constitution generally and disordered the special functions of the uterus, recover their health sooner by the aid of bathing than by any other means, and proceed to their full term with the happiest results. The success which Dr. Mess has met with in the application of this plan of treating cases which are amongst the most trying and intractable with which a medical man has to deal, is such as to show that it is deserving of the commendation which he bestows upon it.

Intermediate in position between baths of air and water, and differing essentially both in their nature and uses from those of vapour, are the so-called "spray baths" which have lately been introduced into medical practice by MM. Sales-Girons and Mathieu (de la Drôme). The principle upon which the inventions of both these gentlemen are based is the same—viz., the reduction of mineral waters and medicated liquids generally to the state of spray or mist, but its application differs in the two cases. The object of M. Sales-Girons, the well-known editor of the 'Revue Médicale,' to whom the merit of originating this plan of treatment is due, is to employ medicated fluids in what he calls their "pulverized" condition for the topical medication of the lungs and air-passages.

The idea itself is in some respects by no means a new one, for "aspiratory" chambers are found in the remains of many ancient Roman baths, and the vapour promenades of Kreutznach, Nauheim, Ischl, and other watering-places, are well known. In some of these, particles of the saline constituents of the waters are actually carried up mechanically in the fumes which arise from the boiling springs; but, as a rule, the "vapours" of these establishments are nothing more than the vapour of water, to which all the therapeutic properties assigned to them are due. In the year 1856, M. Sales-Girons conceived the idea of carrying out on a larger and more complete scale the reduction of mineral waters to a state of spray, arguing that if the inhalation of those vapours which were the result of a natural but imperfect operation had been shown by experience to be beneficial, a more efficient application of the process might lead to still more potent effects. With this view he instituted at Pierrefonds, a watering-place near Compiègne, a chamber in which the patients respired the elements of the mineral spring (sulphurous) disseminated into the finest spray. That in so doing M. Sales-Girons attained at least one part of his object—viz., a *bonâ fide* vaporization of the liquid so treated, is shown by the report of MM. Patissier and Ossian Henry to the Académie de Médecine of Paris, in which they state that they have satisfied themselves that the spray of the chamber at Pierrefonds contains all the elements of the original mineral water. For this process M. Sales-Girons received from the Académie as a recompense a silver medal. Subsequently to this he has devised an apparatus of so portable a nature that it can easily be introduced into an ordinary room, by means of which any fluid may be readily reduced to as fine a spray as that produced by the large machine employed in his "chamber of inspiration." Some of the results of treatment by this method of medication are contained in the work whose title we have given above; and though we think that the dependence of the good effects obtained in some of the cases upon the respiratory treatment alone is doubtful, there is ample evidence that the process promises to be of considerable service for certain therapeutic purposes. Indeed, no one who reflects for an instant upon the vast extent of the pulmonary mucous membrane, upon the activity of its absorbent powers, and upon the utility which the imperfect methods of locally medicating it at our present disposal unquestionably possess, can doubt that a process by which medicinal agents may be directly applied to it in any given strength would be a most important contribution to our means of combating disease.* Some doubts have lately been attempted to be thrown upon the value of M. Sales-Girons' invention by MM. Pietra-Santa, Réné Brian, and Champouillon. The former of these observers asserts, from investigations upon "pulverized" water at the "Eaux Bonnes," that the chemical elements of the "pulverized" water are *not* carried over in the spray, and consequently not inhaled by the lungs. M. Brian caused two or three of the lower animals to inspire a solution of pulverized perchloride of iron, and after killing them, could detect no traces of that substance in the bronchi or air-passages of the lungs. M. Champouillon caused a patient with bronchitis to inhale the spray of a similar solution, and on testing his expectoration could detect no traces of iron. With regard to the first of these objections, the evidence of MM. Patissier and Ossian Henry, who are at least as good chemists as M. Pietra-Santa, shows distinctly that the spray of the "pulverized" water at Pierrefonds contained all the elements of its sources of origin (*toutes les substances propres à l'eau*), and they state that *so far* the object of M. Sales-Girons is attained. Upon the second question at issue—viz., the passage of the medicated spray into the air-passages, we think that the experiment of M. Brian proves little or nothing, as it is perfectly possible that the spray, unless carefully carried to the aperture of the larynx by means of a tube, might have been all condensed on the walls of the mouth and nares of the animals experimented on, and have thence passed by swallowing into the œsophagus. We have found, after repeated trials with M. Sales-Girons' apparatus, that if the solution employed is sufficiently strong, and the spray carried, as we have mentioned, by means of a tube to the back of the fauces, most distinct evidence of its passage into the lungs may be obtained.[*] This is not the place to enter into details on the subject, but we are bound to add that the penetration of the spray into the air-passages, though it does occur with careful management, is by no means so easily demonstrated as M. Sales-Girons seems to assume. Although the first few inspirations cause a little cough, the trachea soon gets accustomed to the impression of the fluid, and the inhalation may be carried on without difficulty for a considerable time.[†]

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* "Thus, both in man and the lower animals, the respiratory apparatus is of all parts of the economy that which combines in the highest degree the conditions of perfection as an absorbing instrument, and one, too, by which the introduction of foreign matters into the organism is most easy and rapid."—Milne-Edwards: Leçons sur la Physiol., tome v. p. 201.

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[*] The following experiment by MM. Ossian Henry and Flubé may be cited in contradiction of the statements of M Brian. A young pig, whose nostrils had been previously stopped up, and whose mouth was open, was exposed for half an hour to the spray of a pulverized (we must apologize for using the word which M. Sales-Girons has appropriated to designate his process, but which it is easier to cavil at than to replace) solution of a salt of iron. The animal was then killed and opened, and the deeper bronchi showed, on applying the appropriate tests, that the ferruginous spray had penetrated into them. Indeed, it is as difficult to conceive how, if the particles of the fluid, mixed up with air as they are, are once carried into the mouth or nostrils, they can fail to pass into the trachea, as it is to suppose that the spray of any given liquid, which may be completely dissipated in that form during the process, can do otherwise than exhibit all the elements which the liquid in its original condition possessed.

[†] Since the above was written, a communication has been made to the Académie de Médecine of Paris by M. Démarquay ('Bulletin de l'Académie,' Sep. 24, 1861), on the subject of the penetration of "pulverized" liquids into the respiratory passages; and the results obtained by that gentleman are so decisive that we have thought it desirable to insert a note of them here. The experiments of M. Démarquay were instituted at the Maison Municipale de Santé of Paris, with the assistance of M. Lecomte, and in the presence of MM. Mialhe, Sée, Pietra Santa, Giraud-Teulon, and the students of the establishment; and they were undertaken in consequence of the "happy results" which M.

In stating our conviction, from personal experiment, that the particles of spray do pass into the trachea, we are far from wishing to prejudice further inquiry into the value of M. Sales-Girons' invention. On the contrary, the whole subject requires a much more thorough investigation than it has yet received, the results of which will, we have no doubt, place the therapeutic utility of the "pulverizing" process, so far as its respiratory employment is concerned, on a much more satisfactory footing than that on which it stands at present.

But if the object which M. Sales-Girons had in view in devising his new method of treatment must at present be considered as still *sub judice*, the invention of M. Mathieu stands on a less questionable footing. Starting from the idea that in a common bath of mineral water, the only portion of the fluid which is of any utility is the thin stratum which is immediately in contact with the surface of the body, and which is only slowly and imperfectly renewed, the end which he proposed to himself was the invention of a process which should renew indefinitely the fluid in contact with the skin, whilst it at the same time economized the quantity to be employed. With this view he has constructed an apparatus which accomplishes, though by a different process, the same result as the *pulverisateur* of M. Sales-Girons—viz., the reduction of the fluid to a state of fine mist or spray; and he introduces the spray so formed into a small chamber, in which the patient to be exposed to its action is seated, naked. By this means, it is stated in the report on the subject to the Académie de Médecine, the body of a patient may be exposed to a bath of fine spray for an hour with the expenditure of only three or four litres (five to seven pints) of fluid, and that costly medicinal agents may be administered at a comparatively trifling expense. M. Hardi, of the Hospital of St. Louis, has made an extensive series of researches on the comparative physiological action of this and of the ordinary system of bathing, and has found that they resemble one another closely, both in the

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Démarquay had obtained from the application of the "pulverizing" process in the treatment of chronic affections of the pharynx and larynx. In the first series of experiments a number of rabbits were made to inhale the spray of a "pulverized" solution of perchloride of iron. At the end of five minutes some of the animals were killed, and in almost every case the ferrocyanide of potassium revealed the presence of iron "in the larynx, trachea, bronchi, and pulmonary parenchyma." Of the rabbits which were not thus killed, nearly every one died within twenty-four hours *of violent broncho-pneumonia*—a clear proof that the liquid had reached their lungs!

In the second series, a number of dogs were subjected to the same treatment, with very similar results. In one case a piece of paper saturated with perchloride of iron was introduced by an artificial opening, into the trachea of a dog which had been made to inhale a "pulverized" solution of tannin; when drawn out again, it gave satisfactory evidence of the penetration of that agent into the trachea.

In the third series, M. Démarquay and the students of the establishment convinced themselves by the sensations experienced in the chest, after inhaling a "pulverized" solution of tannin (as we have ourselves done), that the spray of that liquid really enters the air passages.

Finally, a patient with a permanent tracheal fistula was made to inhale by the mouth a pulverized solution of tannin, the presence of which in the trachea was unmistakeably detected at the fistulous orifice by the appropriate tests. These experiments, undertaken by a competent observer, and performed in the presence of some of those who had previously denied the fact of penetration, are so decisive as to leave no doubt that, so far as the possibility of introducing medicated liquids into the lungs is concerned, M. Sales-Girons has fairly accomplished the object which he had in view.

sensations experienced by the bather, and in the effects produced upon the pulse and urine.

The evidence as to the therapeutic efficacy of M. Mathieu's system of balneation is very satisfactory, and the more so as it does not depend upon the testimony of himself or any other interested person, but is mainly derived from an eight months' trial at the Hospital of St. Louis by M. Hardi. The observations of M. Hardi have been laid before the Académie de Médecine of Paris, and a report founded upon them has been drawn up by M. Gavarret. From this it appears that spray baths of starch, sulphide of potassium, iodide of potassium, natural, mineral, and sea waters, as well as of other medicated fluids, have been used in the treatment of various diseases of the skin and scrofulous affections, with very marked success, and that after the patients had been submitted to the ordinary methods of treatment without any improvement. But it is especially in affections of the face and scalp that the spray-baths have most exhibited their therapeutic efficacy, inasmuch as the mildness of their action allows the patient to expose the entire head to them for a considerable time without any inconvenience. In the *brochure* published by M. Tampier we have also a series of cases in which the spray-baths have been employed under the superintendence of many of the leading medical men in Paris; in the majority of these the beneficial results obtained were very decided, especially in secondary syphilitic affections of the skin which had been treated by mercury without effect.

By M. Mathieu's invention the administration of baths of natural mineral waters is brought home to the very door of the invalid, who may now luxuriate in the waters of Ems, Vichy, and Carlsbad without leaving his own place of residence. It is to be hoped that a more extended experience of its application will confirm the belief expressed in M. Gavarret's report, that the new system of balneation is "a real therapeutic conquest."

We have devoted greater space to the notice of these new plans of medication than we otherwise should have done, chiefly in consequence of their having as yet received little or no attention in this country; and we shall feel that we have not done so without effect if our remarks should lead those who may have the opportunity to give them a fair trial to examine their pretensions with the view of bringing them fully and fairly before the profession.

We hope to return to a more complete consideration of these spray-baths at a future time, when their physiological action has been more thoroughly investigated than it has as yet, and when their therapeutic efficacy shall have been tested by a wider experience. Meanwhile, we feel justified in expressing an opinion, that whether the invention of M. Mathieu shall eventually realize the anticipation of M. Gavarret, or not, both that and the process of M. Sales-Girons constitute an entirely new step in medical treatment; and we have little doubt that, even if their direct utility should prove to be less than their originators expect, they will indirectly lead to other results of no small importance.

We have already so far encroached upon the space at our disposal, that we must postpone to another opportunity the consideration of the recent contributions to our knowledge of the effects of two other varieties of "bath" whose therapeutic employment has but lately come into vogue; we refer to those of compressed and heated air.

Review VI.

1. *Report of the Proceedings of the Fourth Session of the International Statistical Congress held in London, July,* 1860: *Sanitary and Vital Statistics of the Army and Navy.* 1861.
2. *Statistical, Sanitary, and Medical Reports for the Year* 1859: *Army Medical Department.* 1861.
3. *Statistical Report on the Health of the Royal Navy for the Year* 1856. Ordered by the House of Commons. 1858.
4. *Statistical Report on the Health of the Royal Navy for the Year* 1857. Ordered as above. 1859.

The greatest *desideratum*, unquestionably, in the study of medical statistics is an accurate and continuous registration of all diseases as they occur in any large portion of the community, and specially among the labouring and necessitous classes. Some of the most interesting problems of this science must await their solution until the indispensable pre-requisite of regular statistics of sickness can be had; for obviously the facts must be clearly ascertained before we begin to argue, and our data be sufficiently abundant and varied, as well as be duly arranged and collated, before any trustworthy conclusions can be formed. Hitherto but little has been done—nay, scarcely a sure step has been taken—to supply this want even in this country, which has now for upwards of twenty years had a more exact record of the mortality, and of the causes of the mortality, among its people, than any other nation has ever possessed. And who is ignorant of the inestimable benefits which have already flowed to social welfare as well as to medical science from this important work, with which the name of our distinguished countryman, Dr. Farr, will ever be gratefully associated? Has not the health, and therefore the effective strength and the labour-power, of our population been steadily—only too slowly and too partially—improving of late years? This is a great national blessing for which we cannot be too thankful. Deaths have been diminishing; and when we speak of deaths, be it remembered that every death is the expression not only of the loss of one of the community after more or less lengthened suffering and infirmity (the cause, it may be, of much privation to and even the positive destitution of an entire family), but is also the invariable exponent of a no small amount of concomitant sickness and distress in other persons at the same time. For every working man that dies, there are two of the same age constantly on the sick list and off work in consequence, besides a third man who has become permanently disabled, to a greater or less degree, from the effects of disease in a previous year. "The numbers constantly suffering from acute sickness at any year of age," says Mr. Edmonds,* "will amount to double the number of yearly deaths, and the number of invalids or permanent sufferers from past sickness will be equal to the number of annual deaths." For example, out of a thousand persons of our population, all in their thirtieth year, about ten will die annually, twenty will be constantly on the sick list, and there will be ten permanently disabled from active work from the effects of sickness suffered in previous years.

It is not till we realize these things to our mind that we begin to get a glimpse of the large unknown amount of suffering and sorrow which every item in an obituary represents, or that we can appreciate at its just value every successful effort to reduce the death-rate among a people. Every life saved may be regarded as an index of a threefold saving of constant disablement and loss of labour, in consequence of bad health among the working classes of a community. But this view alone gives a very imperfect idea of the entire amount of sickness to every death, which occurs in the general population. Probably we shall not be far wrong if we estimate that five-and-twenty or thirty cases, at least, of illness take place for every one which proves fatal. Many diseases occasion a large amount of distress, permanent as well as temporary, and contribute nothing to the bills of mortality. Ophthalmia and rheumatism are of this class; maladies the prevalence and the effects of which among the working classes it is most desirable to ascertain, but which scarcely ever figure in a register of deaths. And so it is with most diseases of the skin, with ulcers, &c.; although they always form no inconsiderable a part in the records of dispensary and hospital practice, they add but little to an obituary. Hence the obvious necessity of a trustworthy registration of sickness, no less than of an accurate registration of mortality, for the attainment of the full advantages to be derived from that most important branch of medical study—vital statistics in all its manifold bearings.

The question is, how is this much-desired information to be had? is there any existing channel or ready means by which it might be obtained? or must some new machinery be devised for the purpose? A brave effort was made to this end, a few years ago, by the metropolitan medical officers of health, who worked at it most ably for some time, and with every prospect of doing much good, until their labours were suddenly stopped by the ungracious withdrawal by the Government of the trifling money grant to defray the necessary expenses. Weekly returns of all fresh cases of disease coming under treatment in the workhouses, hospitals, dispensaries, and some other public institutions in London, were regularly arranged and tabulated, with appended memoranda on meteorology, the sanitary condition of localities, &c. These registers, at the time of their discontinuance, contained a record of several hundred thousand cases of sickness, probably the largest and most complete registration of the sort in civil life that has ever been published. By affording an immediate notice of the occurrence of zymotic disease, and indicating the spots where most sickness prevailed in the metropolis, they greatly aided the medical officers in maintaining a vigilant supervision over their respective districts, so necessary for the preservation of the public health. At the meeting last year of the International Statistical Congress, a strong opinion was expressed as to the great desirableness of having such returns as those now alluded to, and a resolution was adopted that "the co-operation of all the different Governments (represented at the congress) be requested in collecting and publishing returns of sickness and meteorology in their several capitals."

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* See his paper on Statistics of Health in the 'Proceedings of the Congress.'

The Epidemiological Society also have recently had this important subject under their consideration, and been urging it on the attention of the profession and the public. They have suggested a simple, and what seems to be a very feasible, scheme for utilizing the statistics of disease among the pauper population of the country, through the co-operation of the parochial medical staff (there are upwards of 3000 of these gentlemen in England and Wales)—an admirable existing machinery such as no other country in the world possesses. When it is considered that, although these officers are required to keep a register of all cases treated among the hundreds of thousands of the poor and labouring classes under their supervision, no scientific use has yet been made—or indeed, under the present *regime*, can be made—of the mass of valuable materials from year to year accumulated, every one must lament the continuance of a system so wasteful of labour, and withal so unprofitable.*

Until this channel be opened up, we must look to other sources for authentic information as to the amount and nature of sickness occurring among large bodies of individuals, during successive years. The statistical medical records of the army and navy afford much valuable instruction on this head, as far as regards males at the working period of life—viz., between eighteen and forty-five years of age—and deserve to be much more generally known to the profession than they have hitherto been, even to the medical officers of the two services themselves. For, besides the ample registers they contain as to the sickness, mortality, and invaliding over successive years among our soldiers and seamen, nowhere else will be found so many interesting details respecting the influence of climate and region on health; the hygienic effects of food, clothing, and accommodation; the action of spirituous drinks in the production of disease; the results of fatigue and protracted exposure under varying circumstances of care or neglect; also respecting the geographical distribution of different epidemic diseases, the circumstances attending their origin and spread, and the power of local conditions and other agencies in aggravating or diminishing their frequency and fatality. The opportunities for early and exact information on most of these matters enjoyed by military and naval medical men are calculated to give a special value to their experience on numerous points of sanitary and preventive medicine—a branch of professional knowledge for which, indeed, we are mainly indebted in the first instance to the acumen of such men as Pringle and Jackson in the one service, and Lind and Blane in the other.

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* At some of the annual meetings of the National Association for the Promotion of Social Science, the subject of the registration of diseases has been canvassed, and its necessity strongly urged. Mr. Rumsey's paper (to which we alluded at page 48), read at Bradford in 1859, and published in the ensuing volume of the Transactions, well deserves perusal; and at the recent meeting held in Dublin, a valuable communication from Dr. W. B. Richardson excited much interest. The weight of the Association's influence might probably be brought to bear with good effect upon the Poor-Law Board, with which must mainly rest the adoption of the much needed reform.

The health returns of the army began to be published about three-and-twenty years ago; prior to that time nothing had been done to reduce them to order, or turn them to any useful account. Between 1838 and 1853, five statistical reports on the sickness and mortality in our troops at home and on foreign stations appeared, for which the public is indebted to Colonel Tulloch, the late Dr. Marshall, and to Dr. Balfour. Among the many useful changes resulting from the labours of the Royal Commission to Inquire into the Sanitary Condition of the Army, is that of having in future an annual report, in place of the former system of the occasional issue of more bulky documents at long intervals, and embracing the experience of seven or ten years together. The first of these annual reports, for the year 1859, was briefly noticed in our last number, and we then promised to examine its leading contents in connexion with the results of the previous returns, and also with the returns of the sister service.

The publication of the 'Statistical Reports on the Health of the Navy' commenced in 1840. During the next fourteen years, three or four were issued under the editorial care, first of Dr. Wilson, and afterwards of Dr. Bryson. To the latter gentleman the profession owes also a separate and very valuable report on the health of the African squadron in 1847. In 1857, the statistical returns of the Baltic and Black Sea fleets in 1854 and 1855 were published; and in the following year, the medical department of the navy set the example of beginning the issue of annual reports of the service in all the various stations of our fleets throughout the world. The first published was for the year 1856, the second for 1857, and the third (now in preparation) will embrace the experience of 1858.

What we propose in the following pages, is to attempt briefly to compare the health returns of the army and the navy, with the view of ascertaining the general results of their joint experience, and of seeing how far the experience of the one agrees or differs from that of the other in their leading features, as far as the data of the two services hitherto published will permit. When the system of annual reports in both has been in operation for a few years, and the same nomenclature and classification of diseases has been adopted, as henceforth will be the case, such a comparison will be alike more easy and more instructive. Still, the present attempt may not be altogether unprofitable; for what is true in the case of our sailors, cannot but be, in the main, more or less directly applicable to the case of our soldiers, and *vice versâ;* nor can any sanitary and hygienic improvements be effected in one service without reacting on the other. The two services touch each other at so many points—as, e.g., in the conveyance of troops in transports and men of war, the landing of marines and seamen in military operations, &c.—that the experience of the one cannot be indifferent to the other. There are, of course, special circumstances in the employments and duties peculiar to each service, not without their influence on the health of the persons engaged; but these are of inconsiderable moment as compared with the broad features of general resemblance in the condition of two large bodies of men of the like ages, and scattered alike in a somewhat similar manner over the face of the globe. No other country enjoys such ample and varied opportunities and facilities for the widest scientific observation upon all matters relating to the welfare of armies and fleets, under every imaginable circumstance of climatic and regional diversity; and when to this consideration it is added that—with the single exception, we believe, of the United States in respect of their troops—no other nation has commenced the practice of publishing authentic returns of the health of their army or navy, it will be at once seen how great are the advantages at our command for the study of medical statistics, not only among our own immediate home population, but also in our widely-scattered colonial possessions.

And first, as to the average amount of sickness that has hitherto prevailed in the army at home and abroad. Until within the last two or three years, the daily sick-rate—in other words, the number of men daily on the sick list, and therefore off duty—among the troops of all arms, quartered in the United Kingdom, averaged forty-six or forty-seven per thousand of the mean strength. It was, as might be expected, highest in the infantry of the line, among whom there are always a number of invalids sent from foreign stations to their depôts in this country. Among the Foot Guards, the number off duty from sickness averaged throughout the year about forty-three per thousand. As the chronicity of certain lingering diseases may keep the daily sick-rate high although the regiment is generally healthy, it is necessary to correct this source of fallacy by ascertaining the proportion of fresh admissions into hospital to the strength, during the twelvemonth. This proportion averaged, among the troops at home, about a hundred per cent. of the entire force, so that the numerical strength of the troops indicated pretty nearly the number of men who had passed through hospital, in the course of the year.

The duration of each admission or attack of illness over the entire force seems to have averaged sixteen or seventeen days; and the average period in hospital to each man, in the course of the year, was estimated at between fifteen and sixteen days. In 1859, both of these averages were somewhat higher. The mean daily sick-rate, too, that year was considerably above the estimate in the previous returns, owing probably in part to the records of the average sick being kept with more accuracy.

Mr. Edmonds remarks:

"The average duration of sickness in the general population of the working age—say from twenty to sixty-five years—has been estimated by Dr. Farr at thirty-six days, or one-tenth of a year. Since each person has, on an average, an attack of sickness once every three years, it will ensue that the average duration of the sickness suffered by each individual of the working population in one year will be twelve days, or one-thirtieth part of the year or of his lifetime."*

In the Mediterranean stations at Gibraltar, Malta, and the Ionian Islands, the daily sick-rate was in former years nearly the same as that among the Foot Guards; the admission-rate into hospital was somewhat higher than among the troops at home, but the average duration of each admission was nearly the same. In 1859, the number of daily sick appears to have been generally, chiefly in Malta, greater than it was formerly; and the average sick time to each soldier, as well as the average duration of each case of sickness, to have increased by a day or two.

In the cooler but more variable climate of the North American command, including the Canadas, Nova Scotia, New Brunswick, and Newfoundland, the soldier has generally experienced less sickness than either at home or in the Mediterranean; the number of men constantly off duty not exceeding from thirty-five to thirty-nine per thousand, and the mean admissions into hospital being scarcely above nine hundred per thousand, in the course of the year. In the Bermuda islands—which, although included in the North American command, must, from their semi-tropic climate, be viewed apart—the rates were much higher. The daily sick list of the garrison generally averaged between fifty and sixty, and the ratio of hospital admissions in the twelvemonth was about 1200, to every thousand men. Each attack of illness averaged seventeen days, whereas in Canada, &c., it scarcely exceeded fourteen days. In 1859, there was a marked reduction in the proportion of men constantly sick, and in the average sick time to each soldier, throughout the whole of the command, as compared with the results of previous returns. Even in Bermuda, the daily sick-rate had fallen from fifty-five to thirty-five per thousand of the strength, and the average time in hospital from twenty to twelve or thirteen days.

In the West Indies, the amount as well as the fatality of sickness among our troops used to be excessive. One in every eleven or twelve men in the entire command was continually on the sick list, and the ratio of hospital admissions to strength was such that the whole numerical force may be said to have passed through hospital every seven months. The sick time to each soldier in the twelvemonth averaged about twenty-eight days. Happily, a great reduction not only in the frequency, but also in the duration of sickness among the white troops serving in the West Indies, has taken place since the publication of the returns to which these observations refer (1817 to 1836). In 1859, the daily sick-rate per thousand was about fifty-eight in Jamaica, and only forty-eight in the Windward and Leeward Islands.

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* Loc. cit.

The mean sick time to each soldier had fallen to twenty-one days in the former, and eighteen in the latter command; the average duration of each case of sickness was nearly sixteen days in both.

Equally pernicious to the health of our European troops has service in the East Indies generally proved hitherto, as service in the West Indies used to be. The Bengal army have usually been the most sickly, the Madras army the least so. Among the troops quartered in Calcutta, Berhampore, and other stations in Lower Bengal, the annual hospital admissions during the twelve years from 1824 to 1836 averaged two hundred per cent. of the mean strength. And some recently published statements would seem to show that a like enormous loss of effective service from sickness has occurred, in more than one regiment, of later years.

As the Statistical Report for 1859 does not embrace the troops serving in India for that year, we must wait for the next report before we can ascertain what reduction in sickness and death has resulted from recent improvements in the condition of our European army there. Among the white troops in Ceylon during that year, the constant sick-rate averaged not less than seventy per thousand of strength; the mean sick time to each soldier is estimated at twenty-five days in the course of the year, and the average duration of each case at rather more than fifteen days—"results which correspond very closely with those obtained for the twenty years, 1817 to 1836." At the Mauritius, the health of the troops seems to have been more favourable than in Ceylon, and to have improved considerably since the former period, the mean sick-rate having fallen from sixty-eight to forty-eight in the thousand, and the average sickness to each soldier in the year from twenty-five to about eighteen days. In Australia, the sick-rate among the troops was nearly thirty-four, and in New Zealand only between twenty and twenty-one per thousand of strength.

On the other hand, the daily sick throughout the year averaged no less than one hundred and thirty-nine per thousand among the European troops, and about eighty-nine per thousand among the native troops serving in China, (Hong-kong chiefly, we presume); and the average sick time to each soldier is estimated at forty-seven days among the former, and thirty-two days among the latter. How much may be done by due sanitary and hygienic precautions to diminish the amount of sickness among our soldiers in these regions is conclusively shown by the results of the active campaign in the North of China, when the proportion of men in hospital from all causes, casualties of war as well as from sickness, was only forty-six per thousand of the European force, and thirty-two per thousand of the native force. During the last six months of our occupation in the Crimea, the daily sick-rate gradually became lower and lower each month, until, when the army left, it did not exceed fifty-one in the thousand of the total strength.

From the Army we pass to the Navy. The sick-rates among our sailors appear to be, on the whole, higher than among our soldiers. In 1856 and in 1857, the number of men constantly off duty from illness throughout our fleets was between 61 and 62 in every 1000, or between a sixteenth and seventeenth of the whole naval force, estimated at 51,730 men in the former year, and at 42,470 of all ranks and ratings in the latter year. The force is distributed over nine different stations—viz., the Home, the Mediterranean, North America and the West Indies, Brazil or East Coast of South America, the Pacific, China and the East Indies, Australia, Cape of Good Hope, and West Coast of Africa. To these must be added the Irregular force, which is not confined to any one station, but employed irregularly and on special service.

In 1856, the proportion of attacks of illness to the total strength, in the course of the year, was in the ratio of fourteen to every ten men in 1856, and in the ratio of sixteen to every ten men in 1857. In both years, the aggregate number of days of sickness throughout the entire service was such, as to allow rather more than twenty-two days off duty to every man in the twelve months.

The sick-rate varied much, not only on the different stations, but also in different ships on the same station and when similarly exposed and employed—a fact which is always suggestive, and requiring strict examination. The rate was, in 1856, lowest in the Mediterranean, the North American and West Indian, the Pacific, the Cape of Good Hope and the Australian fleets, ranging from 51 to 52 per 1000. On the Home Station, the ratio was 58, and in the irregular force it was as high as 72, owing "principally to the heavy loss of service from syphilis, and to the sickly condition of newly-raised men, of whom there was a greater proportion in these vessels than in others continually employed on foreign stations."

The most sickly of all the stations is the East Indian and China, where the average daily number of ineffective men, in 1856, was nearly ten per cent. of the entire force. The cause of this enormous amount of sickness is, mainly, the exceeding prevalence of alvine flux. In 1857, the sick-rate per diem on that station was not quite so high as in the previous year, being 88 in place of nearly 100 in every 1000 men. On the other stations, the daily sick-rates varied considerably as compared with those in 1856, being in some higher and in others lower than in that year. The general ratio we have seen to have been rather higher throughout the service.

And now as to the average mortality among our soldiers and sailors. The yearly death-rate among the troops in the United Kingdom was, according to the last published returns prior to the recent report, between 17 and 18 per 1000 men; it ranged from 14 in the Cavalry to 18 in the Infantry of the Line, and to 20 or 21 in the Foot-guards. This extremely high rate had fallen in 1859 to one half, averaging only 8 per 1000 of the entire strength, or little above that of the civil population at the same ages in the healthiest districts of England — viz., 7·23. "There are numerous circumstances," remarks Dr. Balfour, "which have led to this reduction, but I believe that a considerable portion of it is due to the sanitary improvements consequent upon the statistical results brought out by the investigation of the Royal Commission."

In the Mediterranean stations, the death-rate averaged formerly about 17 per 1000; it was generally lowest at Gibraltar and highest in the Ionian Islands. In 1859, the ratio had fallen at Gibraltar from its former figure of 13·58 to 7·76 per 1000, "which is almost identical with that of the infantry regiments serving in the United Kingdom in the same year." In the Ionian Islands, too, the health of the troops had notably improved as respects the amount both of sickness and mortality in 1859, as compared with the results of previous years. The death-rate had fallen from nearly 18 to 12·57 per 1000. At Malta, on the other hand, the rate of mortality in the garrison was higher in 1859 than it had averaged in the ten years 1837–46, as 19·02 exceeds 16·77. This increase was mainly owing to the greater prevalence of continued fever of the typhoïd type, traced to overcrowding, the bad site and sanitary state of barracks, and the use of unwholesome water. No fewer than 47 men fell victims to the fever; 1 case in every 28 proved fatal.

In Canada and Nova Scotia there was a marked improvement in the health of our troops in 1859, as compared with former returns. In the former, the death-rate was 9·70, and in the latter only 7·23 per 1000, whereas formerly the ratios were 17·42 and 16. The health of the troops in this command is liable to be affected by the circumstance that regiments, which have suffered much from sickness in the West Indies, are often sent direct to Canada, &c.

At Bermuda, also, a remarkable diminution of sickness and mortality had taken place. The death-rate was not quite 14, whereas it had previously averaged 38·8 per 1000 in the ten years 1836–47; or, if the epidemic yellow-fever year of 1843 be excluded, 20·57 per 1000: "This fortunate result was probably in some degree owing to the precaution adopted during the summer months. As the barracks were greatly overcrowded, when the hot weather set in, about one-half of the force was placed under canvas, and kept there until the end of the season."

Not less striking has the saving of life been among the troops serving in the West Indies. In the windward and leeward command, the death-rate among the white troops in 1859 was 19·75 per 1000. In Jamaica it was only 14·2 per 1000 during that year, which was probably exceptional. The average of the last three or four years in that island has not exceeded 3 per cent. at most. Twenty or thirty years ago, it was four times as great. In the eight years 1840–48, the ratio of mortality in the troops throughout all the West India colonies was a little above 5 per cent. of the entire strength; it varied much in different years, from nearly 10 per cent. in 1841, to about 3 per cent. in 1847. From 1817 to 1836 it averaged 12 per cent.; in other words, the military were decimated every eight or nine months. In very sickly seasons a third, a half, nay, two-thirds of a garrison have been swept off by fever and dysentery in the course of a year. A fourth part of a body of recruits have been known to perish within one month of their landing.

The black troops serving in the West Indies have, as might be expected, never suffered so much as their white brethren. The average annual mortality used to be about 40 per 1000. In 1859 the ratio was nearly 31 in Jamaica, but only 16·66 in the other West India islands. Why there should be generally more miasmatic and tubercular disease in Jamaica than elsewhere, remains to be ascertained. We read of "7 cases and 4 deaths from typhoïd fever at Up-Parts Camp. The type of disease and its fatal character appear to have been, to a great extent, due to the defective construction and offensive state of the privies."*

The East Indies have proved quite as destructive of the life of the soldier as the West Indies—"the grave of the military." From 1840 to 1848, the death-rate among H.M.'s regiments averaged 68 per 1000 of the strength; and this, too, was independently of the deaths among the sick on the voyage home, and after their arrival in England. In the East India Company's European troops the ratio averaged, from 1835 to 1844, 51 per 1000. As recently as from 1846 to 1854, the loss by death in the Bengal European army has been shown to have exceeded 6 per cent. of the strength in the twelve months. The native or Sepoy army has been comparatively healthy; the average death-rate of the whole has been about 17 or 18 per 1000. In the Bombay army it has not been above 1 per cent. of the force, while among the European troops in that Presidency, it was five times as high. Colonel Sykes has shown that intemperance has had much to do with this marked difference.

The climate of China (Hong-kong) continued to be, in 1859, most pernicious to the health and life of our European troops. The death-rate for that year was nearly 60 per 1000 of the strength. Nearly half of the mortality was due to alvine fluxes. Fevers, paroxysmal and continued, caused more than four times as many admissions into hospitals as dysentery, diarrhœa, and cholera; but the deaths resulting from them were only half as numerous. The native troops were very sickly; the deaths among them were in the ratio of about 54 in the thousand men. It is obvious from these facts, in connexion with the disastrous losses of life in former years at Hong-kong, that some thorough change in respect of the military stationed in this colony—their barracking, diet, clothing, and general duties—is urgently required, as alluded to in the Sanitary Section of the Report at page 178.

The results of the campaign in the north of China in the following year show, as already noticed, how much may be effected to keep down sickness and death among troops engaged in active service. The deaths from all causes in that expedition did not exceed the annual ratio of 45·3 per 1000 among the European troops, and 29·4 among the native troops; or, excluding the deaths from the casualties of war, 38·4 among the former, and 27·9 among the latter. In the Crimea, after the entire operations of war were over, the death-rate fell from 44 per 1000 in November, 1855, to 11 per 1000 in April, 1856; and when the troops left in the following month, it scarcely exceeded 8 per 1000, or a trifle more than that among the males of soldiers' ages in healthy districts in England. Contrast these rates with the following statement by Mr. Edmonds, *loco cit.*:

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* The horrible condition of the latrines ten years ago at this station, and also in the barracks at Kingston and Spanish Town, is described in the official report by Dr. Milroy on the cholera in Jamaica in 1850–51, and which contains an account of the sanitary condition of the military and other public institutions in the island.

> "During the Spanish war ending in 1814, the proportion of the English and French armies in hospital seldom amounted to less than 24 per cent., and yielded not less than 12 per cent. of annual deaths, there having been one death to every two years of sickness. This amount of sickness and death was independent of sickness and death from wounds in battle. In the Spanish, as in other European wars, the proportion of deaths from battle does not amount to more than a third part of the number of deaths from diseases which have for their origin excessive fatigue, insufficient food, overcrowded lodging, and excessive exposure to the inclemencies of the weather."

Let us now compare the mortality in the navy with that of the army. . In 1856, the deaths from all causes in the entire naval force of the country were at the rate of 15·5 in every 1000 men; or, from disease alone, exclusive of wounds and injuries, at the rate of 12·1 per 1000. The rates varied from 10·4 from all causes, and 8·4 from disease alone, on the Home Station; to 22·6, and 26·3 on the North American and West Indian Station; and to 26 9 and 34· on the East India and China Station.

In 1857 there was a considerably greater loss of life among our seamen, both from disease and from external violence and drowning. The deaths from the former cause were in the ratio of 14·7, and from the latter cause in the ratio of 4·7—or from both causes, of 19·4—to the 1000 of mean strength. And this high ratio, too, was exclusive of the deaths from shipwreck, which amounted to 125; so that the total death-rate that year was not less than 22·2 per 1000 of the entire force. As in 1856, the East India and China Station proved the most deadly of all to our seamen; the death-rate in the fleet there from disease alone was above 34, and from all causes above 46 per 1000. On the North American and West India Station, the corresponding ratios were 21·7 and 25·. In the Home fleet they were 8·2 and 10·7, which are nearly the same as in the preceding year, and are considerably above the general rate among civilians of the same ages in the healthy districts of England.

The chief point to be borne in mind in reference to the total death-rate in the navy, when compared with the rate either in the army in time of peace or in civil life, is the large proportion of deaths in the navy from accidental injuries, and especially from drowning. "The number of deaths from these causes," says Dr. Bryson, "was about three times more numerous than they appear to have been for many consecutive years in the army, and probably forty times more numerous than they are in civil life. . . . On an average, in every five deaths that occurred in the naval service in 1856, one was of a violent nature." The casualties of war cause but a small proportion of the total deaths from violence and accident. Of 172 such deaths in 1856, only 9 occurred in action; and of the 196 in 1857, 38 only were from wounds thus received. Twice these numbers were due, more or less directly, to the effects of intoxication! Making every allowance for the larger proportion of violent deaths among seamen than among the military or civil population on shore, the rate of mortality in the navy must be regarded as high—capable, it may be hoped, as in the army, of material reduction by increased attention to sanitary and hygienic appliances.

But the losses by death are not the only permanent losses to the two public services. There is a constant yearly reduction of the force of our soldiers and sailors by invaliding—in other words, by discharging men who from disease or injury are hopelessly disabled and unfit any longer for duty. No inconsiderable portion of these men die not long after their discharge, and their deaths go to swell the mortality in our civil population, and proportionally to relieve the death-rates in the army and navy.

Among the troops in the United Kingdom, the number of men so discharged seems to have averaged in the course of the year about 15 per 1000. In the Foot-Guards it has been somewhat greater. During 1859 the proportion was higher than it had been in previous years. In the Mediterranean garrisons, the yearly average of the invalided in the ten years 1836–47 was between 13 and 14 per thousand; in 1859 it was only 8·14. In the garrison at Bermuda the proportion had fallen that year to nearly one-third; and among the troops serving in Canada and Nova Scotia to one-half of the former rate, which was about 15 per 1000 of mean strength. In the West Indies the reduction was still greater. From 1817 to 1836, besides the enormously high death-rate among our soldiers, the numbers invalided every year averaged 2 per cent. of the strength; in 1859 the proportion was less than a fourth of this. Among the troops serving in the East Indies, the rate of invaliding seems to have been in former years even higher than in the West Indies, averaging about 27 per 1000 among Europeans, and 22 per 1000 among the Sepoys. But accurate information on this head is wanting, and we must wait for another year before we learn the proportion of men discharged from the Indian army, in consequence of broken constitutions, during 1859.

From what has been stated it is obvious that, in estimating the permanent losses among our troops, whether at home or abroad, a large addition—say 15 per 1000 men—must be made to the mere death-rates as recorded, if we would judge aright of the full extent of the yearly reduction of our military strength by disease. Does the navy exhibit an equal amount of impairment of its force by invaliding as the army? The following figures attest the very grave, but too little known fact as to the large losses that our fleets are continually experiencing, and chiefly from disease. In 1856 no fewer than 998 men were invalided, so that the total loss that year by death and permanent disablement (often soon terminating fatally) amounted to 1799, or about 35 in every 1000 of the entire force. In 1857 the proportion was much higher. Out of the total force of 42,470 souls, 1460 were discharged the service; and of this number five-sixths were on account of disease, and one-sixth only on account of wounds or other injuries. The total loss by death (deaths from shipwreck not included) and invaliding amounted to 2279, which gives the ratio of 53·8 per thousand of strength, or about a nineteenth part of the whole navy employed.

We shall now briefly inquire as to the nature of the diseases, &c., which occasion the chief amount of the losses, temporary and permanent, in our armies and fleets.

A very large proportion of the sick entries in both services is due to the following three groups—venereal affections; phlegmon and abscess, ulcers and cutaneous diseases; and, lastly, external injuries, such as contusions, slight wounds, &c. Among the troops at home, considerably more than one-half of all the hospital admissions have usually arisen from this threefold source; and of the three groups, the first has been by far the most prolific of temporary disablement, occasioning nearly as large an amount of loss of service in the course of the twelvemonth as the other two groups together. By the recent report, it appears that the mischief of late years has been on the increase. In 1859, of every 1000 men serving in the United Kingdom, no fewer than 422 passed through hospital from some form of venereal disease; and the average duration of each case was twenty-three days, so that the inefficiency from this cause alone was equal to the loss of the services, for the whole year, of nearly three entire regiments out of a force estimated at about 90,000.

On most stations abroad, the proportion of the sickness caused by the three groups to the total sickness rate has been considerably smaller than among the troops at home; still it has been large everywhere. In Malta and Gibraltar it has been about one-third of the whole; in Canada and other North American provinces the proportion has been higher, or between one-half and a third part of the whole sickness. Venereal diseases were much less frequent, relatively as well as absolutely, on these stations than in Great Britain; while the various affections, usually of a slight character, included in the second group have been considerably more numerous. It would appear from some recent statements that the prevalence of syphilis, &c., in our army in India has been of late years greatly on the increase, so much so that it has often occasioned no small inconvenience to the service, from the large numbers of men off duty in consequence.

The experience of the navy is not very dissimilar to that of the army as respects the proportion of the sickness caused by the three groups, enumerated above, to the total sickness. Fully one-half of all the sick entries in the entire service are so occasioned. Thus, in 1856, out of rather more than sixty-one thousand sick entries from all causes in five of the fleets (the Home, the Mediterranean, the North American, the East India and China, and the irregular force), with an aggregate force of 34,780 on board, upwards of thirty-two thousand entries were due to the three groups; and in the following proportions, 15,050 to abscesses, ulcers, &c., 10,433 to wounds (mostly slight) and other injuries, and 7180 to venereal affections. As might be expected, the last-named diseases were much more numerous in the home and in the irregular fleets than in any of the fleets engaged abroad, although even on some of the foreign stations the loss of service occasioned by them was far from inconsiderable. The serious damage to the efficiency of our naval force inflicted by this class of maladies seems to be on the increase, and is such that it must one of these days force itself upon public attention. Alluding to the far greater prevalence of it in the home fleet in 1856 than in former years, Dr. Bryson justly remarks:

> "That a disease so destructive of health and happiness, which by an acquired constitutional taint may be transmitted to generations yet unborn, should be allowed to go on increasing in our large seaports to an extent unknown in any other part of the world, is greatly to be deplored; but so long as the municipal authorities of those towns where it is most rife refuse to co-operate with the government in establishing hospitals for the cure of the degraded creatures along their pavements, it will be in vain to hope for any abatement of the evil."

Large as is the proportion of the sickness-rate occasioned by the three groups mentioned above, they cause but a small fractional part of the mortality in the public services. In the army, not above one in every forty of the deaths is due to them; it is from the great amount of temporary disablement, not of permanent loss, occasioned by them that their consideration is chiefly important. In the navy, the mortality arising from the two first groups is next to nothing; that from the third group or accidental injuries (exclusive of drowning, suicide, and loss in action with an enemy), is much more serious. In 1856, out of the total mortality of 801 from all causes, 61 deaths were caused by injuries; and in 1857, out of the total mortality of 819, 65 were due to the same.

Having accounted for one-half, roughly speaking, of all the hospital admissions among our troops and fleets, our next point is to ascertain the principal causes of the other moiety; and as this second moiety includes all the acute and dangerous diseases which occasion most of the mortality and of the invaliding in both services, the inquiry would demand, to have anything like justice done to it, a much greater space than we can of course devote to its elucidation. We shall therefore confine our present notice to three only of the principal groups or classes of disease so included—viz., fevers, exclusive of the exanthemata; alvine fluxes or diarrhœa, dysentery, and cholera; and diseases of the air-passages, among which, notwithstanding well-grounded pathological objections, pulmonary consumption is included.

Fevers, continued or paroxysmal, have at all times and in almost all places, been a fruitful cause of sickness and death in our army. Among the troops at home, they have generally occasioned about a fifteenth part of all the admissions into hospital, and between a sixth and a seventh part of all the deaths. The death-rate from fever among the Cavalry, the healthiest portion of the home force, has usually exceeded that among the population of army ages in our large towns. The type has been very generally the "continued;" periodic fever occurring only among regiments recently returned from abroad. Fevers have prevailed more among the troops at home between January and June, than during the rest of the year. The deaths to cases have averaged, on the whole, about one to thirty-two. Occasionally, and in certain barracks, the fevers have been most deadly. Between 1837 and 1846, several epidemic outbreaks took place among the troops quartered in the Tower, and with such malignancy that one in every three or four of the cases proved fatal. Since the barracks were improved, and the adjoining foul ditches were removed, the tendency to the disease has greatly diminished, and the character of the attacks been mitigated. The barracks near Portman-square, too, used to be noted as a fever *habitat;* doubtless they are better now, although from their very position they never can be healthy quarters.

In our Mediterranean colonies, fevers have occasioned three times as much sickness as in the United Kingdom. About a fifth of all the hospital admissions, and between a fourth and a fifth of all the deaths, among the troops stationed there have been due to this cause. Two-thirds of the cases are of the continued type, the rest being either intermittent or remittent. The chief season of prevalence is during the hot months. Beginning to multiply in May, they increase in June and July, about the end of which month, or in the early part of August, they usually reach their acme; after this period they begin to decline till the end of October, by which time they have generally much subsided. The regiments stationed in the Ionian Islands have almost always suffered more from fever than either in Malta or Gibraltar. At times, the disease has been of an extremely bad type and very fatal, and then some local cause of insalubrity, within or outside—or, more generally, both in and out—the barracks has been discovered. In 1859, a bad typhoïd fever prevailed to a great extent among the troops in Corfu. Upwards of 400 men, out of a force whose mean strength varied in the course of the year from 2100 to 3060, were attacked, and 21 died. The causes assigned were "the great heat of the summer, the overcrowding of the barrack rooms, and the very unsatisfactory sanitary condition of the town," to which may be added, "want of due attention to cleanliness in and around one of the barracks." A somewhat similar occurrence took place that year among some of the troops in Malta, and was traced to like causes in the site and condition of the barracks. Dr. John Davy drew attention five-and-twenty years ago, if we mistake not, to the febrific state of the very quarters which have recently been again the seat of so much disease.

In the East Indies, fevers have always been among the chief causes of sickness and mortality among our European troops serving there; a third of the sickness, and between a third and a fourth of all the deaths, may be ascribed to them. In the West Indies the proportion formerly was still higher; between a third and a half of all the hospital admissions, and one-half of all the deaths, used to be caused by fevers. It is most pleasing to observe, by the recent report, that their frequency and fatality have been very remarkably diminished of late years. Want of space prevents our giving any particulars; and we must proceed at once to compare briefly the amount of fever and its results in the sister service. In the navy, during 1856, between a thirteenth and a fourteenth part of all the sickness (exclusive of accidents) on board our ships, and nearly a third of all the deaths arising from disease, were due to this cause. One case was fatal in every twenty-two or twenty-three attacks. By far the most sickly station in respect of fever is the West Indies. The east coast of Brazil has also of late years been very unhealthy. Much fever has likewise prevailed in the

East India and China squadron, and in the Mediterranean fleet. Nor have the home fleet and the irregular force escaped considerable losses, permanent as well as temporary, from this class of disease. A remarkable feature, and one which specially demands attention in naval hygiene, is the frequent limitation of the fever to one or to a very few only of the vessels in a squadron; the rest remaining comparatively or wholly exempt. Thus, in 1856, two frigates, the *Eurotas* and the *Dauntless*, contributed nearly a third of all the fever cases in a fleet of seventy-three vessels in all. The surgeon of the *Eurotas* attributed the sickness to "the extreme lowness and closeness of the deck on which the men were berthed;" and as to the *Dauntless*, it may be stated that not only fever, but bowel complaints, were exceedingly rife among her crew. This ship, moreover, suffered terribly from yellow fever in 1852, and was again very sickly in the Baltic and Black Sea in 1854 and 1855. She has been altogether a second *Eclair*. The ship of the line, *Hannibal*, was also a most unhealthy vessel. Between a third and a fourth of all the fever cases in the Mediterranean fleet, in 1856, occurred on board of her; and she had been extremely sickly the year before. Upwards of two hundred cases of a low adynamic fever occurred among a crew of less than seven hundred. The admiral's ship, too, the *Royal Albert*, was much infested with low fever and diarrhœa; and this had been the case in 1855 as well. The rest of the large ships of that fleet suffered but little comparatively.

This limitation of the continued fever of our own coasts and of the Mediterranean to particular ships holds equally true of the malignant fever of the West Indies and Mexican Gulf, known as yellow fever. One, two, or three vessels, out of a squadron of twenty or thirty, will suffer enormously; while others, exposed to similar or nearly similar external influences, escape either entirely, or with only a few sporadic cases and the disease showing no tendency to spread on board. The experience of both services, during the last ten or twelve years, has afforded much valuable information as to the local conditions which favour the development and diffusion of this formidable pestilence. No evidence can be more conclusive as to the potent and most pernicious influence in this respect of badly situated and impure overcrowded barracks and stations than the histories of the visitations among the troops at Barbadoes in 1848; in Bermuda in 1853; at Newcastle, in Jamaica, in 1856; and in Trinidad in 1858 and 1859, noticed in the new army report, and of which the following are the chief memoranda. The ordinary endemic remittent had been prevalent in the island during the third quarter of 1858. In September, two undoubted cases of yellow fever occurred in St. James's barracks. The men were camped out for two months, and the barracks were meanwhile thoroughly cleaned and whitewashed. No sooner were they re-occupied than fresh cases of fever occurred. Again were the troops put under canvas, and with good effect; "although several cases occurred subsequently, they could all be clearly traced to the barracks. The disease was clearly of local origin." It did not spread over the island, but was confined entirely to the foul-drained and unhealthilysituated barracks. Such has been, almost invariably, the nature of the evidence in the army medical reports respecting this fever in other West India islands.

Of recent years, many of our ships of war on that station have suffered most disastrously—more terribly, indeed, than has perhaps been known before. As they have been almost all steamers, it may be a question whether the extra heat, and other unfavourable conditions resulting from the machinery, may not have something to do with this increased and increasing prevalence of destructive fever among our sailors in that part of the world. Want of space precludes the recital of details. Suffice it to say, that several ships have lost a fifth, a fourth, and even a third of their crews within less than a couple of months. That the excessive overcrowding of the men in the between-decks, far greater than anything known in the worst military barracks, has had no small share in aggravating the virulence of the febrific poison will be questioned by no one acquainted with the history of the disease. Its prompt abatement or entire cessation on removing the crew from on board an infected ship, as in the analogous instance of camping out an infected regiment, proves this beyond doubt. That, moreover, the foul state of the holds is occasionally the cause of sickness, or, at least, of the marked aggravation of sickness, is unmistakeably shown in the statistical reports for 1856 and 1857. The pernicious influence of this agency is, in our opinion, much underrated by Dr. Bryson; while that of personal contagion or of the direct communication of disease from the sick to the healthy, as the chief element of danger on board ship, is very unduly magnified. But whatever opinion may be taken on this point, all must agree that, for the sake of humanity and the interest of the public, it is high time that the liability of our ships of war to such frightful losses of life from fever, as have recently occurred, should be thoroughly investigated by the Government. The condition, too, of all the barracks and military stations in the West Indies stands much in need of a like examination, as affording the only reliable means of safety for the future welfare of our troops.

Alvine fluxes.—There is no tribe of diseases which has at all times produced a greater amount of distress and suffering, and often too of death, among armies (especially in the field) and fleets, than diarrhœa and dysentery; and as there is perhaps none which on the whole better serves to test by its prevalence and severity, or otherwise, the general hygienic organization of the force, their consideration deserves most serious attention. We can only very briefly notice a few points of their military and naval history; but these will suffice to indicate the importance of the subject. Their frequency and fatality are much influenced by season and climate. Heat, and still more the sudden vicissitudes of temperature, are invariably predisponent and aggravating causes. Malarious impurities of all sorts, and humidity, act in the same way; and the nature and quality of the ingesta exert a powerful influence upon their development. Among the troops at home they occasion much sickness, but not many deaths. In the Mediterranean, they are three times as frequent and as fatal. Their chief prevalence is between May and November; the maximum of frequency seems to be in August and September, and the minimum in February and March. Taking all the Mediterranean stations together, intestinal disorders have caused about the same amount of sickness among the garrisons as the class of fevers; but the resulting mortality has been a third less. In Canada, &c., they are much more frequent than in the United Kingdom, but less so than in the Mediterranean. In Bermuda, however, the sickness-rate, as well as the death-rate, from this cause exceeds the rates in the Mediterranean. But it is when we reach the tropics that we find all bowel disorders, more especially dysentery, attain their full disabling and destructive power. In the West Indies, between a fourth and a fifth part of all the sickness, and between a third and a fourth part of all the deaths, among the troops, were formerly (between 1817 and 1836) caused by them. They were only less frequent and fatal than fevers. More than a third of every regiment passed through hospital in the course of the year from some form of bowel disease; and one in every nineteen or twenty cases proved fatal. By the report for 1859, the admission-rate from this cause had fallen to less than a twelfth part of the average twenty years ago; and the type or character of the disease was also, as might be expected, much less formidable. What has taken place in the West Indies will doubtless find its parallel in the East Indies, when once the necessary precautionary and prophylactic measures as to the sites of barracks, &c., have been fully carried into effect throughout that great peninsula.

In the navy, not less than in the army, bowel disorders have always been, and continue to be, a prolific cause of disablement and loss. An eighth part of all the sickness throughout the entire service is caused by them—by diarrhœa, dysentery, and cholera. The proportion varies much on different stations, being lowest on the home station, and highest by far on the East India and China station, where the extremely high rates of sickness and death are principally due to this source. Of sixty-four deaths from dysentery throughout the whole navy in 1856, no fewer than forty-eight occurred on this station, most of them on board ships in the Canton river. After the East India fleet, the Mediterranean fleet is that which stands highest on the list under this head; bowel disorders being extremely common on that station, and proving not unfrequently fatal.

It would take us far beyond the limits of this article to notice, however briefly, the history of Asiatic cholera on board our ships of war and among our troops. The subject deserves an article to itself. Suffice it to mention a fact or two bearing on the present inquiry, and indicating the relation of the disease to hygienic conditions. In 1849, the proportion of the fatal attacks in the barracks, in and around London, exceeded by a third that in the metropolitan prisons. At Port Royal, Jamaica, in 1850, a sixth of the garrison perished within a month; at Barbadoes, in 1854, the death-rate was still higher. On both occasions the officers escaped—always a significant fact. Nearly the same thing was observed in the terrible outbreak at Kurrachee, in 1846, when more than 1400 men "were swept off in less than three weeks." That overcrowding in badly-situated and ill-ventilated barracks, immediately after the fatigues of a lengthened march in sickly weather, had much to do with the deadly virulence of that outbreak is beyond all doubt.

Our navy furnishes similar records. The Black Sea fleet in 1854 lost more than 400 men out of a force of about 13,000, in little more than a fortnight. Three ships furnished 252 deaths. A sixth part of their aggregate crews was smitten, and a tenth part perished. The admiral's ship alone lost, within ten days, no fewer than 140 out of a crew of less than a thousand men—a loss greater than the entire loss from the guns of the enemy in the whole fleet throughout the campaign! The chief cause of this frightful mortality was the dense crowding in the impure, stifling atmosphere of the lower decks, after the disease had made its appearance on board. Very few of the officers were attacked. It would be easy to multiply similar, only less formidable instances of the very marked influence of local and personal conditions on the malignity of cholera, from the reports of the navy. Altogether, both services afford some of the most striking illustrations of this great hygienic truth to be found anywhere. As long as such disastrous losses as those just cited continue to occur in our fleets and armies—and the recent outbreak in N. W. India adds another sad example to the list*—our knowledge or practice of preventive medicine must be regarded as imperfect indeed.

But we must hurry on to another class of diseases, which play so conspicuous a part in the sickness and death returns among our soldiers and sailors—viz., that of the

Diseases of the lungs, &c., including pulmonary consumption.—The following facts and figures will show the importance of this branch of the subject. Among the troops at home, these maladies occasion more admissions into hospital than all fevers and diseases of the bowels together; and more than three times as many deaths. Two-thirds of all the mortality in the army in the United Kingdom are due to them; and of these two-thirds, four-fifths are due to consumption. In the Mediterranean garrisons, the proportion of chest diseases to all other causes of sickness has been nearly as high as in Great Britain—viz.: a sixth or a seventh of the whole. They have not been quite so fatal; still, the mortality from them alone has constituted between a half and a third of all the deaths. In the North American command, their frequency and fatality have been nearly the same as at home. Curious to relate, the death-rate from diseases of the respiratory organs has been lowest in the cold, foggy climate of Newfoundland, and highest in the warm and sunny Bermudas. That hot and tropical regions are far from having been favourable in respect of these disorders, is shown by the fact that the ratio of deaths thus occasioned to strength was as high among the troops in the West Indies from 1817 to 1836, as among the troops in the United Kingdom.

The black troops suffered even more than the white troops, and chiefly from consumption. It would seem that lung diseases have, on the whole, been much less prevalent and fatal to our troops in the East Indies than in the West Indies. Consumption is comparatively rare in Hindostan.

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* From the latest accounts it appears that upwards of five hundred European soldiers perished in less than six weeks. The chief ravages were at Lahore. The 51st Regiment lost a fifth of its strength.

In the navy, respiratory diseases are the most prolific source of sickness and death. A sixth part of all the sickness throughout the service, and more than a fifth of the deaths from all causes, is occasioned by them. In 1857, the ratio was higher in both respects than in 1856. The rates vary a good deal, as might be expected, on different stations; but everywhere they are absolutely, as well as relatively, very high. Mere difference of climate seems to have much less to do with their production than is generally believed. That much may be done by due care on board ship to diminish the amount of chest complaints among our sailors cannot be doubted. Some ships' companies suffer far more than others equally exposed at the very same time, and engaged in similar duties. This fact alone shows how much it is in the power of commanding officers to control or prevent the occurrence of many serious, and often in the end fatal, attacks. Comparing the high sickness-rate in the navy with that in the police force, Dr. Bryson remarks:

> "Athough the police may be exposed to the inclemencies of the weather while on night duty, so also are the seamen of the navy; the former are well protected by waterproof clothing, and when relieved from their duty retire to a warm, comfortable barrack-room, or to their homes, where they can obtain warm food and drink, and dry their clothes; whereas the latter, in consequence of the active nature of their duties, must for the most part remain exposed to the pelting of the rain and spray in their ordinary clothing, and *when relieved, retire to their hammocks or berths between decks, where it is seldom they can either dry their clothes, or obtain (especially during the night time) any kind of warm food or refreshment.*"

Can nothing be done to rectify such a state of things? But a more fruitful cause of evil than the neglect here indicated is, doubtless, the abrupt change of temperature during the night-watches, when the men have to turn out from the hot steaming atmosphere in which they have been sleeping to go on deck, it may be to go aloft at once to take in sail, &c., and this in all weathers and climates. The very great amount of phthisis and other organic diseases of the lungs in the navy, as well as in the army, is certainly one of the most striking features of the statistical returns of both services in relation to mortality and invaliding. We must confine our present remarks to the navy.

In 1856, out of a total mortality of 629 from disease, 175 deaths were due to pulmonic affections, chiefly phthisis; and of the 918 men invalided from disease that year, 103 were discharged on account of consumption, and 49 from other pulmonic disorders. In 1857, of the total deaths (623) from disease in the service, 129 were caused by consumption, and 27 by pneumonia or bronchitis; and of the 1256 invalided from the results of disease in the course of the year, 250 were on account of pulmonary mischief—viz.: 188 from phthisis, 47 from pneumonia, &c., and 15 from asthma.

The great prevalence of tubercular disease among our sailors, not on one station only but on almost every station where our fleets are sent, and therefore irrespective in a great measure of climatic differences, is a subject which surely calls for scrutiny. The men are, of course, free from all signs of the disease when enrolled; and we know that they are well fed and clothed, and have all the advantages of immediate medical care when ill. Sea air and ship exercise are moreover prophylactics rather than predisponents of the cachexy on which it is grafted.

What, then, can be the main cause or causes of so much consumption, from year to year, among the crews of our ships of war? The Royal Commission on the health of the army adopted the conclusion, to which previous observations in civil life had brought the best sanitary inquirers, that the breathing of an impure atmosphere charged with animal effluvia during the hours of sleep has much, very much, to do with the production of that depressed and deteriorated state of health which leads to the development of *tuberculosis*. When once the morbid tendency is generated, every fresh chill and catarrh serves to stimulate and hurry on the latent mischief; while, at the same time, every strong effort of muscular exertion increases the liability to an attack of hæmoptysis. It is probably in the direction here indicated that inquiry will discover the true solution of a problem so vitally affecting the welfare and efficiency of our navy.

Everything indicates that the master sanitary defect in our ships of war is the insufficient accommodation for the men, and the consequent excessive crowding of them at night in the lower and worst-aired decks, while there is always more or less space, purer and better ventilated, that is not occupied by the crew at all. The continuance of this faulty arrangement is but the relic of an old prejudice that the upper gun decks must be kept ready at all times for action with an enemy at a moment's notice. But ships do not encounter ships even in the dark without some warning, and there is always plenty of time to clear away hammocks. Executive officers now admit that there is really no good reason for the practice, and that it would, moreover, be beneficial to distribute the men more apart; of course, all the medical officers of the navy would heartily approve of such a change. Besides the diseases already enumerated, there are other prevalent maladies among seamen, occasioning much loss of service every year, which, if not caused, are at least greatly aggravated by the same agencies as we have indicated. Rheumatism with its sequelæ, and ulcers, may be mentioned in this family.

The other great hygienic evil in the navy is intemperance—a prolific cause, unhappily, of much sickness and mortality from year to year. Diseases of the nervous system—viz., apoplexy, paralysis, delirium tremens, and insanity, occupy a conspicuous place in the registers of deaths and invaliding in both services, but especially in the navy. In a large proportion of the cases, the abuse of spirituous liquors is the cause of the mischief. Many, too, of the violent deaths from drowning, suicide, &c., are due to the same cause. The substitution of suitable and pleasant drinks for the everlasting grog is a problem of immense moment.

The reader may thus have seen what a rich and interesting field of instruction in medical statistics will be opened up by the annual reports of our navy and army. Their value will go on increasing, as the opportunities of larger comparison are multiplied. Would that similar opportunities were afforded in civil life! With this wish, we end as we began, by strongly recommending to the profession the consideration of the best means to effect this highly desirable object.

REVIEW VII.

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2. *Die Mechanik der Blut-circulation in Inneren des Schädels, &c.* Von Dr. OTTO MÜLLER. ('Zeitschrift für Psychiatrie,' 1860. pp. 70.)
The Mechanism of the Circulation of the Blood within the Cranium. By Dr. OTTO MÜLLER.
3. *De la Circulation du Sang dans les Membres et dans la Tête chez l'Homme.* Par Dr. J. P. SUCQUET.—*Paris*, 1860. pp. 55.
On the Circulation of the Blood in the Limbs and the Head of Man. By Dr. J. P. SUCQUET.
4. *Des Effets produits sur l'Encéphale par l'Oblitération des Vaisseaux Artériels qui s'y distribuent.* Par Dr. EHRMANN.—*Paris*, 1860. pp. 60.
Effects produced upon the Brain by the Obliteration of the Arteries distributed thereto. By Dr. EHRMANN.
5. *The Physiology of Sleep.* By ARTHUR E. DURHAM. ('Guy's Hospital Reports,' vol. vi. 1860, pp. 149–173).

CEREBRAL pathology has always ranked among the most difficult subjects submitted to the medical inquirer, and at the present day must be admitted to be more imperfectly understood than any other department of pathology. Nor is the explanation of this circumstance difficult when we reflect on the structure of the brain, on its position—withdrawn as it is from every means of direct observation during life, and on the peculiar nature of its functions, of the mode of relation of which to the nerve-matter we have no distinct conception. The mechanism of the secretion of the bile from the liver, and of the urine from the kidneys, can in all its cruder parts be displayed by microscopical examination, although why bile should flow from the secreting tissue of the liver, and urine from that of the kidneys, we cannot explain; but in the case of the brain, we have no clue to interpret how it is that sensation, motion, and thought are developed from its tissue. It is the medium, we say, of an immaterial principle, which can only act by and through it; but how its functions, as a medium, are performed is an enigma which must apparently ever escape human investigation.

Nevertheless, in the performance of the cerebral functions there are certain factors revealed to us by experiment and observation as essential—such as a proper supply of arterial blood, a due removal of venous blood, and an appropriate quality of blood. With these must concur an integrity of tissue, and, in connexion with the higher faculties of the mind, a sufficient development of the whole brain, and a due relative development of its several segments. These conditions of normal function are in part discoverable during life, and it is of the utmost importance to the acquisition of knowledge respecting both the physiology and pathology of the brain, to inquire anatomically how they are provided for, and, in general, to determine under what circumstances they may be deranged; and then, with what symptoms such derangements are associated. We must therefore hail with satisfaction any contributions which advance our knowledge of the anatomy and physiology of the brain; and having at the present time before us the publications whose titles stand at the head of this article, we shall endeavour to cull from them whatever is novel or calculated to improve our acquaintance with the structure and functions of the cerebral circulation, particularly in the venous system, both in health and disease.

Taking the first work on our list, the monograph of Luschka, we find several interesting anatomical points in it, having, some, the recommendation of novelty, others, the merit of accurate investigation: these subjects, however, are not, as the title of the work would lead one to suppose, restricted to the history of the venous plexuses of the brain; but also the cavities of the brain, the ventricles and sub-arachnoïd spaces, their lining membranes, and the structure of the spinal cord, receive an equal amount of attention. And it seems well worth a momentary digression from the subject of the cerebral circulation, to put on record some of the more remarkable results arrived at by Luschka respecting these several structures.

At the outset of his chapter on the fourth ventricle, or as he calls it, the ventricle of the cerebellum, Luschka adduces some original researches on the much-mooted question of the existence of a central canal in the spinal cord in the human subject, and concludes from them that such a canal exists, and may be traced even in the cords of adults. This central unoccupied cylinder, in the young extends the entire length of the cord, and is separated from the nerve-tissue surrounding it by a layer of connective tissue, overlaid on its free surface with epithelium. But in the adult, the continuity of the canal is more or less interrupted at parts, by abnormal growth of the connective-tissue or epithelium, or by the development of the well-known corpora amylacea, which Virchow, by the way, has described as existing in a sort of central axis of ependyma, occupying the site of the spinal canal, and representing it in mature life.

The spinal canal communicates above with the fourth ventricle, which, in its turn, opens into the sub-arachnoïd space through an aperture at the pointed end of the calamus scriptorius. This aperture was described by Majendie; but its existence has been denied by Todd,* and more recently by Virchow and Kölliker. To decide the point, Luschka has taken great pains, and proceeds by anatomical investigation, by experiment, and by an appeal to recorded surgical cases where the cerebro-spinal fluid has escaped from the cranium during life, to demonstrate its existence; and, as a corollary to it, the fact of the intercommunication of the several sub-arachnoïd spaces or confluences. Indeed, the case cited by Dr. Burrows, of spina bifida, might of itself be considered well-nigh conclusive of the fact of the direct communication of the sub-arachnoïd spaces together and with the spinal sub-arachnoïd cavity; for in that instance, pressure on the fluid tumour of the spine was seen to upheave the brain from its base. On the other hand, Luschka confirms the now prevailing opinion that the canal described by Bichat as extending into the third ventricle has no existence; and that what that distinguished anatomist conceived to be such is nothing more than one of those sheath-like prolongations of the arachnoïd given off around every vessel or nerve passing in or out of the brain, and in this particular instance surrounding the venæ Galeni.

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* Anatomy of the Brain, p. 45. 1845.

The arachnoïd membrane, both of the brain and spinal cord, has been very attentively examined by Luschka, who asserts, that not only is it covered with epithelium on that surface which is turned towards its opposite layer in the arachnoïd cavity, but also on the one which faces the pia mater—not, indeed, where it is in actual contact with the latter, but where it bridges across the cerebral inequalities and constitutes sub-arachnoïd spaces. This he affirms of the arachnoïd both of the brain and spinal cord. With this fact is associated another, of much interest—viz., that there is a covering of epithelium on the pia mater,—alike on that covering the brain, and on that investing the spinal cord. We here content ourselves with stating the facts as asserted, leaving the reader to examine Luschka's demonstration of them in the work under notice (see pp. 132, 141).

Another disputed point in anatomy may be considered to be settled by the researches of Luschka—viz., the presence of cilia upon the epithelium lining the ventricles of the brain in adult age; for he has met with these organs in three cases where he had the opportunity of examining the brain very soon after death. But though cilia exist on the ventricular epithelium, they are not discoverable on that covering the choroïd plexuses within the ventricles, except in the case of the young of some animals; what may be the real physiological import of cilia in the cerebral cavities it is difficult to surmise, for by their distribution over such wide surfaces, and from the relation of the ventricles to their outlets, they can scarcely serve the function usually assigned to them of aiding the escape of a secretion. And if their presence were at all necessary to the functions of the ventricles, it would be equally difficult to account for the absence of cilia in those cavities in various animals, such as—according to Luschka—the horse and rabbit. But though the office of the cilia within the ventricles is obscure, there is no doubt that the epithelial cells, both of the ventricular epithelium and of the choroïd plexuses, are actively concerned in the secretion of a serous fluid. Luschka met with pellucid drops within the cells, and saw them escape as a transparent fluid; and without doubt these hyaline drops are the same as the oil globules described by

Purkinje, on the surface of the ventricles. Considering the vascular nature of the choroïd plexuses, we may regard them as probably very actively secreting organs; indeed, their villi-like processes are very fairly comparable with the Malpighian tufts of the kidneys. However, they are peculiar in their minute structure, for beneath the epithelial coat, and overlaying the bloodvessels, is a lamina of considerable thickness, of a homogeneous or very finely granular texture, which it must be admitted constitutes a mechanical impediment to that free exosmose of serous fluid from the vessels which we meet with in the case of the arterial plexuses of the kidneys. Moreover, this structureless layer appears to have the power of producing from itself villi-like processes, not entirely homogeneous in composition, but having a darker and more roughly granular centre, which augments in length with the growth of the villus. Luschka has well displayed this lamina and its processes in his figures, and states that in its chemical composition it somewhat differs from common connective tissue.

Before dismissing these anatomical memoranda, it is desirable to note the villi-like processes given off both from the visceral and parietal layers of the arachnoïd, for the knowledge of them throws light on various recorded morbid phenomena in the anatomy of the surface of the brain. These processes are met with along the contiguous borders of the hemispheres, on each side the falx, sometimes as far backwards as the vermiform process of the cerebellum. They are normal structures, for unlike the Pacchionian bodies, they occur at the earliest period of life, and have no connexion with the pia mater. On the parietal arachnoïd, they are chiefly restricted to the sides of the longitudinal sinuses; are very small in young children, but may attain so considerable a length in old persons as to extend between those formed on the visceral surface of the membrane and unite with them.

Let us now revert to the consideration of the cerebral circulation. This is distinguished, as is well known, by not a few remarkable peculiarities dependent on the structure of the brain, and on its position at the summit of the cerebro-spinal axis, enclosed within the hollow and well-nigh completely closed cranial cavity. For instance, the active function of the brain demands a very large supply of blood, but so delicate is its tissue that the permeation of it by a network of arteries and veins, in the manner common in other organs, would be incompatible with its integrity; hence it is that the arteries break up into minute branches before entering its substance, and that the venous blood is collected by small radicles within the nervous tissue and discharged into large peripheral venous sinuses, or into large veins, all so placed in the hollow interspaces between the cerebral convolutions as to obviate pressure upon them. With the same view of avoiding injury to the soft brain matter, the great arteries are much contorted on entering the cranium, so as thereby to diminish the impulse of the arterial current through them, whilst at the same time they very freely anastomose so as to favour an equable distribution of blood to every part, and to counteract as far as possible the ill effects of the arrest of arterial supply in any one of its normal sources.

The supply of blood to the brain is very large, and is estimated at one-fourth of the whole quantity in the body. It reaches the cranium by four capacious arteries, so derived from the several main trunks from the arch of the aorta as to preclude the possibility of its being cut off from all. And should it by disease or accident be interrupted in one or other channel, the remarkable arterial anastomosis, the so-called "circle of Willis," at the base of the brain, affords a most powerful safeguard against the otherwise rapidly destructive consequences of such an interruption, by the collateral circulation it at once ensures. How great is the security this anastomosis furnishes, is shown by the records of cases of disease, and of surgical operation as well as by experiment. For it is astonishing, especially when we take into account the evident demand made by the cerebral tissue for arterial blood for its healthy activity, how largely the supply may be arrested by ligature or stoppage of the arteries entering the cranium, and more particularly when the arrest is effected gradually. For striking examples of this fact we have only to turn to the excellent brochure of M. Ehrmann, who has collected notices from almost every available source, of cases of operation and of disease involving the stoppage of the circulation through the several arteries entering the cranium.

In the lower animals the tolerance with which the cutting off the arterial supply to their brains is borne would be scarcely credible had we not abundant experimental evidence of the fact. In dogs and rabbits both common carotids may be tied, and mostly with impunity, for symptoms of cerebral disturbance after the operation are more frequently absent. In the case of rabbits, not only may the two common carotids be ligatured, but in addition, one of the vertebral arteries, and yet the brain appear to be uninjured. But if in these animals or in dogs all four arteries going to the encephalon be tied, convulsions of an epileptiform character at once occur, and death quickly follows; nevertheless, under this aggravated assault upon the cerebral circulation, death is no necessary consequence, for if some of the ligatures be shortly removed, the cerebral functions speedily recover themselves.

In the human being the tolerance of arrest of the arterial current, though considerable, falls much short of that exhibited by these and other lower animals. Of the effects of such arrest on the latter, as far as their brain functions are concerned, it must be admitted that we have less evidence than of the consequences ensuing when man is the subject of operation. For in those animals we have to deal with a much less highly organized or developed brain, and with beings more incapable of conveying or of manifesting to the observer the actual effects of the experiments upon the encephalon. The partial abolition of cerebral function in them is of less moment to their entire organization than it is in man; during the operation they are well-nigh in the condition of anencephalous creatures, but this state is not incompatible with the performance of the functions essential to life, and in these lower animals as compared with the human subject, the cerebral organization occupies a much less important position in the economy. Moreover, there is another cause of the same class, why the deprivation of arterial blood is of less importance in the inferior animals, and it is one indicated by the results of experiments—viz., the greater tolerance of injury as we descend in the animal scale; for instance, the experiments on the arrest of arterial blood to the brain may be carried out to a greater degree in rabbits than in dogs; that is, in animals whose existence is of a more vegetative nature than in those of a higher grade. In the horse, the deligation of the carotids is borne with less impunity than in the dog; but here this circumstance is best explained by the fact of the vertebral arteries in the former being relatively of small size, and therefore less able to compensate for the diminished supply from the carotids.

To return to the results of observation on the human subject: the sudden tying of one common carotid is often fatal, but the stoppage of its arterial current, when gradual, as by disease, is mostly without prejudice to the brain. Indeed, a case is on record where both carotids had been rendered imperforate by ossification of their coats, and yet the cerebral functions had remained intact; and in the history of surgery there is a long array of cases in which both carotids have been tied, and provided that several days have elapsed between the operation being performed on the one and on the other artery, the majority of the patients have escaped not only death, but also any appreciable detriment to their brains. Ehrmann has collected a very large number of cases of ligature of the carotids, and arrives at the conclusion that, in only twenty-one per cent. is there a fatal termination. However, of the forty-nine cases of ligature of one carotid, which are more particularized by him than the rest, there were symptoms of cerebral disorder in forty, and death in twenty-eight.

The cerebral disturbances following ligature of the carotid are hemiplegia, with, or more frequently without, convulsions; and these symptoms may appear soon after the operation, or be delayed some days. There is another point respecting the injurious consequences of the ligature—viz., that they may disappear after a brief continuance; the hemiplegia declines and ceases, unless indeed it is associated with organic change of the cerebral tissue—a change which the autopsy of fatal cases shows, and which from *à priori* considerations might otherwise be predicated to be—i.e., softening, pretty constantly of the white or anæmic form, but sometimes inflammatory. The hemiplegia is almost invariably on the opposite side of the body to that on which the artery is tied; still this is not invariably the case, for Ehrmann quotes two instances in which the paralysis was on the same side. Other accidents after ligature of the carotids deserving mention, are temporary or complete blindness, loss of hearing, severe cephalalgia, and now and then loss of power over the speech, a consequence which some are disposed to consider due not to the cutting off of the arterial current from a part of the brain, but to the recurrent laryngeal or some other nerve supplying the larynx being included within the ligature.

The marvel that the arrest of so much arterial blood to the brain as is involved by the tying of one or both carotids, is not more fatal, or at least not more prejudicial to the human subject, is mainly explicable, as before hinted, by the existence of the free intercommunication of the several cerebral arteries in the "circle of Willis," and of the consequent facility with which the blood received from the channels yet open finds its way to all parts of the encephalic mass. But we venture to assert, that this admirable provision of our Creator would be even more effective than it seems to be in man, probably well nigh as much so as we find it in the more highly organized mammalia which have been the subjects of experiments, were it not that when one or other principal artery of the cranium is obstructed, there is almost always more or less wide-spread disease in other arteries of this region, and therefore the co-existence of a cause of cerebral disturbance over and above that which is furnished by the arrested flow of blood through one, or it may be two, of the ordinary channels. In proof of this we have only to study the history of the cases in which resort to ligature of the carotids has been sought as a mode of treatment. The nearly universal cause for such a severe remedy has been aneurysm; a lesion of the arterial coats, and one which pathology teaches us is not of a local character, but pervades other arteries in various degrees as a form of disordered nutrition. Consequently, it is correct to argue that, as a rule, where disease has invaded the coats of one or other great arterial trunk from the aorta, it has extended itself also, in a varying degree, to the ramifications of that trunk, and that as a necessary sequence those branches will be less fitted to serve their purpose in the processes of nutrition of the organ they supply. This consideration will often afford, in our opinion, a better explanation of the different termination of similar operations on the carotid arteries in the human subject, than does the varying size of the arteries in the "circle of Willis," as put forward by Ehrmann. Not that we would deny the importance of this latter explanation, for the relative facility offered by anatomical arrangements in different persons for distributing an adequate supply of blood to every part of the brain, when one or two modes of its access are withdrawn, must be of much moment, but we would insist on the necessity of coupling with this condition that of the varying state of the arteries in regard to their healthy state, as well those concerned in the formation of the "circle of Willis," as of those beyond it. Again, though Ehrmann has shown that in different individuals the relative size of the arteries in the "circle of Willis" varies, and this almost exclusively in reference to the "*communicating*" arteries of the anastomosis, yet he has failed to connect the diminished calibre with the occurrence of cerebral disorder in any of the detailed cases of stoppage of the cranial vessels.

This same fact, that where the arterial supply has been cut off in any human subject, it has been as a consequence of disease, and mostly also connected with disease in other arteries of the cranium, may, as before intimated, be employed in part explanation of the circumstance that such arrests in the cranial circulation are much more frequently attended by injurious effects than happens when any of the lower animals are experimented on, because such animals are often young, and may pretty confidently be presumed to have almost invariably healthy cerebral arteries.

Sufficient has been said to prove, that though the brain in man requires so large a share of blood, that though its tissue be so delicate and readily deranged, and though the arterial pressure at the base be presumed of importance to its healthy action, yet that this free and vigorous current may be interrupted in one or even two of its principal channels with no material and no lasting injury to its integrity or functional activity. And we have found the explanation of this immunity in the admirable arrangement of the channels of communication between the several arteries at the base of the brain. The importance of this confluence of the arteries is further exhibited by the history of cases where obstruction has occurred in the vessels on the other side of it, or, in other words, distal to it. We cannot here appeal to experiment to show this, but there is ample evidence for it in pathological anatomy, in instances of occlusion by coagula and fibrinous masses—cases, that is, of *embolia*.

When such local arrests of the circulation occur, the symptoms are those of apoplexy, and though consciousness may not always be lost, there is hemiplegia more or less complete; and if no collateral circulation can be sufficiently set up, the paralysis is permanent. Convulsions, however, do not enter into the category of symptoms. Patients do not, in these examples of isolated stoppages of the circulation, escape scot free, as they may do when a grand arterial trunk, such as the carotid, is tied; they suffer from paralysis: and the circumstance of this condition being temporary, or of its becoming permanent by the production of softening, appears to us to be regulated almost entirely by the position of the obstructed vessel. For if this be on the external surface of the brain, and some short distance before the artery has broken up into its minute branches, piercing the cerebral mass itself, then a collateral circulation may be readily established with surrounding vessels; on the contrary, if the obstruction be beyond such a point and within the cerebral tissue, then there is little chance of an adequate collateral circulation being set up, for the vessels into which the artery has resolved itself are very small, and traverse the brain-substance like a leash of collateral straight capillaries, recalling in some measure the straight course of the arteries passing through the medullary portion of the kidney.

The copious arterial supply to the brain through four large channels, so derived from the aorta or from one of its primary divisions as to admit the current upward of the blood direct from the left ventricle, involves a large amount of pressure on the arteries at the base of the brain, some estimate of which it is usually sought to deduce by citing the fact of the blood being propelled upon division of the carotid—as, for instance, in decapitation—to a distance of five or six feet. Certainly allowance must be made for the curved course of the arteries before entering the cranial cavity; but after this and every other conceivable cause of diminished cardiac impulse be admitted, a very considerable degree of pressure must necessarily exist. Indeed, we get direct evidence of this fact in the movement of the brain, which is synchronous with the systole of the heart. Some of the older physiologists attributed the pulsating movements of the brain entirely to the arterial impulse; but it is now generally agreed that the more evident upheaving and subsidence of the brain are due to respiration, —expiration being attended by an expansion of the brain-mass, and inspiration by its sinking. The cerebral pulsation is in fact double, being in part due to respiratory movements, in part to arterial impulse. But respiration acts only indirectly through the venous system, the flow of blood from the veins of the brain and from the sinuses being interrupted during expiration, whilst at the same time the spinal veins, being unable to empty themselves, force up into the cranium a certain quantity of cerebro-spinal fluid, and conspire still further to increase the fulness of the cranial cavity. In inspiration these conditions are reversed, and the blood is drawn rapidly towards the heart, with a corresponding subsidence of the encephalic mass. On the other hand, the movements due to the arterial pulsation at the base of the brain, though very much slighter, and in animals much smaller than man, inappreciable, are in fact of more real importance, for they are directly associated with the process of nutrition, the rising of the brain corresponding with the interpenetration of its whole tissue with arterial blood.

Any cause, therefore, which interferes with the due supply of arterial blood will more seriously affect cerebral function than one acting, for instance, through the respiration, as an impediment to the proper return of venous blood. Hence heart disease, causing a decreased supply of arterial blood to the brain, ought to be followed by more pronounced disturbance of function than lung disease, which directly creates an impediment only to the return of venous blood. Yet so interwoven are all the parts of the economy, that the impeded venous circulation soon comes to materially interfere with healthy nutrition, by opposing itself to the complete distribution of arterial blood; for an equilibrium must be maintained within the cranium, and if more venous blood be retained, less arterial will be required.

Although, as Burrows has demonstrated, the cranial cavity cannot be regarded as a perfectly closed sphere, and that therefore gravitation does operate upon its contents; yet its character, as a shut sac, is not without its influence on the circulation within it. For example, it is a common occurrence in anæmic patients to find more or less congestion of the cerebral membranes. And to account for this and similar phenomena, Otto Müller has insisted on the possession of a suction-power by the cranial cavity, acting in antagonism to the force of gravity; the two forces, however, always being *in equilibrio*. This suction-power is attributed to the atmospheric pressure on the body at large, and is illustrated by the effects experienced on any remarkable alteration of the latter, as in ballooning or in mountain-climbing. Again, the two forces may be made to co-operate instead of antagonizing each other, by placing the head in a dependent posture, and thus mechanically counterpoising the force of gravitation. Then disordered circulation is the consequence, but its ill effects are obviated by a provoked augmentation of the heart's energy to propel the blood onward, and by the escape of some of the excess through the external veins of the head.

The connexion of cerebral with heart disease has been much noticed, and yet in how many instances of the latter are brain symptoms deficient. Some explanation, therefore, is wanting to show why, if there be any such direct relation between lesions in those two organs, it should not display itself with more constancy. This explanation, we believe, is principally to be found in the state of the bloodvessels, the walls of which, if abnormal in any way, produce irregular circulation, disordered function, and diseased nutrition in the brain. The operation of this cause in the system generally has been well elucidated by Virchow's examination of disorders of the circulation dependent on lesions of the bloodvessels; and what is true of tissues generally, may, *à fortiori*, be maintained with respect to the tissue of the brain, which is intimately dependent on its blood-supply, and connected with a more complicated vascular arrangement than found elsewhere.

To illustrate some of the various directions in which the circulation in the brain may be modified by an alteration in the walls of the arteries going to and distributed within it, we may remark that diminished elasticity of the walls, whether due to defective nervous supply or to organic changes, favours the gravitation of the blood, and predisposes to tardy circulation and to stasis, particularly in the more dependent parts of the cranium. So, further, if this decreased elasticity of the vessels be combined with diminished cardiac impulse, the arterial circulation is still more embarrassed; the flow of blood through the capillaries is impeded, and an irregular distribution follows, which extends forwards to the venous radicles. And here, when there is heart-disease, we shall generally have the condition further aggravated by an impeded return of blood to the right side of the heart, by the general disturbance of the balance of the circulation, and oftentimes by associated pulmonary disease. Indeed, if there be free return of blood from the head, the effects of diminished cardiac activity are not necessarily injurious; but, on the other hand, if it be impeded, the venous stasis reacts backwards upon the arterial supply, and, as a consequence, we have occurring irregular circulation, local congestions, altered pressure on the brain mass throughout, retained blood with effete material, and obstruction to the supply of the arterial blood, so absolutely necessary to healthy activity and nutrition. Pathological observation teaches us how frequently the heart's energy is seriously affected by degenerative processes and by atheroma of its arteries, and we learn also that the mischief is not confined to the central organ of the circulation, but extends in a varying degree to the arterial channels, and even to the capillaries, throughout the body. The cerebral vessels are particularly prone to the morbid change, which destroys in a greater or less degree the elasticity of their walls, and thereby affects not only the vigour and regularity of their circulation through them, but also their function as the media for the nutrition of the brain, and renders them very liable to rupture. In short, in such morbid conditions of the vessels we have an explanation of some of the most important lesions of the brain—of congestion, of rupture and apoplexy, and softening. And if we proceed to analyse the history of cases of these lesions, the fact comes prominently out that heart-disease has been a most frequent concomitant.

Dr. Inman, of Liverpool, in his able essay on 'Atheroma in Arteries,' has adduced numerous examples illustrative of this connexion between degenerative disease of the heart and of the cerebral vessels. Analysing Andral's cases, he remarks that "of five fatal cases of cerebral congestion, three had phthisis, two atheroma and disease of the heart. . . . In sixteen fatal cases of cerebral hæmorrhage there was no history with two; in eight cases there was disease of the heart, and strong analogical proofs of atheroma; in the other six there were great debility, anthrax, carbuncle, &c."

Müller endeavours to explain why decreased cardiac energy should, on the one hand, predispose to anæmia of the substance, and, on the other, to congestion of the membranes of the brain. This he does very fairly by an appeal to anatomical and physiological facts. For, as can be proved, the impediment to the current of arterial blood will be greater through the multitude of fine arteries which penetrate the cerebral mass and originate in a sudden breaking up of a larger artery on the periphery, than through the more capacious arteries which are distributed in the pia mater and are continuous with the large arteries at the base of the brain. If this be so, then some degree of hyperæmia of the pia mater must follow; and if there be any lung disease, this abnormal distribution will be proportionally exaggerated, on account of the retardation of the escape of venous blood. But this is not all, for the presupposed defective energy of the heart and enfeebled respiration directly involve diminished brain movements, and, as a consequence, decreased activity of the cerebral circulation, for the upheaving of the brain and its pressure against the unyielding cranium are presumed to vigorously aid in promoting the discharge of venous blood from the membranes and sinuses, whilst the alternating opposite state of subsidence promotes and is connected with augmented arterial supply and pressure.

The persistence of this passive hyperæmia must in course of time originate structural changes in the cerebral substance and in its membranes and vessels; such, for example, as thickening, exudation, apoplectic effusions, chronic inflammatory processes, and the less appreciable, yet not less important, modifications of the nerve matter itself.

We have as yet only alluded to the morbid effects of enfeebled cardiac action and arterial pressure on the brain, but there are others induced by the opposite condition—viz., by undue heart and arterial impulse. However, the latter are of far less frequency as morbific causes than the former, whilst their mode of action is more readily conceivable. To this last-named circumstance it was no doubt greatly owing that active hyperæmic or inflammatory states were for so long a time supposed to be the basis of most head symptoms.

An increased arterial supply presupposes increased functional activity or hyperæmia, and when moderate only, and the venous system is free, no lasting ill effects necessarily occur. Such an amount of augmented cardiac and arterial activity we have exemplified in the effects of the emotions and passions. A more rapid whirl of blood through the cerebral vessels constitutes the leading character of active delirium; and whilst it proceeds there are the elements of exhaustion both in overworked function and in the interference with nutrition by the too rapid current through the capillaries. Should the vascular disturbance be more violent, there will be stasis at parts, and such an interruption of the nutritive changes as is comprehended under the term inflammation.

Dr. Robinson* has well studied the pathology of increased arterial supply, and in the case of the liver and kidney, has shown how largely the quantity of blood may be increased in them by their capability of enlargement. But in the case of the brain, though we may admit the possibility of an augmentation of the quantity of blood within it, yet its position within the unyielding cranium prevents it expanding beyond a very limited extent, and consequently the pressure primarily directed through the larger arteries, must be expended upon their ramifications and the capillaries throughout the brain, as lateral pressure, ending in complete arrest, with the effect of producing in a higher or lesser degree the phenomena of cerebral compression, of coma, and epilepsy. However, we cannot go so far as to suppose, with Dr. Robinson, that to have an uniform pressure, we must presume the cerebral mass to be under the operation of the hydrostatic law of equal pressure in every direction; for the brain, though soft, seems to us much too far removed in consistence from that state of fluidity which would bring it within the scope of that natural law of liquids.

In the foregoing remarks we have confined ourselves to the elucidation of some of the modes in which alterations in the cerebral circulation, particularly in the matter of arterial pressure, operate as causes of cerebral disorder, for though perfectly conscious of the potent morbid effects of altered quality of blood in the encephalon, it would have much too widely exceeded the subject-matter of a paper of this sort to have considered them. At the same time we have not omitted to point out that deranged circulation cannot go on without inducing changes in the tissue of the cerebral substance, in the coats of its vessels and in its blood, particularly when by venous retardation, as from pulmonary disease, the proper access of aerated blood cannot take place, and vitiated blood cannot be duly withdrawn.

But the brain, in its normal existence also is the subject of very considerable variations in its vascular system. The blood sent to it, the pressure upon it, and the venous current from it, are varied in sleeping and waking, by active mental exertion, by changes in atmospheric pressure, by strong exercise, and by the emotions and passions. The operation of these circumstances upon the cerebral circulation is a matter of every-day observation, and were it not that some new researches are before us on the physiology of sleep, by Mr. Durham, of

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* Contributions to the Physiology and Pathology of the Circulation of the Blood.

Guy's Hospital, we might abstain from further notice of those conditions. However, Mr. Durham's views, as sanctioned by experiment, demand consideration, especially as they are opposed to those which have universally been held among physiologists. For the accepted doctrine has been, that during sleep there is a certain degree of venous congestion of the brain and of its sinuses, whereas the recent experiments quoted demonstrate that though this be true of the comatose torpor produced by chloroform, it is not so of natural sleep, since in this condition there is no distension of the cerebral veins.

Mr. Durham's first experiment consisted in removing a portion of the skull and subjacent dura mater in a dog, so as to expose the cerebral hemispheres; but as it was objected that by this proceeding the brain was placed "in an unusual condition with regard to atmospheric pressure, and that thus an unnatural state of the circulation might be induced," and that, as from the removal of the natural support of the cranium, the brain projected from the opening, and might thereby probably entail some pressure upon its superficial vessels, the aperture was closed by an accurately-fitting watch-glass, rendered air-tight around the junction of its edges with the bone, by means of inspissated Canada balsam. However, the result showed that this precaution was scarcely needed, for when it was taken the appearances of the brain corresponded as nearly as possible with those observed when the cerebral mass was fully exposed. Pressure upon and ligature of the jugular veins, instead of developing similar phenomena to those witnessed in sleep, produced congestion with torpor, whilst the condition most like to that in natural sleep was met with when both carotid arteries were tied in young animals.

Another instance, showing how long an erroneous hypothesis may hold sway, is furnished by the history of the accepted doctrines respecting the state of the cephalic vessels after death by hanging. And we are led to allude to this matter here, because Ackermann adopted a similar mode of proceeding to that employed by Mr. Durham—viz., that of removing a portion of the cranium and dura mater, and then closing the orifice by a piece of glass. These experiments were extensively carried out, and are reported in Virchow's Archiv,* and the grand inference from them is that instead of there being congestion of the brain in hanging, there is anæmia, and that the reported presence of much blood within the cranium in some instances is a post-mortem result due in an especial manner to the remarkable fluidity of the blood after death by suspension.

To return from this digression to the account of the cerebral circulation in the sleeping and waking states: Mr. Durham confirms the statements of other physiologists regarding the sinking of the brain during sleep, its decreased movements and vascular activity, and he finds in these conditions that which is, "from physical causes, most favourable to the nutrition of the brain tissue." On the other hand, the remarkable arterial and capillary activity displayed so soon as an animal awakes from sleep, offers a condition "most favourable to oxidation of the brain substance, and to various changes in its chemical constitution," essential to mental exercise.

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* Band xv. p. 40: "Untersuchungen über den Einfluss der Erstickung auf die Menge des Blutes in Gehirn und in Lungen."

Thus far we have reviewed the variations to which the circulation of the brain is subject, both within the range of health and in disease, and have endeavoured to arrive at an explanation of the manner in which organic lesions may be superinduced upon disordered circulation. And from the considerations entered into, the delicacy of the brain tissue, and the apparent facility with which it may be injured, it becomes an object of inquiry, what are the conservative provisions supplied by nature to ward off the ever-threatening mischief? In following up this inquiry, we shall omit all consideration of the bony case in which the brain is so securely lodged, and confine our remarks to those internal arrangements in connexion with the vascular system, excepting, however, that most efficient provision against defective arterial supply in the "circle of Willis," already sufficiently insisted upon at the commencement of this paper. The conservative provisions to be considered, then, are—the structure and position of the membranes, the venous sinuses and choroïd plexus, the cerebro-spinal fluid, and the "derivative circulation" of the head.

This last of the arrangements specified we may take first, for it has the charm of novelty, and owes its elucidation to M. Sucquet, the title of whose memoir stands at the head of this article. To make the matter more clear, we must appeal to the account of this "derivative circulation" in another member than the head, and shall select the arm, since there it is most completely developed.

Now, in the arm and hand there are two well recognised sets of veins; one deep, following the arteries in their course; the other, superficial beneath the skin—which receive blood from the fingers and conduct it through the cephalic and basilic veins straightway to the large veins at the base of the neck. And it will have been noticed by every one, how variable are these superficial veins in their capacity and fulness; yet that, unlike the deep veins, they contain blood differing only in colour from arterial blood, and take their origin from the arterial ramifications of the fingers without the interposition of a capillary network between the two, are facts, in all probability, not so widely known. But to avoid further anatomical details, for which the reader may resort to the brochure of M. Sucquet, we may at once state that there is direct evidence from experiment to show the peculiarity and special character of the superficial system of veins in the arm. For instance, M. Sucquet points out that if we take a coloured fluid (and he prefers one coloured black on account of the distinctness of the effects produced), and inject it through the axillary artery, it neither returns by the large deep veins, nor even finds its way into their small radicles, but makes its appearance in the superficial veins of the arm and hand, which appear as brownish-black streaks through the skin. And further, the experiment displays the fact that these veins particularly abound at certain parts—for instance, in the hand and about the elbow, where, from their plexiform arrangement, they produce bronze-brown patches of discoloration.

Both anatomical considerations and experimental research therefore demonstrate, that the venous circulation in the arm is of a twofold character; one division being connected with the nutrition of the muscles of the limb, and forming venæ comites of the arteries; the other, independent of nutrition, with a separate and peculiar distribution near the surface, and affording a ready and direct channel for the rapid return of blood from the distal arteries and chiefly those of the hand. To this latter form of venous circulation, M. Sucquet applies the appropriate term "derivative;" and one other peculiarity attaching to it, and worth noting, is, that it is variable or intermittent, whilst that through the deep veins is constant. Further illustration of the distinct functional characters of the two sets of veins is furnished by M. Sucquet, by reference to the effects of cold and heat on them severally, but we must forbear quoting it.

Having made good these facts relative to the arm, the ingenious experimenter sought for the like in the venous system of the lower limbs and of the head. In both these segments of the body he succeeded in demonstrating the existence of this venous derivative circulation, although it is not developed in them to the same extent as in the arm, and in several respects presents apparent anomalies. Pursuing his experiments on the head, M. Sucquet shows that if the coloured solution be driven through the carotid arteries, it is in the lips, the nose, the ears, and the cheeks that the discoloration appears. The lower jaw, the temples and posterior portion of the head, are unaffected. And if the parts are dissected, it will be found that the injection has made its way into the branches of the facial vein appertaining to those parts mentioned, by the channel of the ophthalmic vein and its branches, the angular vein, the supra- and infra-orbital veins, and the smaller communicating malar veins. About the upper lip and the nose, and particularly within the cavity of the latter, upon its septum and the spongy bones, the vessels are so injected as to form a dense network, composed both of minute arteries and veins. Within the cranium and in and upon the surface of the brain all the veins are empty, except the superior longitudinal sinus and the cavernous sinus; the latter derives its supply from the same source as the ophthalmic vein, whilst the former receives the injection in less quantity from the vessels mounting up from the supra-orbital foramen. Thus the face is pre-eminently the seat of the derivative circulation in the head; its veins receiving the surplusage of arterial blood which may from any cause be propelled into the cranium, through a series of minute arterial radicles chiefly of the ophthalmic, and in a minor degree of the facial and auricular arteries.

Now there is this peculiarity about the derivative circulation, that it becomes more highly developed as age advances. Anatomists have long ago noted the amplification of the venous channels in old age, but it will be found that it is more pronounced in the veins of the derivative circulation than in others, a circumstance well illustrated in the large and always full veins of the hands of old people. And it is worth while to revert to the cranial circulation in connexion with this phenomenon. Thus it is well understood that there is in old age an increased determination of blood (so termed) towards the head, and at the same time a constant tendency to degeneration of the arteries supplying it, and therewith an increasing difficulty of circulation. Under such conditions we ought therefore to look for a higher development of the derivative system as a means of carrying off the excess of blood collected in the arteries and constantly propelled into them from the heart; and in fact we do find this expansion of the derivative circulation, and get proof of it in the frequent turgescence of the nose, and the reddening of the ears; in the enlargement of the middle meningeal vein, and in the development of the large venous channels, in the diploë of the cranial bones. Sucquet adds that the minute arteries of the scalp, of the dura mater, and elsewhere, open direct communication with the venous radicles by interposed lacunæ,—by a local degradation of the vascular system, assimilating it in some measure to that which is met with in the mollusca and other inferior animals.

Physiologists must feel much indebted to M. Sucquet for his able elucidation of the derivative circulation. It is a subject deserving attentive consideration and renewed research. We have adduced it chiefly as a conservative provision against over-repletion of the cerebral vessels, and particularly against undue arterial pressure; but it must be much more widely investigated, for it is doubtless a generally pervading system in the body, and should be sought after in all regions and structures, in some of which we may hope it will furnish a clue to peculiarities in vascular arrangements which have hitherto escaped explanation.

Respecting the other conservative provisions in the circulating system of the head we must be brief. The cerebral membranes have probably not generally received so much attention as they deserve, at least in their physiological relations. They are much more than protectors and coverings of the brain; they are intimately connected with its circulation, nutrition, and movements. They are, as Müller remarks, not only passively concerned in the cerebral movements, but they participate in them, and the circulation through their vessels is greatly facilitated by the intermitting pressure. By their high degree of vascularity, they exert a pressure on the brain, and are interposed, like an elastic pad, between it and the unyielding cranium, and by their varying fulness, never permit the existence of an empty space between the two.

Burdach called the pia mater the choroïd of the brain, and considered it to serve as a most efficient regulator of its arterial circulation. In truth, the choroïd plexus itself is nothing else than a portion of pia mater, modified by its situation, and also so constituted as to serve some special purpose. For it must be remembered that the large lateral sinuses, in which the principal choroïd plexuses occur, are contingent developments, arising from the extraordinary size of the cerebral hemispheres, which spread out and so fold on themselves as to leave an unoccupied central space; consequently the choroïd in a ventricle represents the portion of the pia mater belonging to that involuted surface of the hemispheres, shrivelled together, as it were, instead of being outspread upon it. But if this be the nature of the choroïd, this structure becomes, in connexion with its peculiar position, invested with special functions. The old notion that it was a gland was not far wrong; for Luschka's researches go to prove that it is an actively secreting organ, and in all probability is concerned in the production of the cerebro-spinal fluid. And to our mind, it is a farther indication of its secreting function and of activity in it, that it becomes with advancing life so generally diseased. It evidently attracts towards it a large quantity of the earthy salts—phosphate and carbonate of lime—from the blood; for in adults concretions of those salts may always be looked for in the meshes of its vessels and in the cysts which so often arise from their epithelial coating. Again, if we look into its anatomy, we find it has numerous vessels which pass to or from it out of the cerebral substance; it is connected on one side with the pia mater, and on the other sends processes to join with the velum interpositum and the choroïd prolongations extending from that structure into the third and fourth ventricles, and with the great venæ Galeni. Moreover, it contains no true capillaries in its meshwork of vessels, and is more highly developed in man than in any other animal. These facts intimate a derivative purpose; a provision against the ill effects of undue arterial pressure and fulness in the surrounding parts; a ready channel for the transmission of arterial excess into a system of veins which immediately run into the venæ Galeni and thence into the great sinuses of the occiput.

The sinuses of the cranium are among the most remarkable conservative provisions of nature. The collection of venous blood on the periphery of the brain, in non-extensible channels so placed along its anfractuosities as to obviate the chances of pressure, is an arrangement without parallel in any other structure. The brain is too delicate an organ to be exposed to the risk of pressure from elastic-walled, extensible vessels, whether arteries or veins, at least from that of any but those of the smallest dimensions; hence the arteries are distributed and supported in the meshes of the pia mater over the whole surface of the brain, whilst outside of all the veins are collected, and pour their blood into the great sinuses. These last then constitute an interposed venous system between the cerebral and the jugular veins—a protective arrangement securing, both by their size and frequent communications, the rapid removal of blood from the cranium, and yet allowing the presence of a large quantity without detriment to the subjacent cerebral mass. For though their fibrous walls will not distend, this very circumstance tends to compel the onward progress of the blood, favoured by gravity, towards the great venous outlet at the jugular foramen; and moreover, the pressure upon them gets partial relief through the veins opening into them from the exterior of the cranium and from the diploë, and particularly through the ophthalmic vein and its branches.

These provisions for a fluctuating quantity of blood within the cranium now considered, required on account of temporary interruptions and changes of the circulation and respiration, have their homologues in other regions in some other animals, where such interruptions are of much more frequent occurrence and continuance. We have not space to go into particulars on this head, but would refer for illustration to the anatomy of the Cetacea, in which we find arterial plexuses between the pleura and ribs and around the upper end of the spinal cord, as well as a marvellous venous plexus—its component veins without valves, like the sinuses and veins of the human brain, beneath the skin, communicating with the inferior vena cava. So likewise in diving animals such venous reservoirs exist—as, for example, in the seal—about the heart.

Some few words are needed respecting the cerebro-spinal fluid, the last of the conservative provisions connected with the cerebral circulation which we have to notice. As we observed in the earlier part of this paper, the objections raised by Virchow and Kölliker against the permeation of the cerebro-spinal liquid through all the cavities within and around the brain, are overruled by the researches of Luschka and others. Considering, therefore, its general diffusion to be proved, its importance in keeping up an equable pressure upon the brain must be admitted. Indeed, the fact that its sudden removal in animals causes a staggering faintness and convulsive movements, intimates its functional importance. And we gather the same conclusion from other facts, such as the increase of the fluid in old age, in persons in feeble health, and in cases where the arterial supply is largely cut off. In the last-named cases, its outpouring—if Schlossberger be correct, that a decrease of arterial blood involves a more rapid coagulation of the cerebral substance, and a consequent shrinking of the brain—must be particularly demanded.

Lastly, the cerebro-spinal fluid must be allowed to be something more than a mere passive agent concerned in the movements and circulation of the brain, if the choroïd plexuses are, as we believe, proved to be, secretory organs, and the pia mater to be covered with an epithelium. Indeed, regarding only the structure of the pia mater, there appears to be all the elements both for serous secretion and serous absorption, and the relative preponderance of one or of the other process will be determined by the amount of pressure of the brain against its bony case. How considerable this pressure is, is demonstrated by the thrusting out of the brain when an aperture is made in the cranium, by the moulding of the outline of the cerebral convolutions in progress of time upon the hard inner table of the skull, and by the unmistakeable signs of pressure exhibited by morbid effusions on the surface of the brain. So great have some pathologists deemed the importance of the pressure between the osseous cranium and the cerebrum, that they have concluded that the very thick cranial bones encountered in many cases of chronic shrinking of the brain, prove a natural compensatory endeavour to adapt them by an extraordinary development of diploë to the reduced size of the organ.

Review VIII.

1. *On Poisons.* Art.: "Arsenic." By Alfred S. Taylor, M.D., F.R.S.—*London,* 1859.
2. *Facts and Fallacies connected with the Research for Arsenic and Antimony.* By Alfred S. Taylor, M.D., F.R.S. ('Guy's Hospital Reports,' Oct. 1860.)
3. *On the Application of Electrolysis to the Detection of the Poisonous Metals in Mixtures containing Organic Matters.* By Charles L. Bloxam. ('Quarterly Journal of Chemical Society.')
4. *On the Alleged Practice of Arsenic-eating in Styria.* By H. E. Roscoe, B.A., Ph.D. ('Memoirs of Literary and Philosophical Society of Manchester,' 1859–60.)
5. *Annales d'Hygiène.* 1859.

We propose to lay before our readers a brief account of several important points in connexion with the industrial, accidental, and criminal uses of arsenic which, within the last few years, have attracted considerable notice—viz., the commercial and industrial applications of arsenite of copper in green paper-hangings, artificial flowers, &c.; the presence of arsenic in the water and mud of rivers; the alleged practice of arsenic-eating; the effects of arsenic on lower animals; and the latest researches upon the means of its detection.

1. *The commercial and industrial applications of arsenite of copper.*—Self-interest and money influences for some time essayed to ignore or explain away the ill effects of paper-hangings coloured with pigment containing Scheele's green.* Facts, however, proving beyond dispute the injurious and even fatal consequences resulting to the health from the use of such papers, have been so rapidly accumulated, that those who recently doubted or denied the possibility of such an occurrence are now among the foremost to direct attention thereto. We may here enumerate a few published instances of these ill effects of green paper-hangings.

Dr. Halley,† in relating his own personal experience of the poisonous influences of the green arsenite, recorded one of the earliest examples—one which attracted considerable attention, and became the subject of dispute, as we shall have occasion subsequently to show. Dr.

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* Aceto-arsenite of copper, called also Schweinfurth green, Vienna green, Imperial green, Emerald green.

	Composition.	Per cent.
4 CuO	160	31·50
3 AsO_3	297	58·46
$C_4H_3O_3$	51	10·94
$3(CuO,AsO_3)$ $+C_4H_3O_3,CuO$.	508	100·00

3 Arsenite copper (Scheele's green).
1 Acetate copper.

† Pharmaceutical Journal, Feb 1858.

Hinds,[*] having suffered also himself from symptoms which he attributed to the influence of arsenic, directed his attention to the green paper of his room, and finding it to contain arsenite of copper, forthwith had it removed, whereon the symptoms disappeared. Mr. Whitehead[†] related the case of a youth suffering under all the signs of arsenical poisoning, and in whose case all means in the shape of treatment were useless until the green paper-hangings were removed from a room which he inhabited. Dr. Wright[‡] recorded his personal sufferings from the same cause, and their removal in a similar manner. Mr. Gay[§] likewise placed before the profession his own experience of injured health, attributable to a like cause, and confirmed by Dr. Taylor's examination of the suspected paper. Mr. Kesteven[||] stated the particulars of a case of long-continued derangement of the health, relieved by the accidental removal of a green paper, receiving confirmation by the subsequent examination thereof. Dr. Alfred Taylor[¶] published an instance of conjunctivitis occurring with other symptoms of arsenical poisoning, as the result of inhabiting a room hung with green paper-hangings. The paper having been removed, the symptoms subsided, but soon afterwards returned. On close inquiry, it transpired that the patient had been dusting some books that had long stood undisturbed on his shelves. The relapse was explained by the discovery of particles of arsenite of copper in the dust on the books and shelves. Dr. Ballenden[**] relates also the cases of three children who, occupying a sleeping chamber newly papered with green hangings, "pined unaccountably," became emaciated, grew restless and nervous, and had involuntary twitchings of the muscles of the face, with smarting of the eyelids, ophthalmia, and gastro-enteric symptoms, from arsenical poisoning.

The latest recorded case has proved a fatal one:

> "On the last day of October, a little boy, three years and a half old, was seized with sickness, chilliness, and loss of appetite; on the following day, convulsions and a semi-comatose state supervened. During the night the little sufferer became very restless, and sank into collapse. At this time a sister was also seized with convulsions, followed by violent screaming and copious dysenteric discharge from the bowels. Alternations of repose and violent tetanic convulsions continued during the day, and the little boy died thirty-eight hours after the commencement of the attack. Suspicions were aroused by the peculiarity of the symptoms, and the simultaneous seizure of the two children. Three months previously they had been attacked in the same manner, and had recovered after leaving the house for the sea-side. The second illness seized the children after their return home. It was discovered that they had within the last few days been playing with their toys in the cupboard of the breakfast-room, the room and cupboard being both papered with a green flock paper; that two or three days previously they had been amused by helping to clear out the cupboard; and that the little boy had sucked a piece of lace which he found amongst the books and toys there. The evacuations were preserved, and sent to Dr. Letheby for analysis; subsequently, the sto-

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[*] Medical Times and Gazette, Feb. 14th, 1857.

[†] British Medical Journal, Sept. 25th, 1858.

[‡] Medical Times and Gazette, Feb. 12th, 1859. [§] Ibid., Jan. 22, and Feb. 12, 1859.

Ibid., Nov. 8th, 1859. [¶] Ophthalmic Hospital Reports, Jan. 7th, 1859.

[**] Lancet, Feb. 4th, 1860.

mach, with its contents, part of the liver, and the sigmoïd flexure of the colon, were also forwarded, together with portions of the green paper. Dr. Letheby reported that the stomach, liver, and evacuations contained distinct traces of arsenic. His examination of the paper revealed the fact that *no less than one-third of its whole weight consisted of arsenite of copper!* A piece of this paper, five inches square, contained seven grains of arsenic." *

Dr. Taylor found in another sample of green paper 59 per cent. of the arsenite. The same authority reports that the quantity of this pigment consumed weekly by one manufacturer is two tons!

The symptoms manifested in the various reported accounts of the ill effects of green paper-hangings have been clearly those of chronic arsenical poisoning; and when it is further borne in mind that these symptoms disappear when the suspected paper has been removed, there can be no room for doubt on the matter. The principal symptoms observed have been—headache, conjunctivitis, hoarseness, thirst, loss of appetite, nausea, dryness of mouth and throat, aphthous ulceration of gums and mouth, diarrhœa, colicky pains, prostration, general debility, sleeplessness, cutaneous eruptions, convulsions, coma, death. A reference to any treatises on toxicology will satisfy that the above are symptoms of chronic arsenical poisoning.

In opposition to the preceding facts and conclusions, it was argued by some, that the workmen employed in making and hanging green papers do not suffer in health from their occupation. In answer to this we observe simply, that these people do suffer, especially when they work on the dry powder—cutaneous eruptions and symptoms of chronic poisoning being commonly met with under those circumstances. It is when working the pigment in a moist state that the exemption is found. The proprietor of an extensive manufactory of paper-hangings informs the writer that years past he had made up many tons of the green pigment, and did not feel any evil effects, as it was in a damp state. "We use it in our factory; the men never complain while it is damp; but when it is dry, while rolling up the pieces, some particles get detached, which produce irritation of the nostrils and skin of the face, giving rise to pimples. Some appear more susceptible to these effects than others. The eruptions go off in a few days, without the appearance of sickness or impairment of the general health." The same effects, this gentleman observes, may occur from the detachment of particles of Scheele's green from the surface of "common machine papers." The arsenite of copper is laid on with gum or size, and in papers of inferior quality is readily detached by variations in atmospheric heat, or dryness, or moisture, or according as they are or are not varnished or sized. Even in the latter case it is not unattended with danger.

Our readers may remember that Dr. Alfred Taylor a few years ago removed arsenite of copper from the crust of a slice of bread that had become thus contaminated by the paint on a baker's shelves; in this case no volatilization of the poison could have taken place. It is futile to bring forward instances of exemption against the unanswerable facts adduced of illness produced by inhabiting chambers papered with Scheele's green, and the disappearance of such illnesses on the removal of the papers.

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* Lancet, Nov. 24th, 1860.

It has been stated that the green pigment of "flock" papers consists of chromate of lead and Prussian blue; and that our carpets, curtains, tables and chair covers, silks, muslins, &c., may become loaded with the poison of lead as well as of arsenite of copper. Dr. Hassall* is of opinion that the danger is not very great, because confined to unsized papers. It unfortunately happens, however, that these "unsized papers" being the cheapest, are those most commonly in use.

The "flock" of green papers is not coloured with arsenite of copper, nor with chromate of lead, or any other "body-colour," but is simply wool-dyed with *grains d'Avignon*, French berries, which are indigenous in France. Our correspondent, already quoted, informs us, that "the flock is all dyed"—"that it is now wholly manufactured in Paris"—and "that it is attached to the green paper by a mordant of lead and gold size, so that the ground of a flock paper is the only part that contains arsenic." That the flock is easily detached may be seen by the simple experiment of exposing a pane of glass smeared with glycerine in any room papered with such hangings; a very few weeks will suffice to cover the glass with a coating of flock-dust.

The late Duke of Wellington said that he mistrusted a man's judgment in any matter where his wishes were interested; this remark, like many uttered by that keen observer of mankind, has passed into a household word. We have lately seen a striking application of the maxim in a discussion which arose upon the publication of the cases of poisoning to which we have already alluded. The Commissioners of Inland Revenue, on the publication of Dr. Halley's case, requested their chemist, Mr. Phillips, to undertake the examination of the papers thus brought into discredit. Mr. Phillips's report† to the Commissioners was occupied chiefly in combating and criticising certain chemical explanations offered by Dr. Halley, who wrote as a pathologist; his chemical explanation proved to be erroneous; it was, however, less discreditable to him to have pointed out correctly the pathological fact, and traced it to its true source, while he failed to show its exact chemical relations, than it was that Mr. Phillips should have expended his energies in looking for the proofs of the existence of a vapour which was in no degree in question. The experiments of searching in the air of certain chambers for the vapours of arsenic proved nothing either way, any more than did those of Mr. Abel‡ or Mr. Dugald Campbell,§ who at considerable expenditure of ingenuity and trouble caused heated air rapidly and with force to pass over green paper-hangings, the air being subsequently made to traverse a solution of potash. No result followed, nor was likely to follow. These gentlemen missed the opportunity of an explanation by being altogether on the wrong scent. They might almost have been led in the right direction had they studied the objects to be seen in every sunbeam that shone through the chambers they examined. Had they reflected that arsenious acid volatilizes at 343° Fah., they would hardly have looked for it in the manner they did.

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* Lancet.

† Pharmaceutical Journal, Oct. 1858.

‡ Ibid., April 1st, 1859.

§ Ibid., May, 1858.

Dr. Taylor simply and logically disposed of all doubt and difficulty in this matter as follows, in a case quoted above:

> "Some of the dust was carefully removed on the 21st December from the tops of a few books by a feather, and submitted to a chemical analysis. The dust weighed one grain and a half; it had an olive-green colour, and under the microscope it presented the appearance of fibres, with numerous particles of various colours, chiefly of a greyish black. Treated by Reinsch's process, a portion of this dust yielded a deposit of arsenic, and there was therefore clear evidence that some of the arsenical pigment formerly on the walls had found its way through the glass-doors of the bookcase, and was deposited in the form of a fine dust on the tops of the books."

In fact, the vapours of arsenic had as little to do with the effects under discussion, as they had with the deaths of some persons on the occasion of a fire in Bloomsbury. A medical witness, Mr. Rogers, on the inquest gave it as his decided opinion, founded on the discovery of traces in the bodies, that death had been caused by the vapour of arsenic disengaged from some minerals that had been known to be on the premises. Had the witness been acquainted with the researches M. Bernard, he would have found an explanation readier to his hand and truer to the laws of nature, in the effects of carbonic oxide disengaged in the conflagration.

Following up the investigation, Dr. Taylor,

> "on the 23rd December, procured from the shop of Messrs. Marratt and Short, opticians, 63, King William-street, London-bridge, a quantity of dust for the purposes of analysis. The walls of this shop are covered with an unglazed arsenical paper, and, as I am informed, they have been so covered for a period of three years. In collecting this dust from the tops of the instrument cases, great care was taken not to touch the walls. The quantity thus collected for examination amounted to about 450 grains. It was nearly black, and under the microscope it appeared to consist of fibres and sooty particles. It was very light and flocculent. One hundred and fifty grains of this dust were examined by Reinsch's process, and enough metallic arsenic was obtained from this quantity to coat about ten square inches of copper-foil, in addition to a piece of copper-gauze. From the deposit on the latter, by the application of heat, octahedral crystals of arsenic were readily obtained. The cases had not been dusted for a period of nine months.
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> "The instrument cases are secured by glass-doors, and they are lined inside at the back with arsenical paper. A small quantity of dust was removed by a camel's-hair pencil from the projecting portions of the thermometers and barometers which are kept there. The quantity thus obtained weighed about eight-tenths of a grain, of which five-tenths were taken for examination. This half-grain of dust sufficed to cover with metallic arsenic a square inch of copper-gauze. A portion of this when heated, yielded a large number of well-defined octahedral crystals of arsenious acid."

Every case that has been recorded since the preceding has confirmed the conclusion. We have ourselves had repeated opportunities of verifying its accuracy. The results of inquiries among the manufacturers of green paper-hangings, of artificial flowers, and other manufactures in which Scheele's green is employed, all confirm the statement that it is to the diffusion of the poison in the form of impalpable dust that the effects are due.

We were not a little surprised to find, that notwithstanding the publication of these facts at the time, together with the conclusive refutation of Mr. Phillips's report in the 'Medical Times and Gazette' of Sept. 11th, 1858, Mr. Hunt, in his capacity of editor of the new edition of 'Ure's Dictionary' (Art. Arsenic), has very recently stated that Dr. Taylor attributed the effects of green paper-hangings to the volatilization of arsenic from their surfaces.

We cannot better express the inferences to be deduced from what has now been stated, than in the words of Dr. Taylor:

> "These facts lead to the inference that the air of a room, of which the walls are covered with an *unglazed* arsenical paper, is liable to be charged with the fine dust of the poisonous aceto-arsenite of copper. Those who inhabit these rooms are exposed to the risk of breathing this dust. The poison may thus find its way, either by the pulmonary membrane into the system, or it may affect the eyes, nose, and throat by local action. That but few cases of actual poisoning under these circumstances have occurred, is a fortunate circumstance; but cases involving serious symptoms only would be likely to attract attention. There may have been numerous instances of a disturbance of health depending on this arsenical paper, which, from absence of suspicion, has been referred to other causes. The degree of exposure—the state of health—peculiar susceptibility, and the eliminating power of the system—may account for the comparative rareness of these cases. The mode in which the pigment is laid on the paper may be such as to prevent in some instances the escape of the fine particles of dust. The fact, however, now demonstrated, that arsenical dust is breathed by those who occupy rooms thus papered, explains the similarity of symptoms observed, justifies the statements made by Dr. Hinds, Dr. Halley, and others, and proves that those who have experimented on this subject with negative results, have not taken the right course to arrive at the truth. Their results have to a certain extent misled the public, by teaching them to rely on what is now proved to be a false security. If, as a general rule, the quantity of arsenic which can penetrate the body from this source is very small, it is still desirable that arsenic should not be breathed day by day in any proportion. The defenders of this noxious manufacture will hardly go to the length of asserting that this arsenical green, which is a potent poison in the stomach, can exert no injurious effect when taken into the lungs, and yet unless this assumption be made, the inevitable inference is, that these papers should not be used for covering the walls of our dwellings.
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> "It is quite true that in numerous cases in which arsenical papers have been used, persons inhabiting such rooms have not suffered or appeared to suffer from any unpleasant symptoms. This, however, is no reason why we should wilfully make an arsenical atmosphere in our sitting and bed-rooms. It is not intended that arsenic in any form, in however small a quantity, should be a constituent part of the air we daily breathe, and the fact that we have before us a pleasant green colour, affords no compensation for the risk which may be incurred. Sanitary reformers object to an atmosphere containing $\frac{1}{2000}$th or even $\frac{1}{10000}$th part of sulphuretted hydrogen gas; but there are numerous instances in which it is breathed with impunity, and although no medical man would advise a person to live in such an atmosphere, yet it is most difficult to produce well-marked cases of illness which can be conclusively assigned to the effects of this gas. We should not knowingly contaminate the air we breathe

with any poison, whether in the shape of fine dust, gas, or vapour, and it appears to me that this rule should be most strictly observed with reference to a poison like arsenic."*

Manufacture of artificial flowers, toys, &c.†—The pigment employed by artificial flower-makers is the mixed arsenite and acetate of copper (*vert anglais*, or *vert de Schweinfurth*), with which are coloured various herbs, as the *poa vulgaris*, and others, as well as certain fabrics from which the artificial flowers are cut. For these purposes the green pigment is purchased by the workmen either in powder or as an aqueous preparation, and, according to the effect to be produced, is mixed with size, starch, gum, honey, or spirits of turpentine. Sometimes it is applied in the form of powder, to be dusted on articles already coloured with arsenical green. The Scheele's green is frequently mixed with chromate of lead, or picric acid, in order to modify the tint, or bring out more prominently its characteristic hue.

The mode of working with the pigment is as follows: the natural herbs, previously perfectly dried, are plunged into a shallow vessel containing an aqueous mixture of the arsenical green, which is briskly stirred with the plants held by the stalks. This operation, which is termed "tempering" (*Trempage*), occasions a good deal of splashing of the colouring matter on the hands, arms, and dress of the workmen. The coloured plants are then hung to dry for from thirty-four to forty-eight hours; at the end of this time they are tied in bundles or bouquets by their stems; and sometimes, to meet the caprices of fashion, are again dusted over with dry arsenite of copper (*le poudrage*). This preparation of "bouquets" constitutes one of the most dangerous parts of the manufacture, the pigment not being fixed by any mordant, is readily detached as a fine powder, which penetrates the skin, and is constantly breathed by the workpeople; the danger being greatly augmented when the powdering is also practised.

In this case the workpeople are exposed to the noxious influences of an atmosphere loaded with poisonous particles. The addition of spirits of turpentine, which gives a smoother aspect, prevents the immediate diffusion of the poisonous powder in the air, but as the turpentine dries the pigment falls off in flakes, becomes mixed with the dust of the floor, and only somewhat retarded in mixing with the air of the apartments. This branch of industry is moreover followed under the worst possible circumstances, in small, badly-ventilated, and ill-cleansed apartments, so that the furniture and garments of the workmen are always impregnated with this poisonous powder.

Those who prepare the tissue for the fabrication of artificial leaves, are exposed to the more deadly influence of the poison, as they use the arsenite of copper alone, or mixed with starch. Some makers in the first instance dip the cloth into a mixture of picric acid and green indigo, while other manufacturers employ colouring matters of a harmless character. During this work the fingers and fore-arm of the workmen are covered with the arsenical mixture. The cloth is by the use of the hands thoroughly impregnated with the paste until it attains an uniform colouring, it is afterwards stretched tight and dried. In the subsequent manipulation, the diffusion of the dust of the arsenite exposes the workpeople to as great danger as in the case of those who colour the plants. These noxious effects are somewhat diminished, but not altogether prevented, by the process of calendering. When the coloured leaves are coated with wax, they are afterwards unlikely to prove injurious to those who make them up into bouquets, but very few are so protected, since the process alters the hue of the green. The injurious consequences of working with arsenical greens are found to vary according as the poison is employed in the moist or dry state; among the workmen who prepare the plants and the cloth, they may be distinguished as external and internal. The former present themselves as eruptions—e.g., diffused erythema, minute vesicles, papulæ between opposing surfaces of the skin, lastly, pustules, with ulceration and gangrene of the surface. At the bend of the fingers, elbows, arms, the circumference of the lips, the angles of the nose, on the forehead, along the edges of the bands of the hair in women, on the scrotum and inside the thighs of men, and between the toes—in the two last mentioned regions assuming the characters of syphilitic eruptions. The two last affections, however, are exceptional with this class of workmen, and are seldom met with except where an abrasion of the surface exists. In short, wherever the arsenical pigment comes in contact with the skin, and there only, are these eruptions seen, clearly separating them from the internal effects of the poison.

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* Pharmaceutical Journal, Jan. 1859.

† Dr. Vernois: Annales d'Hygiène, 1859, p. 319.

Still more serious results follow among the workmen employed in drying the coloured cloth; the sharp points fixed to the wooden frames on which the cloth is stretched, are frequent causes of punctured wounds. The inoculation of the arsenical salt produces vesicles, then pustules, followed by fungoïd ulceration and gangrene of the skin. These ulcers, by their frequently daily renewal, are slow in healing, and are further aggravated by the picric acid which is mixed in the paste. Glandular swellings of the axilla, and other indications of absorption of the poison resulting from these ulcers, not seldom compel the workpeople to seek admission into the hospitals. The appearance of the hands is characteristic of the occupation, the skin being of a greenish-yellow, and the under surface of the nails filled with a yellowish-green crust, while the nails themselves are stained yellow by the picric acid, and their edges disfigured by whitlows or ulcerations. A close resemblance to the local effects of tartar emetic is often presented by these arsenical eruptions.

The introduction of the arsenical powder into the air-passages by respiration, and in contact with the mucous surface of the alimentary canal in articles of food, form the transition from the external to the internal effects of this pigment.

The phenomena of chronic internal poisoning observed among these persons, are loss of appetite, nausea, colic, frontal headache, with a sensation as if the temples were squeezed in a vice. The females become chlorotic and anæmic, suffer gastric derangements and constant headache. The share of the work that falls to the women being that of folding, and stamping the material for artificial leaves, and the making these into bouquets, they do not present the discoloration of the hands and other external affections exhibited by the men, but they suffer all the evil consequences of constantly breathing an atmosphere loaded with the poisonous dust.

There are in Paris about nine hundred artificial flower dealers, and upwards of fifteen thousand persons engaged in the manufacture, one-fourth of which number are directly or indirectly engaged in the use of Scheele's green, many of whom can earn from four to six francs a day. Inasmuch as it appears to Dr. Vernois difficult, if not impossible, to throw so large a population out of work by prohibiting the manufacture, the author submits for the consideration of the Parisian police a series of suggestions for diminishing the dangers attending the employment of arsenical pigments.

Dr. Hassall* has related two instances of poisoning by Scheele's green employed in the manufacture of artificial flowers.

In November of the past year a girl, aged nineteen years, died from the poisonous effects of arsenite of copper employed in dusting wax leaves. Mr. Paul, of Burton-crescent, stated on the coroner's inquest† that he had found, on examination of the body, the presence of inflammation and ulceration in the stomach, and inflammation also in the bronchi. Mr. Paul had detected the arsenic in the lungs, liver, and mesenteric glands. The medical witness further stated that he had seen many cases of cutaneous diseases in young persons from the same factory. The sister of deceased had died some time since with similar symptoms, but the cause had been overlooked.

Poisonous Confectionary, &c.—Dr. Taylor‡ has recited several examples, and others are only too familiar, of poisoning by confectionary, such as the frightful catastrophe at Bradford.

M. Chevallier§ relates the case in which arsenite of copper was used for ornamenting a boar's head supplied at a breakfast given on a festive occasion by an eminent Parisian lawyer. The head was artistically decorated with masses of fat, which were coloured red and green. One of the guests, acquainted with chemistry, was struck with the rich green colour of the fat, and reserved a portion for private examination. He found the colouring matter to be arsenite of copper, forming two per cent. of the fat!

In Dr. Hassall's work,‖ we find that the more potent mineral poisons are as freely and recklessly employed to colour confectionary, as are the most inoffensive vegetable dyes. Preparations of lead, mercury, copper, and arsenic, are used to give the brightest colours—e.g., red lead, vermilion, orpiment, iodide of mercury, blue verditer, chromate of lead, carbonate and acetate of copper, &c. &c., are among the pigments enumerated as being used to colour confectionary. Surely the laws might render it impossible to apply such dangerous colours as these, and that without infringing upon the just liberty of the subject, or trespassing upon the legitimate trade of the druggist or pharmaceutist.

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* Lancet, Dec. 1st, 1860.

† Daily News, Nov. 26th, 1861.

‡ On Poisons, p. 430 et seq.

§ Ibid.

‖ Adulterations Detected, 1857, p. 491.

Dr. Rose, of Hampstead,* relates the narrow escape of a child, aged nine months, whose life was endangered by sucking a green paint out of a child's paint-box. The paint proved to be Scheele's green.

"Air-balls" of coloured india-rubber† were found by a coroner's jury to have been the cause of death to two of the children of a manufacturer of these toys. The bright green colour on toys of various descriptions has been found to consist of arsenite of copper.‡

The papers used for wrapping up articles of food, tobacco, snuff, &c., are often coloured with arsenical green.§

MM. Erdmann and Ziureck (in Berlin),‖ discovered that some green tarlatans are covered with arsenite of copper. The colour was merely fixed on with starch paste, so that the least friction sufficed to remove it. Erdmann also ascertained that a colour known in Berlin as "cochineal red" contains arsenic in the form of arseniate of alumina. In considering these various modes of arsenical poisoning, we cannot omit the expression of regret that the attempts to introduce more efficient legislative restrictions upon the sale of poisons should have been opposed and thwarted by certain members of the Pharmaceutical Society. The fatal accidents that have occurred during the last few years having given rise to a growing conviction of the necessity for some more stringent laws than exist at present, bills have been introduced into Parliament for this purpose. Parliamentary committees gave their attention to the questions involved, and a mixed board was proposed, to consist of physicians, apothecaries, and druggists. The Council of the Pharmaceutical Society succeeded in defeating these measures, on the ground that no restrictions but the requirement of a higher standard of education from the vendors of drugs can ever prove efficient safeguards. The College of Physicians and the Society of Apothecaries admit the advantages of education in all engaged in the sale or preparation of medicines, and would doubtless effect this very desirable reformation through the means of the proposed board—but the Council of the Pharmaceutical Society has opposed every measure which does not place the chief control in their own hands, or which does not confer upon them exclusive privileges and exempt them from the clauses of the Act. A glance at the past volumes of the 'Pharmaceutical Journal' will confirm the justice of our opinion, that but for the opposition of this Society we should not have to lament so many fatal accidents from the ignorant or careless use of deadly poisons.

The following instance has been made widely known by the daily and other journals:

"*Arsenic in the water and mud of rivers.*—From the northern and western sides of Black Combe, a mountain in the southern part of Cumberland, situated near the sea, numerous streams or *becks* originate; I believe that one only of these exhibits any marked peculiarity. Whitbeck—such is the name of this stream—is fed by several small springs, and it was from the source of the most southerly of these where it rises from the ground, and at an elevation of about 900 feet from the sea, that I obtained a specimen of the water for examination.

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* Lancet, March 5th, 1859.

† Medical Times and Gazette, May 22nd, 1848.

‡ Chemical News, Dec. 1860.

§ Medical Times and Gazette, Jan. 16th, 1858.

‖ Chemical News, vol. i. p. 83.

"On the 29th of June in the present year, the water, at the time of collection, had a temperature of 8° 5′ C., the air being 10° 6′. The reaction of the water as it issues from the earth was faintly but unmistakeably alkaline: on testing the water after ebullition the effect was more decided. The water from many other sources in the neighbourhood of Whitbeck, where decomposing granite is of common occurrence, has an alkaline reaction. A large and deep pool in the course of Whitbeck towards the sea shows the colour of the water to be a rich clear greenish blue.

"The water, on examination, gave distinct indications of the presence of arsenic. This element, which here probably exists as an alkaline arsenite, occurs not as a mere trace, but in determinable quantity. I have not yet ascertained the amount present, but hope to do so shortly, when I have obtained specimens of the water collected at different seasons of the year. I have satisfied myself, however, that in some seasons of the year the quantity present approaches a *good fraction of a grain of arsenic (metallic) in each gallon of water*. [The italics are ours.]

"The arsenic in the water of Whitbeck is most probably derived from the veins of arsenical cobalt ore through which it percolates.

"The arsenical water is *habitually used for every purpose* by the inhabitants of the little village of Whitbeck, and, as far as I can learn, with beneficial rather than injurious results. But it is remarkable that Whitbeck, though in every respect suitable for trout, is the only stream in the neighbourhood from which that fish is absent; eels, however, have been found in it. Ducks will not live if confined to this arsenical water. When the railway was being carried past Whitbeck, the first use of the water quickly produced the usual marked effect on the throats both of the men and horses employed on the works. The soreness of mouth from which they at first suffered soon, however, disappeared, and in the horses gave place to that sleekness of coat assigned as one of the effects produced by the administration of arsenic. It is a question how far the rosy looks of the Whitbeck children and the old age which a large proportion of the inhabitants of the village attains are to be attributed to the arsenic present in the water they drink."*

Mr. Campbell† finds that the sands of rivers yield the greater proportion of arsenic. The presence of arsenic in this, as in many other rivers both in this country and other parts of the world, affords proof that so minute a fraction of arsenic may pass through the human system without producing any ill effects; but if our readers will compare the dose as stated above by Mr. Church, and as expressed by the analyses of Taylor and others, with that affirmed to be habitually taken by the Styrian arsenic-eater, it will be at once apparent that the latter statement receives little support from the former, which clearly shows the poisonous action of arsenic upon men and animals.

Among the explanations at one time put forward to account for the presence of arsenic in a body exhumed, was that of the absorption of the poison from the soil of the graveyard. We need only refer to this point to mention that it has been completely set aside by the demonstration that arsenic, if present in the soil, exists there in an insoluble form. The presence of arsenic as a normal constituent of bone, once received by some toxicologists on the authority of Orfila, has been entirely abandoned on failure of proof by the author of the statement.

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* Mr. Church: Chemical News, Aug. 25th, 1860.

† Ibid., April 6th, 1861.

Dr. Letheby* stated that Dale's perchloride of iron, proposed as a disinfectant of the water of the Thames, contained in each gallon no less than 238 grains of chloride of arsenic—"enough to kill forty persons"—consequently, that as one million of gallons of sewage would require sixty-six gallons of perchloride to disinfect them, there would be present enough arsenic to kill 2640 persons, and that to disinfect the whole daily amount of sewage (80,000,000 of gallons), 180 pounds of the chloride, the most dangerous preparation of arsenic, would be thrown in the river daily.

Dr. Hofmann and Dr. Frankland reported as the result of their analyses just half the above quantity of arsenic,† and pointed out that the mixture of perchloride of iron with the alkaline solutions of the sewage produces the hydrated peroxide of iron in combination with arsenic, a perfectly insoluble and consequently inert substance. They had mixed sewage with the solution of perchloride, and found that the filtered fluid contained neither iron nor arsenic, but that these were precipitated in the insoluble residue.

They estimate the quantity of water in the Thames passing the metropolis to be 1,000,000,000 gallons per diem, that the quantity of arsenic, even if it could be retained in the soluble form, would be one grain in 1450 gallons, or one part in 3000 of the mud; whereas the Wiesbaden water, usually regarded as a wholesome water, contains one grain in 166 gallons; so that it would take ten years for one individual to consume a fatal dose of arsenic, drinking at the rate of a gallon of Thames-water daily: a quantity double the average that is consumed. Or, as MM. Vertmann, and Rodgers have pointed out,‡ even on Dr. Letheby's own calculations, four gallons must be imbibed to incorporate one-sixteenth of a grain of white arsenic—a medicinal dose often taken three times a day. The waters of the Whitbeck stream in Cumberland contain a fraction of a grain per gallon, and are used for all domestic purposes.

What might be the effect of the insoluble compound of iron and arsenic if taken into the stomach, is beside the question, since being insoluble, it is not taken, but is deposited with the mud, and therefore is not likely to come in contact with the "acid secretions of the stomach."

We are at a loss to conceive how so minute a proportion of arsenic as one grain in 1450 gallons of water could be a source of protection in criminal cases, as suggested by Dr. Letheby, supposing a murdered corpse to be thrown into the Thames after deodorization by Dale's perchloride of iron. We cannot participate in Dr. Letheby's alarm. Dr. Taylor found in the summer of the last year—1860—that two ounces of the mud of the Thames yielded $\frac{1}{2000}$th of a grain of arsenic.

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* Chemical News, Aug. 17th, 1860.

† This difference may be owing to differences in the manufacture of the compounds compared.

‡ Chemical News, vol. i. p. 150.

As no chemical liquids were introduced during that summer for the purpose of deodorizing, the arsenic must have been derived from factories on the banks of the river, or diffused through the soil.

Arsenic-Eating.—It is obvious that before any weight can be allowed to attach to the alleged impunity with which large and repeated doses of arsenic may be taken, before any value can be accorded to the story of the Styrian arsenic-eaters, when adduced as an argument on occasion of a criminal proceeding, its truth must be undeniably established. If, moreover, the impunity pretended to be bestowed, together with the possession of heightened personal charms, by the practice of arsenic-eating, be altogether fallacious, then the fact cannot be too widely made known as a warning to those—and some there are—who may be tempted to acquire such advantages at the risk of their lives.

Many highly coloured accounts of the practice of arsenic-eating have been given to the public. We quote as the best known that of the late Mr. Johnstone:*

> "The Styrian peasant-girl, stirred by an unconsciously-growing attachment, confiding scarcely to herself her secret feelings, and taking counsel of her inherited wisdom only, really adds by the use of *hidri* to the natural graces of her filling and rounding form, paints with brighter hues her blushing cheeks and tempting lips, and imparts a new and winning lustre to her sparkling eye. Every one sees and admires the reality of her growing beauty; the young men sound her praise, and become suppliants for her favour. She triumphs over the affections of all, and compels the chosen one to her feet.
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> "Thus even cruel arsenic, so often the minister of crime and the parent of sorrow, bears a blessed jewel in its forehead, and as a love-awakener becomes at times the harbinger of happiness, the soother of ardent longings, the bestower of contentment and peace."

It is scarcely possible to condemn too severely such language as the preceding. The writer having a scientific reputation, has had his followers in the "felon" literature of the day. Even a professed physiologist—Mr. G. W. Lewes—has, in a second edition of Mr. Johnstone's work, endorsed these dangerous embellishments of a disputed "fact," without the slightest expression of caution in their reception. The author of a recent popular work on the metals† repeats the same tale, and adorns his pages with a woodcut of a supposed Styrian maiden with heightened charms, and admiring swain, grateful for the "blessings" of "cruel arsenic!"

So entirely at variance with all our experience of the effects of arsenious acid did these narratives appear, that they were received with a highly reasonable scepticism by both British and foreign toxicologists. In order, however, to ascertain what might be the grounds of such marvellous statements, Mr. Kesteven made a series of inquiries, with a view to discovering the truth, or the source thereof. The results, for the details of which we must refer our readers to the original papers,‡ were, that all the accounts put before English readers in various non-professional journals, were traceable to one source—viz., a paper by an Austrian traveller, Von Tschudi;§ that the

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[*] Chemistry of Common Life.

[†] Pepper: Playbook of Metals, 1861, p. 432.

[‡] Association Medical Journal, 1856.

[§] Wiener Medicinische Wochenschrift, 1851.

assertions of Von Tschudi have been, with sundry additions and embellishments, the staple of the tales repeated by Johnstone,* and by Mr. Boner in 'Chambers' Journal.'

Correspondence with medical men in Styria afforded no direct evidence of the fact, but furnished only second or third-hand information, upon which, as Mr. Kesteven fairly urges, we can place no reliance in so important a matter. Arsenic works exist in several parts of Cornwall, and arsenic is eliminated from the ores of other metals in the metallurgic operations to which they are submitted. Inquiry was made among the medical men resident in the vicinity of the Cornish works. The correspondence published by Mr. Kesteven† shows the poisonous influence of the external application of arsenic in the cases of the lower animals and of vegetables, as well as on human beings. At the Cornwall Lent Assizes, 1851, on occasion of a trial to suppress the nuisance of some arsenic works, it was given in evidence that where the smoke from the furnaces fell, the vegetation became blackened and dried up; that cattle and horses grazing thereon were poisoned, lost their hair, and died. In all these works in Cornwall the greatest care and precaution is required to protect the health and lives of the workpeople. Again, the statements of Von Tschudi have themselves a want of coherence and probability. Thus, while we are told that the Styrian peasant has acquired such an impunity that he can eat arsenic as a condiment to strengthen his digestive and respiratory functions, and as a cosmetic to improve the complexion, we are informed that deaths from poisoning by arsenic are by no means infrequent. Such facts would seem to imply that arsenic is a poison still, even in Styria as in other parts of the world, rather than that the Styrian peasantry are poison-proof. We are, moreover, asked to believe, that although the arsenic acts instantly in augmenting the respiratory capacity, or in heightening the personal charms, symptoms of poisoning are manifested if the use of the poison be discontinued, or if it be not taken at the full moon; under the latter conditions it obviously cannot be taken daily, as alleged.

The conclusions arrived at by Mr. Kesteven from the data then before him were to the effect that the story of the Styrian arsenic-eaters was incredible and totally wanting in proof. These conclusions remained unchallenged until in the last year Mr. Heisch, of the Middlesex Hospital, and subsequently Dr. Roscoe, of Manchester, reopened the question by the publication of several apparently well-attested cases.

Mr. Heisch‡ obtained information from Styria, which he affirms (but we cannot agree in the opinion), that "if human testimony be worth anything, places the fact of the existence of arsenic-eaters beyond a doubt." One of his informants, Dr. Lorenz, Imperial Professor of Natural History, formerly of Salzburg, states that—

"The arsenic is taken pure in some warm liquid, as coffee,§ beginning with a bit the size of a pin's head, and increasing to that of a pea. The complexion and general appearance are much improved, and the parties using it seldom look so old as they really are; but he has never heard of any case in which it was used to improve personal beauty, though he cannot say that it is never so used. The first dose is always followed by slight symptoms of poisoning, such as burning pains in the stomach, and sickness, but not very severe. . . . Once begun, it can only be left off by gradually diminishing the daily dose, as a sudden cessation causes sickness, burning pains in the stomach, and other symptoms of poisoning, very speedily followed by death."

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* Chemistry of Common Life. † Association Medical Journal, 1856.

‡ Chemical News, May, 1860.

§ Dr. Kopp, in the Moniteur Scientifique, contradicts Dr. Lorenz, inasmuch as he says, on the authority of Dr. Schäfer, that no fluid is drunk when taking the arsenic.

"As a rule, arsenic-eaters are very long-lived, and are peculiarly exempt from infectious disease, fevers, &c.; but, unless they gradually give up the practice, *invariably die suddenly at last.*"

We have italicised the preceding words to point the difference between the picture by Dr. Lorenz and that by Johnstone. Mr. Heisch gives the case of the director of some arsenic works near Salzburg, as related by himself, suppressing, however, his name, which nevertheless, he states, will be forthcoming should any judicial inquiry render necessary positive evidence of the fact. This gentleman states that he began arsenic-eating at the age of seventeen years, while studying assaying. He did so at the advice of M. Bousch, Professor of Chemistry and Mineralogy at Eisleben, as a protection against the ill effects of the fumes of arsenic. The dose with which this gentleman commenced was *three grains* daily! In conformity with his preceptor's advice, when getting towards fifty years of age, he then reduced his dose to *twenty-three grains!* The first dose was followed by slight perspirations and griping pains in the bowels, and after three or four hours a loose evacuation. This was followed by a keen appetite and feeling of excitement. With the exception of the griping, every dose since has been followed by these same effects. Long-continued interruption only is followed by ill consequences. On two occasions he has, at the persuasion of friends, attempted to leave it off, and has suffered severely in his health, restoration being only obtained by resuming the use of the arsenic.

Mr. Heisch has collected several "reported" cases, but the above is the only one in which he has obtained direct information. But this, we may remark, comes to him at second-hand, while its authentication is withheld.

The first dose (three grains!) is admitted to be attended with symptoms of poisoning, and yet the dose has been gradually augmented until twenty-three grains are a reduced dose! and although every dose has been attended with the same symptoms, "except the griping," we are told that the discontinuance of the dose only has been followed by ill consequences! If such be the character of the single instance in which direct information has been obtained, what may be supposed to be the value of the "reported" cases? especially when compared with the accidental poisoning of three hundred and forty children at the Annerley schools by less than one grain each; and of more than two hundred persons, with seventeen deaths, by the Bradford lozenges.

Wonders never cease! Dr. Lorenz adds that—

"In his part of the world, when a graveyard is full, it is shut up for about twelve years, when all the graves which are not private property by purchase are dug up, the bones collected in the charnel-house, the ground ploughed over, and burying begins again. On these occasions the bodies of arsenic-eaters are found almost unchanged, and are recognisable by their friends!"

Such a remarkable occurrence as the preservation of human bodies above stated, demands more accurate confirmation than we can discover in Mr. Heisch's paper. We should like to know how those preserved bodies are disposed of. Arsenic, it is well-known to physiologists and chemists, is not an accumulative poison, but is one that is constantly eliminated from the system, especially by the kidneys. MM. Danger and Flandin found no trace of arsenic in the bodies of animals to which doses of fifteen grains daily had been given: the bodies were examined three days after the last dose. The sudden appearance of symptoms of poisoning under the medicinal use of arsenic usually spoken of as proofs of its cumulative character, as pointed out by Dr. Taylor, is owing to too frequently repeated, or too rapidly increased doses. Statements so contrary to universal experience require the most unexceptionable evidence to support them.

Dr. Roscoe's pamphlet, while to the same effect as the published statements of Von Tschudi and others, does not countenance the exaggerated colouring of Johnstone and his followers. Avoiding the discussion of "the superstitious notions with which many of the reports naturally enough abound," Dr. Roscoe addresses himself to the question whether arsenious acid is or is not extensively prevalent among the Styrian peasantry, and taken by them "in quantities usually supposed sufficient to produce immediate death." In the first place, Dr. Roscoe gives us the result of his analysis of a portion of "Hidrach," forwarded to him with the certificate of a district judge, that he received it *from a woman who said she took it from a man whom she believed to be an arsenic eater.* The analysis gave 99·97 per cent. of AsO_3. We have marked in italics the alleged connexion of this specimen of "Hidrach," with the argument for the proofs of the alleged arsenic-eating, inasmuch as we cannot believe that such third-handed evidence would be received in any court of law in support of any statement.

Dr. Roscoe finds that seventeen medical men in Styria who had reported on this subject to their government, "agree in acknowledging the general prevalence of a belief that certain persons are in the habit of continually taking arsenic in quantities usually supposed to produce death;" while on the other hand, no one of them "denies, or attempts to disprove, the truth of the generally expressed opinion concerning arsenic-eating."

Dr. Roscoe must excuse us when we remark that these opinions of seventeen medical men prove nothing more than was known before—viz., the prevalence of a belief in the practice. We apprehend that it was to combat this belief, not in Styria only, but in England, and to point out the frightful consequences of such a belief, that Mr. Kesteven was at the trouble to push his inquiries into the very region of the alleged practice. With no other success than that of obtaining second-hand reports, Dr. Roscoe has followed the example.

Dr. Roscoe's strongest case is one of simple hearsay. A man informs

Dr. Knappe, and Dr. Knappe informs Dr. Shäfer, and Dr. Shäfer reports to the Academy of Sciences of Vienna, and Dr. Roscoe repeats to the Philosophical Society of Manchester, the following :—

> "In the presence of Dr. Knappe, of Oberzehring, a man, thirty years of age, and in robust health, on the 22nd of February, 1860, ate a piece of arsenious acid weighing four grains and a half, crushing it between his teeth and swallowing it, and on the 23rd, another piece weighing five grains and a half. Dr. Knappe does not prove that it was arsenious acid that was taken into the mouth. His urine on that occasion was carefully examined, and shown to contain arsenic. On the 24th, he went away in his usual health, without, however, having been watched as to what became of the arsenic, or whether he had soon afterwards rejected it from his stomach. He informed Dr. Knappe that he had been twelve years in the habit of taking the above quantity three or four times a week."

The quantity here alleged to have been taken is so large as to lead to the suspicion of jugglery. The feat could have been performed with the less risk, if, as is clear from the narrative ("a piece," "crunching it between his teeth"), the solid semi-transparent acid was used. Vomiting might then be produced, leaving, nevertheless, sufficient to furnish evidence of its presence in the urine, as only a small quantity is carried off daily by this secretion, while from the insoluble form in which it was taken, life would, under the circumstances, be safe. Dr. Taylor has recorded cases of persons not dying from much larger doses, but in these there had been vomiting and purging. The trial would have been very different had a chemist previously dissolved a like dose of white arsenic, administered it himself, and subsequently himself watched the taker for at least ten or twelve days. The following experiment bears out this view of the case. A small mass of semi-transparent arsenious acid was suspended by a horsehair for *sixteen years* in a closely-corked six-ounce phial of distilled water. At the end of that period the under surface presented a slightly eroded and crystalline appearance, the entire mass having assumed an opaque white aspect. By careful evaporation below 212° Fahr., the solution yielded in two experiments 1·46 and 1·47 per cent. Without an opportunity of watching this "Styrian arsenic-eater," we cannot give credit to the assertion that he is able to take enough arsenic to kill from seven to ten Englishmen, remaining himself *always in good health.* Our incredulity is strengthened, moreover, by the fact, that while these reports, from which Dr. Roscoe quotes, contain accounts of many persons in Styria by whom arsenic is regularly taken into the system with impunity, Dr. Shäfer, the reporter of the preceding cases, states, that *within two years, out of twenty fatal cases of poisoning** *which have come under his single notice, no less than thirteen were cases of arsenical poisoning!* Many of these are stated to have occurred from overdoses, not from the withdrawal of the arsenic. In a sparsely populated district, this number is obviously so alarmingly high as to excite the vigilance of the authorities. The sale of arsenic, we are told in all the narratives that have yet appeared, is strictly prohibited by law, and is obtained only by stealth and used only with the greatest secrecy, yet the practice of arsenic eating is said to be so common as to admit readily of verification.

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* Moniteur Scientifique, 1860.

Arsenic, says another of the reporters, Dr. Haller, is used in Styria for flavouring cheese, adding, that he was once eye-witness to the preparation of such cheese. The only evidence in support of his statement is, that he saw a woman dissolve a white powder in some boiling water, and mix this with the curds. The white powder "she said was Hütterrauch." Dr. Haller, however, states further, that being directed to where he could obtain some of this kind of cheese, he went and procured some, which he ate to the amount of the size of two walnuts, without experiencing any ill effects. On another occasion, however, a piece the size of a small nut caused vomiting and colic. As the flavour of this cheese was bad, these symptoms were probably due to its decomposition. There is not a shadow of chemical proof that arsenic was taken on either occasion.

Mr. Stern, of Kerndorf, was called to a case of arsenical poisoning arising out of an excessive dose, by a person who stated that he had seven years previously been persuaded to become an arsenic-eater; but the statement is unsupported. Dr. Kropsch, of Leoben, relying upon the assertion of another individual, reported a somewhat similar case coming within his own knowledge and observation. We pass over those unsupported cases reported to the Academy of Sciences at Vienna, as having been "related by trustworthy persons." Dr. Haller and others guarantee that they are acquainted with forty arsenic-eaters, including charcoal-burners, stablemen, field labourers, innkeepers, and tradesmen. These, however, as well as nine other instances quoted by Dr. Forscher, of Grätz, not being authenticated by personal knowledge or chemical analysis, cannot be allowed the slightest weight.

Dr. Roscoe has been supported in his views by Dr. E. Kopp,* who states that having been for six or seven months daily engaged in the use of arsenious acid for industrial purposes, experienced no ill effects, but observed a notable increase of his own weight, which rapidly disappeared on his discontinuance of his operations in the arsenious acid. Dr. Kopp quotes the researches of Schmidt and Steuerzwage,† to the effect that arsenious acid arrests the metamorphosis of tissues, and considers this hypothesis as explanatory of the accounts of the Styrian arsenic-eaters, which we would suggest must be established as facts before they can admit of explanation.

Having carefully considered the statements by Dr. Roscoe and Mr. Heisch, we cannot agree in their conclusions. So many marvels have been brought before the public, and have been credited upon the respectability of those who have pretended to investigate them, that we refuse to receive other than direct and positive evidence of any such alleged facts. Sufficient evidence, we submit, is wanting to the story of the Styrian arsenic-eaters. Some thirty years ago, a M. Chabert gulled the public into a belief that he could swallow Prussic-acid with impunity, as he was in possession of its antidote. The late Dr. Barry exploded the imposture by preparing some really strong acid, which he offered to Chabert, who, however, wisely declined to take it, as he "did not know how it was prepared!" He offered to give it to a dog! Dr. Halley, in a paper read before the

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* Ibid., Mai, 1861.

† Journ. für prakt. Chem., Band lxxviii. S. 373.

Royal Medico-Chirurgical Society, Nov. 13th, 1861, related the results of the administration of small doses of arsenic to dogs in order that he might observe the phenomena of chronic poisoning. These somewhat cruel experiments only still further proved that arsenic is a deadly poison, and cannot be taken with impunity.

If any of the believers in arsenic-eating will take the doses which they endeavour to make us believe are taken by the Styrian peasantry, then will we abide by the results. Until then, we conceive that we are justified in doubting their marvels, which remain as "unsupported by adequate testimony, inconsistent, improbable, and utterly incredible," as Mr. Kesteven showed them to be in 1856. The utmost that can be admitted is the possibility of the human system coming by long-continued use to endure the administration of doses of arsenic so small as to be within the limits of poisoning. This, however, is totally opposed to the arsenic-eating romances related by the writers now referred to.

Influence of arsenic on lower animals.—This has been exhibited in the fatal effects of arsenic, when used as a "sheepwash," as proved in courts of justice. Thus at Berwick, in August, 1858, it was sworn that 850 out of 869 sheep had been killed by its use. Dr. Balfour, of Edinburgh, has favoured us with exact information on this subject. A compound, known as "Biggs's dipping fluid," consisting of sulphur, arsenic, and grease, is employed by shepherds to destroy vermin in the fleeces. The effects on those who use the compound are œdema and inflammation of the skin of parts exposed to contact with the liquid; these are followed by pustules or vesicles, which dry off in the course of a few days, on discontinuing the use of the mixture. Dr. Crawford, of Peebles, states that the injurious consequences attending the use of arsenic for the purpose have become so well known, that the shepherds reluctantly and less frequently than formerly employ the "Biggs's liquid."

The following is the method followed in "sheep-washing:"—About eight or nine shepherds assemble at some convenient spot near a stream, where the sheep to be dipped are enclosed in a fold. The bath is an oblong box, about two feet wide, four feet deep at one end, shallowing upwards to an inclined plane with small spars nailed across the bottom, up which the animals walk dripping with the poisonous fluid, which again drains off the sheep into the bath. The sheep are handed one at a time from the fold to the dippers (three men, one at the head, and one on each side of the bath), who tumble the animal upside down; the man at the end takes hold of the horns or ears, those at the sides seize each two feet, and dipping the whole body, allow it to remain about a minute in the liquid, so as to soak it thoroughly. The sheep is then allowed to get on its feet and walk up the inclined plane on to the platform, draining off the liquid.

The bath is composed of about twelve or fourteen ounces of Biggs's dipping-liquid boiled in a large potful of water, and then poured into the bath half full of cold water. After every twenty sheep have been dipped, another potful of the poison is added, to make up for what has been absorbed in the sheep's fleeces.

What is termed "pouring" is effected by a decoction of tobacco, soft-soap, and arsenic, the sheep being held down on a stool, the liquid poured into the fleece, held apart along the side for the purpose, and thoroughly rubbed in by the shepherd. Six pourings of this kind are applied to each sheep, so that all vermin are effectually killed. The hands of the operators become inflamed and sore.

The hollowness and absurdity of the defence advanced in the case of Madeleine Smith, that she had employed arsenic as a cosmetic wash for her face, is exposed by all the preceding facts. We may further quote from Dr. Balfour the instances of a carpenter who caused inflammation of the scalp with papular eruption, through rubbing arsenic in his hair to destroy vermin; also of a bride and her maidens, who all suffered in a similar manner through the accidental use of white arsenic instead of hair-powder. We have ourselves met with a case where the health was seriously injured by following the example of the alleged Styrian arsenic-eaters.

An appeal in support of the attempt to prove that arsenic is not a deadly poison, has in some instances been made from the practice of grooms and others of giving arsenic to horses, with a view to improving the appearance of their coats. This practice is by no means harmless, but on the contrary has often proved fatal.[*] Dr. Kögler, veterinary surgeon to an Austrian cavalry regiment, states that the administration of one to two grains of arsenic daily with their corn gave a fuller aspect to the coats of the animals; but, he adds, this arises from a puffiness or œdematous state of the skin, and is followed by profuse sweating, emaciation, and death. Similar effects were observed by Dr. Halley in the course of his experiments. Symptoms of acute poisoning readily appear, if great caution and due order be not observed in giving the doses.[†]

Modes of testing for arsenic.—Although of itself presenting very striking and distinctive characteristics, arsenic, from its almost universal diffusion among earths and ores, presents oftentimes real and apparent obstacles to its detection in toxicological investigations, and affords occasion for forensic quibbling to throw doubts upon or suggest perplexity in the evidence of scientific witnesses. It will not be desirable that we should here repeat all the objections that may be made against the several methods of analysis, but shall limit ourselves to laying before our readers a brief abstract of the latest methods of analysis—viz., that by Dr. Taylor, in which is combined all the best features of Reinsch's and Marsh's processes, at the same time that the objections to which these modes are open have been obviated; that by Dr. Guy, consisting of a modification of the reduction process; and that by Mr. Bloxam, the application of electrolysis.

Having carefully followed the directions laid down by Dr. Taylor,[‡] and repeated his experiments, we feel confident in expressing our opinion that by observing the cautions which he enjoins against possible sources of fallacy or contamination in the processes described, these place within the reach of any one possessing merely a moderate extent of chemical knowledge, the means of arriving at satisfactory and reliable conclusions.

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[*] Morton: quoted in Pereira's Materia Medica, vol. i. p. 702; and Taylor on Poisons, pp. 89–91.

[†] Kesteven: Association Med. Jour., Sept. 1856. [‡] Guy's Hospital Reports, Oct. 1860,

The following constitutes the plan which Dr. Taylor, after many experiments, feels himself justified in recommending for the separation and detection of arsenic in liquids and solids, whether organic or inorganic.

"It is based, 1st, on the ready conversion of arsenic into *chloride;* 2nd, the conversion of this compound to *hydride* by Marsh's process; 3rd, the production of metallic arsenic, of arsenious and arsenic acids from the hydride; and 4th, testing the products. When once obtained in the state of chloride, the presence of arsenic admits of easy verification, either by resorting to the process of Marsh or that of Reinsch.

"First stage—*conversion to chloride.*—If the substance to be examined for arsenic is *solid,* it should be reduced to a fine powder, or cut into small pieces. In the analysis of the liver, spleen, or other soft organs, it will be proper to cut these into small portions, and to dry them either by a current of air or by the aid of a water-bath, so as to deprive them as much as possible of water. The complete elimination of the arsenic depends greatly on the perfect desiccation of the substance and the concentration of the acid.

"The substance, whatever may be its nature, having been brought to perfect dryness, is covered with concentrated hydrochloric acid, the purity of which has been previously tested, by passing its hydrogen through a solution of nitrate of silver" (see p. 44). "The quantity of acid employed should be sufficient to break up, dissolve, or mix freely with the whole of the solid. The mixture should be made in a retort or flask fitted with a condensing tube, and then gradually heated by a sand-bath, until the acid liquid begins to pass over. The retort or flask may be connected with a receiver closely fitting to it, and holding a small quantity of distilled water. The water in the receiver should be just sufficient to condense and fix the acid vapours. The receiver, as well as the condensing tube, should be kept cool by wetting its surface with cold water, or otherwise. The perfect condensation of the distilled liquid is ensured by this arrangement.

"The distillation may be carried to dryness on a sand-bath, or nearly so; and it may be sometimes advisable, in order to ensure the distillation of the whole of the arsenic as chloride, to add to the residue in the retort another portion of pure and concentrated hydrochloric acid, and again distil to dryness. Portions, however, of dried liver and stomach have been found to give up every trace of arsenic by one distillation, when a sufficient quantity of hydrochloric acid has been used in the first instance, and the distillation has been slowly conducted by a regulated sand-bath heat.

"Second stage—*conversion of chloride to hydride.*—The chloride should be immediately examined, or placed in a stoppered bottle to prevent loss by evaporation. One-third of the liquid should be preserved for the purpose of quantitative analysis, if such be required. The larger quantity should be placed in a flask, or Marsh's apparatus, fitted with a tube of a quarter of an inch bore, bent at a right angle, and extending for a foot or more, then bent down again at a right angle, so as to dip into liquids contained in tubes or glass vessels. The horizontal portion should be drawn out at three different places, until the diameter of the tube does not exceed the tenth of an inch. This allows of the collection of metallic deposits in a concentrated form, and on surfaces in which they may be easily examined and tested. Care must be taken to use glass free from lead. A piece of pure zinc should then be added to the mixture of chloride and hydrochloric acid; if the effervescence be too violent a little water may be added and the cork then fitted. The hydride of arsenic as it passes over should be received first into a tube containing about a drachm of moderately strong solution of nitrate of silver, which is blackened thereby, owing to the precipitation of metallic silver.

"Third stage—*production of arsenic and its oxygen compound.*—Heat being applied to the tube about a quarter of an inch before each contracted space, beginning with that which is nearest the flask. At a full red heat the hydride is decomposed, and the metallic arsenic deposited in a dark ring or crust on the interior of the glass at a short distance from the part that is heated. If the end of the tube be kept still immersed in the solution of nitrate of silver no arsenic will be lost. The tube with its deposits may then be removed, and another right-angled tube being rapidly substituted, the remaining gas should be allowed to exhaust itself in a tube containing strong nitric acid. The whole of the arsenic is arrested and converted into arsenic acid.

"Fourth stage—*testing the products.*—The conducting tube should be divided into three equal parts, the contracted end being sealed, and the metallic deposit heated; if it be arsenic, octohedral crystals will be formed in the cooler portion. The other metallic deposits, broken up and heated in a test-tube, will afford sufficient to be subjected to the ammonio-nitrate of silver, and ammonio-sulphate of copper tests. The *arsenious acid* left in solution from the decomposed nitrate of silver may be made evident by sulphuretted hydrogen. The *arsenic acid* obtained by the action of strong nitric acid on the hydride may be dried and tested by nitrate of silver, when the brick-red arseniate of silver will make itself visible.

"With these combined results," says Dr. Taylor, "it is conclusively proved that arsenic must be present in the substance analysed. We have here, in the production of metallic arsenic, and of its two compounds with oxygen, as well as in the conversion of these to sulphide of arsenic and arseniate of silver, the maximum of evidence which an analyst could desire. Beyond this it appears superfluous to carry the chemical proof."

Dr. Guy,* finding certain practical objections in the use of the ordinary reduction-tube, proposed the following ingenious plan. A tube, smaller in bore than the common "specimen-tube," and larger than the "reduction-tube," about three-quarters of an inch long, is supported in a vertical position by being dropped into a hole punched in a piece of copper-foil or brass. Into this short tube the dried powder or mixture is dropped. The tube thus suspended is held by the left hand, with its closed end in the outer flame of a spirit-lamp, while a piece of dry microscopic glass, large enough to cover the mouth of the tube, is adjusted over it with the right hand. After a few seconds the glass is covered with a circle of sparkling crystals, or a distinct metallic crust, either of them in a state most favourable for further examination.

Dr. Guy considers that this process presents special advantages when dealing with small quantities; nevertheless we have ourselves found in some cases that when minute quantities of the poison have been used the crystals deposited have been too minute, or too irregular in form, to be identified, unless by the highest powers of the microscope. Moreover, the thin glass cover being loosely applied is apt to slip off in the course of the experiment, in which case the fumes are entirely lost in the air. As the metal itself sublimes at a few degrees higher than the acid, the continuance of the heat rapidly covers glass and crystals with a coating of the metal.

The same end may be attained (as was suggested by a writer in the 'Chemical News,' March 31st, 1860) by affixing the microscopic glass to a glass-holder or platinum wire, and passing it down a common test-tube; the fumes that escape the glass would then be condensed on the upper cold portion of the tube.

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* Beale's Archives of Medicine, No. 3, p. 255. 1858.

The application of electrolysis to the detection of arsenic suggested itself to Mr. Bloxam as a means of avoiding the objections to Marsh's and Reinsch's tests.

> "The apparatus which was at first employed, consisted of an ordinary U-tube, one limb of which was closed with a perforated cork, through which passed a tube for the escape of hydrogen, and a platinum wire connected with the zinc extremity of a Groves's battery of five cells; to this wire was attached a platinum plate, measuring about two inches by three-quarters of an inch, which was thrust down almost to the bottom of the U-tube. The other limb of the tube was left open for the escape of oxygen, and contained a similar platinum plate connected with the platinum extremity of the battery. The tube which carried off the hydrogen was connected with a straight tube of hard glass drawn out to a long open point, and heated to redness at the shoulder, in order that any arseniuretted hydrogen might be decomposed in passing through it."

The experiment was performed by placing dilute sulphuric acid in the tube, and adding thereto the solution to be examined for arsenic, the presence or absence of which is ascertained by the application of heat to the drawn-out tube, with a view to the formation of a mirror of arsenic within it. Certain modifications of this apparatus have subsequently been made by Mr. Bloxam, the principle, however, remaining intact.

The advantages possessed by this method, Mr. Bloxam adds, are, that it involves the use of a metal which has never been known to contain arsenic; that the same portion of sulphuric acid may be subjected to the test before the suspected liquid is added; that the evolution of gas is uniform, and always so slow that no dread of losing the arsenic need assail the mind of the operator; that the experiment may be interrupted for any length of time without injury by breaking contact with the battery; that the foulest liquids can be as readily tested as those which are perfectly clear; and that the same portion may be further tested by any other process.

The process here described avails itself of the properties of arseniuretted hydrogen, but in a less ready manner than in the last stage of the process adopted by Dr. Taylor. The organic matters must first be digested in hydrochloric acid before a fluid is obtained for exposure to the electric action; during this boiling, some of the arsenic may go over as chloride, this being a volatile compound. In Dr. Taylor's plan the distillation of the chloride of arsenic direct from the organic matters is the first step; the next step is the formation of the arseniuretted hydrogen, in a mode in which it is readily available for the production of the metallic mirror, and the arsenious and arsenic acids under favourable conditions for the application of their respective tests. It is true that the hydrogen disengaged from Mr. Bloxam's U-tube is also available for like purposes, but is not so rapidly or abundantly given off, and a Groves's battery furthermore is not always at hand, while the simpler apparatus required for Dr. Taylor's process may almost always be extemporized.

The editor of the 'Chemical News'* gives a simple and ready method by which the presence of arseniate of copper in paper-hangings, or any other fabric, may immediately be recognised. A small portion of the suspected object is to be immersed in strong ammonia in a test-tube. If a blue colour be communicated to the ammonia, the presence of a salt of copper is proved. The paper or other object should then be withdrawn, and a fragment, or crystal of nitrate of silver be dropped into the ammonia; if arsenic be present, the nitrate of silver will be covered with a yellow coating of arseniate of silver, which will disappear on stirring. For the medical practitioner who has not the time, or probably the means at hand, for more accurate analysis, the means are here presented for the immediate detection of the presence of the poison.

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* Chemical News, vol. i. p. 12.

REVIEW IX.

The Forms, Complications, Causes, Prevention, and Treatment of Consumption and Bronchitis; comprising also the Causes and Prevention of Scrofula. By JAMES COPLAND, M.D., F.R.S., F.R.C.P., &c. &c.—*London*, 1861.

WHEN an author of Dr. Copland's standing and age solicits professional attention, by the publication of a book on one of the most important subjects in medical science, we believe it would be unfair to estimate his labours by the same rules and standard of criticism we should think it imperative to apply in reviewing the work of a writer fresh from the newest school of medical philosophy. We have no right to expect from a physician who has been engaged in the actual practice of his profession for well-nigh half a century a parade of the last-broached theories, or an exhibition of the latest importations from the fields of chemical, pathological, or microscopical science into the domain of practical medicine. Unquestionably, if we seek in the work before us a *résumé* of all that modern investigation has brought to light respecting the pathology of pulmonary consumption, we shall be disappointed; but we are not warranted on that account in dismissing the book as worthless, or in indulging in ill-natured comparisons at the author's expense. We may at least expect to find in his treatise something that may prove of practical value, and whatever exception may be urged to his conclusions, it must be allowed that they are the result of a lengthened experience such as few physicians have enjoyed, and of an acquaintance with the archives of medicine such as few authors can boast.

One of the principal charges to which Dr. Copland's treatise lies open, is that of unduly depreciating the advantages which practical medicine has derived from the introduction and improvement of the art of physical diagnosis. This is scarcely to be excused, for the change has taken place in the author's own time, and its results must have constantly forced themselves upon his observation. The course of his professional career has witnessed doubt replaced by comparative certainty, and a flood of light admitted to dissipate the obscurity of surmise and conjecture. The value of auscultation and percussion, and their kindred arts, has now been tested by more than one generation of physicians, whilst with each succeeding year the indications they afford have been received with greater confidence, and their cultivation has been more zealously prosecuted. The diseases of no organs of the body are so thoroughly understood and we believe so scientifically treated, as those of the heart and lungs; whilst before the sense of hearing was made subservient to diagnosis, their recognition, and consequently their rational treatment, were involved in the same uncertainty with morbid affections of the nervous centres and of the abdominal viscera. If proof be necessary to support facts so generally acknowledged, we need only refer to the author's historical sketch of the treatment of phthisis, in which he himself ascribes the contradictory conclusions of the older writers on therapeutics to the fact, that up to the present century chronic and acute bronchitis were constantly confounded with and treated as pulmonary tuberculosis. The author is open to still graver animadversion, when he condescends to impute sordid motives of personal advantage to practitioners who, fully persuaded of the superiority of diagnosis based on the observance of physical signs, omit the requisite examination in no suspected case. In thus ignobly quitting the arena of scientific discussion, he forsakes the vantage which in argument his high and well-earned reputation might afford him, and places a powerful weapon in the hands of opponents. We regret that such passages as the following should occur in the opening paragraphs of his work:

> "Manipulations which strike the senses of the attendants, and more than one sense of the patient—examinations which may be seen, felt, and talked about, have a much more impressive and lasting influence upon the patient and spectators, than the close observation of symptoms and the pertinent inquiries of the profound and comprehensive thinker. The former are lights which the possessor places upon an eminence for his own advantages; the latter are intended entirely to benefit the person for whose safety they are employed." (p. 3.)

And again:

> "Too great dependence upon, and a too *ad captandum* parade of this mode of diagnosis, sometimes even with the fussiness and flourishes of vulgar craft, have tended to the neglect of those states of vital manifestation, of disordered functions, and of vascular action, which, whilst they indicate incipient or early pulmonary disease, also characterize its forms, and point to the changes in which those forms originate, and on which they continue more or less to depend." (pp. 3–4.)

Passages containing imputations of a similar kind occur more than once in the course of the work. We are told of the "fuss, parade, manipulation, and charlatanry of a physical examination of the bare chest," and a certain class of practitioners are satirized as "those who have adopted diseases of the lungs as their speciality, the examination of the bared chest for the grand *coup* of fussy diagnosis, and the stethoscope as the baton of transcendental medical knowledge, if not of actual inspiration." Many of our readers may think—and we are disposed to agree with them—that here and there an adept in auscultation and percussion has erred in relying too exclusively on the information they are capable of affording; but the great fact still remains, that during the short era of their cultivation a more certain diagnosis in cardiac and pulmonary diseases has been established, than was attained by the accumulated labours and observations of previous centuries. Dr. Copland is of course justified in an expression of his opinion, but as an opinion its validity is to be tested solely by the facts of the case. The alliance with sarcasm and invective confers no force on scientific argument.

As the mode of detecting disease by physical examination has evidently failed to obtain from the author more than a very limited confidence, our readers will be prepared to learn that the portion of the work devoted to a description of auscultatory signs is entirely devoid of original observation, meagre in detail, and in many particulars absolutely deficient. In fact, the description of physical signs in the first part of the book is either confessedly quoted, or copied with slight verbal alteration, from the section on tubercle in Dr. Markham's translation of Skoda. There is not a positive original observation, and scarcely a sentence couched in the author's own language on the physical diagnosis of consumption in the whole book. In the description of phenomena accompanying the first stage, we look in vain for any notice of alterations in the vocal fremitus, in the rhythm of the inspiratory murmur, for any reference to the dry, crackling, rhonchal sound which is peculiar to this period of the disease, and for any observations on the information which the arterial and cardiac sounds are capable of affording. These, it may be said, are minor points, and a diagnosis is not to be founded on them separately. We quite admit it, but we should have preferred seeing them noticed by the author, were it merely to prove that he is fully acquainted with the indications furnished by the method of investigation which he has undertaken to criticise. Again, not one word is said of the signs by which perforation of the pleura may be recognised. Indeed, the reader who may be seeking information respecting the symptoms and signs which accompany this somewhat important complication of phthisis, is only furnished with a reference to the articles Pleura and Pneumatothorax in the author's work on Practical Medicine.

The symptoms by which Dr. Copland believes the ordinary form of pulmonary consumption may be diagnosed with a higher certainty than by the revelations of the stethoscope, are, increased frequency of respiration in the incipient stage, diminished capacity of the lungs for air as determined by Hutchinson's spirometer, hæmoptysis, cough, expectoration, pain, the condition of the pulse, hectic fever, impairment of the digestive functions, emaciation, clubbing of the fingers, œdema of the extremities, morning perspirations, aphthæ, and a falling off of the hair. Although the chapter devoted to a consideration of these phenomena is of considerable length, we cannot say we have derived much information from its careful perusal. The statements made, as far as they extend beyond an ordinary compilation of the current knowledge, appear either too vague, or of too debateable a character, to form a guide in diagnosis. Phthisis may doubtless be recognised when such a group of phenomena present themselves, but not one of the indications described is absolutely pathognomonic. With regard to one—clubbing of the fingers and incurvation of the nails—Dr.

Copland has been led into manifest error in assigning to it undue diagnostic import. "This appearance is most remarkable when emaciation exists, and with the symptoms just mentioned, *or even alone*, is a most unerring sign of tuberculous phthisis." (p. 54.)

We hardly expected to find such an assertion in the work of a physician of such experience, and so largely acquainted with professional literature. We thought that this symptom was universally allowed to occur in all chronic diseases in which the transmission of blood through the lungs and its due aeration become impeded. Its occurrence in many conditions, amongst which may be mentioned empyema, cyanosis, and aneurysm, has been frequently noticed, and although the majority of cases in which the change in question presents itself are decidedly phthisical, there can be no doubt that, taken by itself, it is merely an indication of prolonged interference with the pulmonary function. Of the above enumerated category of symptoms, hæmoptysis and the evidence furnished by the expectoration, are certainly of the highest value. But the most decided proof afforded by the latter is only to be obtained by a microscopical examination, and to this no allusion is made. On the discovery of elastic lung-fibre in the sputum we should certainly place a higher reliance than on the appearances in the early stages described as characteristic. Hæmoptysis, if cases of heart-disease and of cancer of the lung be excluded, is almost, if not quite pathognomonic of tubercle. The first class of exceptions has received due notice from the author; to the second he does not refer. We are not prepared to affirm that hæmoptysis cannot occur except in cases of tubercle, cardiac disease, and pulmonary cancer, but we hold that its occurrence under other conditions, such as local plethora of the lung, dependent or not on suppression of the catamenia or of other sanguineous discharges, is excessively rare. The author believes with Andral, that the effused blood, if not excreted, may, by infiltrating the air-cells and pulmonary tissue, form a nidus for fresh tubercle, but he is also of opinion that its presence, by irritating the lining membrane of the minute bronchi, not infrequently proves the exciting cause of pneumonia.

> "One of the most common consequences of hæmorrhage into the bronchi is inflammatory action. The effused blood irritates the mucous membrane of the bronchi, especially in the minute ramifications, and the morbid action often extends to the air-cells and substance of the lungs. This is very frequently observed in weak and susceptible constitutions, and when the effused blood has been imperfectly excreted from the bronchi. The softening and discoloration of the bronchial surface, generally seen in fatal cases of hæmoptysis, arise from this consecutive inflammatory irritation, and the puriform matter sometimes poured into the bronchi, with or without fibrinous concretions, or a coloured lymph, proceed from the same source. A part, doubtless, of the fibrinous matters arises from the effused fluid; but a part also consists of the lymph given out by the capillaries which had shortly before discharged blood. In all cases of hæmoptysis, it is not merely the development or accelerated progress of tubercle which is to be dreaded, but also the supervention of circumscribed or diffused pneumonia, which may assume either sthenic or asthenic forms." (p. 39.)

We should have been glad to find that these statements were supported by the detail of carefully observed facts, as we are not disposed to think them in accordance with ordinary clinical experience or anatomical observation. As we do not deny the possibility of sanguineous effusion, or of any effusion by retrogressive changes degenerating into tubercle, neither do we affirm that the presence of coagulated blood in the air-passages may not determine inflammatory action, but, clinically speaking, we believe that either of these occurrences is exceptional.

The author's predilection for vital semeiology, as the basis of diagnosis and prognosis, has led him to attach specific importance to indications which at the best must be allowed to be extremely vague and uncertain. Thus some score of phenomena, principally of the sensational class, are erected into a group of premonitory symptoms of hæmoptysis. The enumeration we allude to commences with "chills or horripilations of the general surface," various sensations in the different regions of the chest and other parts of the body are then specified, together with dyspnœa and cough, and the catalogue terminates with "borborygmi, costiveness, and pale urine." If these symptoms, apart from any information to be obtained by a physical examination, furnish reasonable ground for predicting the supervention of hæmoptysis, at least one-half of the female out-patients at our metropolitan hospitals are in immediate danger of bleeding from the lungs. Again, considerable stress is laid upon the appearance of the eyes as a diagnostic sign of early phthisis, the disease being supposed to be indicated by dilatation of the pupils and a pearly hue of the conjunctivæ—conditions which are certainly frequently absent in well-marked cases, although as often to be noticed in young persons who show no other evidence of tubercle.

Under the head of acute phthisis, Dr. Copland describes a rapid form of the disease which, in the symptoms it presents, closely simulates typhoïd fever. He is under the impression that this form of acute pulmonary tuberculosis has hitherto escaped the notice of those writers who have paid particular attention to thoracic affections. Now, it so happens that there are few points connected with the subject to which professional attention has been more carefully directed of late years than the occasional occurrence of tubercular disease of the lungs under a form which may be easily mistaken for the fever in question. In such cases the disease is found after death to have involved both lungs throughout the lower as well as the upper lobes, the pathological changes being either a studding of the entire pulmonary tissue with grey granulations, or a general infiltration of the pulmonary lobules with crude tubercle, which here and there may be found to have undergone softening and excavation. The acute miliary and acute softening forms of the disease may occasionally be found co-existing in the same subject. Acute tuberculization, with ulceration of the small intestines, together with enlargement of the spleen, are frequently present, and render in some cases the distinction from typhoïd fever a matter of no small difficulty. The adynamia, abdominal symptoms, and the low form of delirium which is a frequent concomitant, may mask the chest symptoms, or may lead to the condition being mistaken for typhoïd fever complicated with pneumonia. The non-existence of the papular typhoïd eruption is one of the most certain points of distinction, but cases of acute phthisis have been observed which have presented here and there a speck on the abdomen, having many of the characters of the typhoïd rash. Now, the diagnosis between this form of rapid pulmonary tuberculization and typhoïd fever has been discussed with considerable minuteness by several writers, amongst whom are Walshe and Louis; and on careful comparison of the symptoms and post-mortem appearances detailed by Dr. Copland, and those observed by the above-named authors, we fail to find any ground for specific distinction. The author insists particularly upon the trifling character of the cough and the scantiness of expectoration, in the cases which have come under his notice; but the absence of expectoration in some instances, in others its clear or opalescent character, and the rarity of viscid sputa, have been noticed by Walshe, who also observes that cough may only follow the fever in the order of development. Again, in three of the cases of acute phthisis detailed by Louis, the amount of expectoration was trifling, and in one instance the entire absence of both cough and expectoration in the initial stage is especially noticed by him. In every other essential indication, as well as in the post-mortem appearances, the form of phthisis simulating low fever described by Dr. Copland is the same with that which has been for years fully recognised.

The sections on etiology and treatment embody the results of an extended acquaintance with ancient and comparatively recent medical literature, together with the fruits of the author's personal observation and experience. The consideration of treatment is prefaced by an historical introduction which fully upholds the reputation for erudition which Dr. Copland enjoys. We doubt, however, whether many readers will agree with the conclusion at which he arrives as to the relative position of past and present methods of treating phthisis. He is of opinion that, with the single exception of the introduction and use of cod-liver oil, the treatment of pulmonary consumption in the present day is nearly the same as that advised by the best medical writers of the seventeenth and eighteenth centuries, and more particularly by Bennet and Rush. The following is a sketch of the treatment recommended by the former physician—

> "For hæmoptysis leading to phthisis, he advises bleeding, warmth to the extremities, and bleeding from the feet in females, if the catamenia be scanty or suppressed. He recommends milk and milk diet, but prefers medicated whey, and reprobates the use of saccharine substances, as productive of an injurious fermentation. He considers the best expectorants to be those which contain resin and turpentine. Bennet also has recourse to frictions and fomentations, and to balsamic fumigations. These last should consist, in his opinion, of frankincense, turpentine, and styrax, with cinnamon, coltsfoot, and other articles, made into a powder or troche, and burnt on coals. He prescribes also mixtures of herbs, on which boiling water is poured, and the vapour to be inhaled by holding the head over the vessel containing them. Issues are much praised, and, according to my experience, with great justice. He directs them in various situations, according to the symptoms, and he considers that they may be kept sweet by using peas of orris root, and when the discharge should be promoted, he advises equal parts of hermodactyls and wax. He recommends Welsh flannel to be worn next to the skin, and not

to be too frequently changed. Animal food, neither very fat nor lean, is allowed, and a gentle emetic is given when the stomach is loaded; and a decoction of sarsaparilla and other woods, with ginseng, is recommended for drink. If we except the recent employment of cod-liver oil in phthisis, in what, it may be asked, has the treatment of this disease been advanced since the appearance of the work of Bennet, by the voluminous writings of specialists and stethoscopists in recent times?"—pp. 191, 192.

To the question thus triumphantly put, it may be answered, first, that bleeding for hæmoptysis is not a practice which meets with much favour in the present day. Most living physicians with whose opinions we are conversant, would as soon think of drawing blood to arrest uterine hæmorrhage, or to stop a wounded artery; and, secondly, that the main improvement depends on the universal acceptation of the doctrine, that phthisis is essentially a disease of imperfect nutrition and assimilation, and that efforts in treatment must therefore be mainly directed, not to the pulmonary, but to the digestive systems. The principal treatment of phthisis now-a-days consists in the free administration of nourishing animalized diet, containing a large proportion of the fatty elements of nutrition; in exercise, with unrestricted exposure to the invigorating influences of sun and air; and in the substitution of a few simple but effective tonic medicines for the effete polypharmacy of our predecessors. Doubtless an analeptic method was occasionally advocated by physicians of a past age, although its supporters have been in the minority; but even their advocacy was imperfect, because founded on no clear view of the pathology of tuberculization.

Constantly haunted by a dread of the supervention of inflammatory action, they were ever and anon recurring to small bleedings or other antiphlogistic measures; whilst the principal part of their ordinary treatment consisted in the exhibition of expectorants, sedatives, counter-irritants, and derivatives. We by no means deny the use of the latter-named classes of remedies in relieving symptoms, but every country apothecary is now aware that such remedies are mere palliatives, and that they do far more harm than good when they are permitted to interfere with true reparative measures. Were it needful to say one word respecting the fumigations recommended by the physician thus eulogized, and as used in olden times, we might stop to prove that some advance has been effected in medicine by the progress of chemistry and physiology. We now know that a man deprived of a portion of his lungs is virtually in the position of one whose supply of oxygen has been in part cut off; and that, were there no other objection to burning a farrago of drugs constantly in his apartment, by the very combustion itself his supply of the life-giving element would be still further diminished.

On turning to the treatment recommended by the author, we have certainly been surprised to find local and in some instances general bloodletting still advised in the first stage of phthisis; but the advice is surrounded with so many cautions and exceptions, that we feel convinced the cases in which he in practice would prescribe it must be very few and far between. In most other respects his treatment is of a reparative character, although occasionally allowing too great deviation, as it appears to us, in an opposite direction. The following are amongst the results of Dr. Copland's personal observation. An inland locality is to be preferred to a residence by the sea, provided the former offers equal advantages as to dryness of air, annual, monthly, and daily changes of temperature and vicissitudes of weather, together with facilities for out-door exercise. During the early stages and in the winter months, sea voyages, especially in the Mediterranean, Pacific, and Atlantic Oceans, between latitudes 10° and 30°, are recommended. Under the head of residence, the value of a dry atmosphere and of elevation above the level of the sea, is insisted on, although the assertion that inhabitants of marshy districts, where ague is endemic, are singularly free from pulmonary tuberculosis, is to a certain extent confirmed by the author's experience. The influence of various localities, both in this country and abroad, is discussed at considerable length, and the chapter on this subject will afford a valuable fund of useful information to the practitioner. In respect of purely medical treatment, great stress is laid on external medication, especially on the employment of issues and setons in the earlier stages, and on the continued application of terebinthinate liniments, the good effects of which Dr. Copland believes to depend partly upon inhalation of the vapour. The internal administration of turpentine is spoken of as a most valuable remedy in hæmoptysis; in this we most fully concur, although we cannot assent to the propriety of even the small bloodlettings "to arrest hæmorrhage and remove local congestion," which are also recommended. Cod-liver oil is stated to be the remedy on which most reliance may be placed to diminish the excessive perspirations which accompany the softening stage, and the administration for the same purpose of Griffiths's mixture, or of the muriated tincture of iron in twenty-minim doses, as recommended by Dr. Watson, is spoken of with approbation. Amongst the older medicines which have fallen into comparative disuse, sulphur has appeared to the author to be occasionally productive of benefit; he does not, however, particularize the conditions under which its exhibition is likely to prove advantageous. The sulphureous waters of Harrowgate are recommended in scrofula, glandular affections, and the earlier stages of phthisis, the curative influence of the waters being favoured by what are considered the advantages of a high, dry, temperate, and inland locality.

The latter portions of the work are allotted to phthisical affections of the larynx and trachea, and to a treatise on the various forms of bronchitis. Our space, however, is already exhausted. It only remains to observe that the expectations with which we commenced our review of Dr. Copland's labours have been in most respects fulfilled. Although we have ventured to disagree with many of the opinions advanced, and however we may regret the tone in which some of those opinions are enunciated, whilst, moreover, we are fully aware of omissions, some of which—as, for instance, the neglect of all reference to the influence of pregnancy on phthisis—constitute serious defects, we must yet allow to the treatise a certain value as a learned and laborious compilation, and as at the same time embodying the individual experience of the author.

PART SECOND.

Bibliographical Record.

Art. I.—*An Introduction to Practical Chemistry, including Analysis.* By J. E. Bowman, F.C.S., late Professor of Practical Chemistry in King's College, London. Edited by C. L. Bloxam, Professor of Practical Chemistry in King's College, London; Lecturer on Chemistry in the Royal Military Academy, Woolwich. Fourth Edition.—*London*, 1861.

A new issue of this well-planned and well-written book has lately appeared; the subjects treated of are explained in such simple yet elegant language, that the work has deservedly become a general favourite. Having pointed out the chief features of the author's plan in noticing former editions, we may now content ourselves with a few words as to the difference between the present and the last (1858) edition.

The editor announces in the new preface several alterations and improvements, especially in the course of qualitative analysis, the use of the blowpipe, and the examples of volumetric analysis.

Chapters I. to IV. of Part I. remain almost unaltered, and in the early part of Chapter V. we find no change. But at the end of Chapter VI. an excellent "Systematic Examination of Unknown Substances with the Blowpipe," is detailed; it proves, however, on closer inspection, to be nothing more than a part of what is generally termed the "Preliminary Examination." Section 147, p. 52, now seems obsolete in great measure, owing to the improved photo-chemical methods of Cartmell and Bunsen, a simple and brief notice of which might very well be here introduced.

One or two additions have been made to the chapter on alkalimetry and acidimetry, including the determination of iron by means of permanganate of potash. Might not an example of quantitative gas-analysis have been given here? such, for instance, as Pettenkofer's beautiful and accurate process for estimating carbonic acid in the air —a process which any student taking moderate care can perform successfully.

In Part II. the reactions of the bases are described under the metals, and the reactions of the acids under the non-metallic bodies. Although beyond this we do not see any very striking alteration in the course of qualitative analysis, yet it is evident that this part of the volume has been very carefully revised. Most of the changes made are decidedly for the better, and tend to facilitate the progress of the student. Among these improvements, a place might advantageously have been found for the following special tests of tried excellence, most of which are now in common use in the laboratory:

Binoxide of lead with nitric acid, as a test for manganese.

Binoxide of lead with hydrate of potassium, as a test for chromium in the state of sesquioxide.

Nitrite of potassium, as a test for cobalt.

Sulphate of strontium, to discriminate between barium and strontium.

Disulphate of copper as a test for iodine.

Potassio-iodide of mercury as a test for minute traces of ammonia.

In the case, too, of molybdate of ammonium, a meagre foot-note on p. 122 is all the information given, and does not describe the way of applying the test for the detection of phosphoric acid. Chloride of palladium as a test for iodine is referred to with similar brevity on p. 126. In the examination for acids, the further analysis of the precipitate produced by nitrate of silver is not given with a fulness sufficient to ensure satisfactory results.

In the table of the more important elements given on page 2, we think that lithium, cadmium, molybdenum, and tungsten might be added with benefit, since several compounds of these metals are now manufactured, and have become comparatively common.

We note a few additions in the remainder of the volume: for instance, some fresh examples of quantitative analyses are given. One or two of the more modern and more perfect forms of gas-burners and gas-blowpipes devised by Griffin and others might have been substituted for those represented on pp. 214 and 215; Bunsen's burner, in some of its numerous forms the most useful of all, is dismissed in a foot-note of one line.

But the merits of the book are so conspicuous, and the faults so trivial, that we may be excused for pointing out the defects which seem to us to mar, though slightly, the general perfection of this sound and useful work.

ART. II.—*Die Formen des Beckens, insbesondere des engen weiblichen Beckens, nebst einem Anhange über die Osteomalacie.* Von Dr. CARL CONRAD TH. LITZMANN.—*Berlin*, 1861.

The Forms of Pelvis, especially of the Narrow Female Pelvis; with an Appendix on Osteomalacia. By DR. C. C. TH. LITZMANN.

PROFESSOR LITZMANN, the distinguished Professor of Medicine and Midwifery, and Director of the Lying-in Institution of the University of Kiel, has contributed in this work an elaborate and most useful account of the varieties of that formidable obstetric difficulty, the narrow pelvis. His descriptions are based upon extensive personal inquiries, and undoubtedly constitute the most systematic and accurate account of the subject hitherto published. Inverting somewhat the order followed in the book, we find a most interesting historical review of the steps by which our actual knowledge of the narrow pelvis has been developed. He says that attention was first directed to the existence of narrow pelvis as an obstacle to labour in the latter half of the sixteenth century. Previously to that date, and indeed for long after, the notion prevailed that the pelvis was always and naturally too small for the passage of the child, and that birth was effected by the efforts of the child itself, acting perhaps with other forces in producing a violent expansion of the bony canal of the pelvis by separating its joints. Vesalius, by an accurate description of the normal pelvis, prepared the way for a better theory of labour; he denied the separation of the pubic bones during labour. Accurate ideas were gradually established through the researches of Arantius, Scipio Mercurio, Mauriceau, Durnter, De la Motte, Dionis, and of Smellie, to whom our author assigns a foremost place; of G. W. Stein, of Denman, de Fremery, Stein the younger, who taught and extended the views of Smellie; of Naegele, who described the generally-contracted pelvis and the obliquely-narrowed pelvis; of Rokitansky, who traced the influence which hip-disease exerted on the shape of the pelvis; of Betschler and Michaelis, who described the flat non-rachitic pelvis; of Robert, who described the transversely-narrowed pelvis; of Kilian, who added so much to our knowledge of the osteomalacic pelvis; and of Rokitansky again, who with Kiwisch pointed out that form of distortion which is the result of the sliding down of the lumbar vertebræ.

The materials thus accumulated and enlarged by his own investigations, Litzmann has classified into a systematic account of the entire subject.

Adverting to the frequency of narrow pelvis, he says, having measured the pelves of 1000 women admitted at Kiel under his care, he found that in 14·9 per cent. the conjugate diameter did not exceed 3–5″, and that in 1000 women admitted under Michaelis, his predecessor, 13·1 per cent. had narrow pelvis. This is a subject upon which our positive information is exceedingly scanty. We may observe that the possible variations in the frequency and degrees of narrowness in different countries may account largely for the great diversities found in the application of instruments and in the mortality of lying-in women.

In a distinct chapter the author discusses the variations in the form of the pelvis; and the causes which determine the form, under the heads of—1, original disposition, development, and growth; 2, pressure of the weight of the trunk; 3, the resistance of the pelvic bones and cartilages; 4, the dragging and pressure of the muscles attached to the pelvis. The influences of sex and age are carefully considered.

He divides narrow pelves into two leading groups: those in which there is general narrowing without deviation from the normal form, and those in which narrowness is combined with deformity. Pelves of the first group, although not so rare as is thought, are of less practical importance than are those of the second. These latter he classifies as follows:—First.. The flat pelvis, which is of two kinds—*a*, the simple flat pelvis; *b*, the simple rickety flat pelvis. The common character of both is flattening of the pelvis from before backwards, shortening of the antero-posterior diameter, especially at the brim. The first kind was first noticed by Betschler and Michaelis. Litzmann's description is based upon four specimens. At first sight, these pelves would hardly appear shortened. The conjugate diameter is found to be from seven lines to an inch under the normal length. The texture of the bones is healthy. The basis of the sacrum is wider than in the normal pelvis; there is excess of development transversely. In the rachitic flat pelvis, all the parts are below the normal size; the sacrum is driven down between the innominata, with its base hanging more over the cavity. The tubera ischii also project more backwards beyond the posterior surface of the sacrum than in the normal or in the flat non-rachitic pelves. The brim is sometimes cross-elliptic, kidney, broad-hearted or flat, three-cornered in shape. Towards the outlet the pelvic walls diverge. A certain asymmetry is proper to these pelves. The variations in the relative proportions of the several diameters are minutely given.

The *generally-narrowed flat pelvis* is conjectured to arise from the supervention of rickets in a generally-narrowed pelvis, and too early use of the lower extremities. Compared with the simple flat rachitic pelvis, it is observed to have a finer and more graceful build; to be excessively flattened, with simultaneous narrowing in the transverse direction, with a pointing forwards, resembling a transition into the form of the osteomalacic pelvis.

A second variety is the transversely-narrowed pelvis. Amongst its characters are unusual narrowness of the sacrum, together with an irregular expansion of the ossa innominata, whereby the whole pelvis is narrowed in the transverse direction. In the higher grades the sacrum has always been found amalgamated with the innominata.

A third kind is the obliquely-distorted pelvis. In this a persistent pressure from above towards one side of the pelvis has caused a twisting in an oblique direction. The conditions under which this happens are: lateral spinal curvatures, mostly of rachitic origin; increased or entire use of one leg, from coxalgia, amputation, or luxation of the other upwards and backwards; the higher grades of asymmetry of sacrum from imperfect development of the sacrum on one side, as from amalgamation of the sacrum with the os innominatum in early life, or from caries. To these causes may be added shortening of one leg from fracture, as in the case of asymmetrical deformity described by Dr. Barnes in the 'Obstetrical Transactions for 1860.'

Distinguished from the preceding is the pelvis obliquely twisted in consequence of extreme asymmetry of the sacrum. In this form there is primary defective formation or development of the sacrum on one side, and secondary twisting of the pelvis and ankylosis of the sacrum with the os innominatum; or amalgamation of the sacrum with the os innominatum in early life; and hence arrested growth of the amalgamated parts, causing asymmetry of the sacrum and secondary twisting of the pelvis; or asymmetry of the sacrum from loss of substance by caries in the ilio-sacral joint of one side, with secondary twisting of the pelvis, and termination of the caries in ankylosis. The fourth kind is the compressed (*zusammengeknickte*) pelvis. Of this there are two varieties—one (the most frequent) the osteomalacic pelvis, the other (rare) the rachitic.

The Appendix on Osteomalacia is also a very valuable addition to obstetric literature. There is a very complete bibliography of the subject. The author relates a very interesting case which came under his own observation, and concludes with a critical analysis of the cases recorded.

The descriptions of the pelvis are illustrated by several excellent drawings. The work, in short, deserves to arrest the attention of teachers and practitioners of midwifery, who may find in it much precise information upon a subject which is very imperfectly handled in our text-books.

ART. III.—*Medical Jurisprudence.* By ALFRED SWAINE TAYLOR, M.D., F.R.C.P., F.R.S., &c. &c. Seventh Edition.—*London*, 1861. pp. 947.

THIS manual is too well known to require more than simple mention. The fact that fifteen thousand seven hundred and fifty copies have been printed in seventeen years is proof sufficient of the extent of its circulation and of the estimation in which it is held. We must not, however, pass over one important improvement in the present edition—viz., that the author has omitted controversial matters arising out of certain trials that have occurred within the last few years. These topics, which occupied a considerable space in the preceding edition, are replaced by valuable new matter of an entirely practical nature, calculated still further to enhance the value of the work and to raise the character of the author, which already stood so high.

ART. IV.—*Hints on Insanity.* By JOHN MILLAR, L.R.C.P. Edin., Medical Superintendent Bethnall House Asylum, London.—*London*, 1861. pp. 105.

THE little book before us is designed to meet wants and deficiencies which no one attempts to conceal, and which the requirements of the law in respect of lunacy make abundantly manifest. From his large experience, Dr. Millar has furnished us with some very valuable and useful "hints," which may tend, we hope, to the more correct filling-up of lunacy certificates, and a more general acquaintance with a malady which appears to be on the increase. In fact, Dr. Millar's book is just the sort of book which every practitioner ought to have. True it is that the author's style is not strikingly lucid, and his sentences are not always the most correct English, yet the general arrangement of the manual is admirable, the facts pertinent, and the illustrations apposite. These "Hints" will enable the "non-alienist" to meet the pressing emergencies which are sometimes so embarrassing to the inexperienced, and always so alarming to the friends and family of the patient.

From among the various practical conclusions adverted to in this little book, we may notice that Dr. Millar has "long ceased to regard" setons in the back of the neck as of any value in this frightful malady, and that he esteems dietetic regulations and physical exercise as of primary importance in controlling the severity of epileptic seizures.

ART. V.—*Om de Pathologiska Hufvudmomenten af Allmän Paresis eller Förlamande Sinnessjukdom* (*Paralysie Générale*). Af Dr. E. SALOMON.—*Upsala*, 1861. 8vo, pp. 30.

On the Pathological Elements of General Paresis, or Insanity with Paralysis. By Dr. E. SALOMON.

IN the above-named little work, Dr. Salomon, whose position as Medical Superintendent of the Malmö County Asylum for the Insane in Sweden, containing 180 inmates, peculiarly fits him for the task, presents us with a comprehensive, accurate, and very useful description of one of the most melancholy and insidious diseases to which the human frame is liable.

In limine the author takes exception to the term "general paralysis" as a singularly inappropriate denomination, "because that which is generally paralysed is certainly dead, and not living." We have therefore followed him in retaining in our translation of the title of the work the name "paresis," as signifying a lesser degree or an incomplete form of the affection.

The leading feature in Dr. Salomon's pamphlet is his arrangement of the symptomatic and pathological stages of the disease, and the connexion which he establishes between them. The following is the division alluded to:

Symptomatic Stages.

I. The stage of *altered* mind (= stadium alterationis).
II. The stage of *alienated* mind (= stadium alienationis).
III. The stage of *blunted* mind (= stadium dementiæ).
IV. The stage of *mindlessness* (= stadium amentiæ).

Each of these stages being referrible respectively to one of the following pathological stages:

I. Lepto-meningitis chronica.
II. Periencephalitis chronica diffusa.
III. Degeneratio substantiæ corticalis cerebri.
IV. Atrophia vera substantiæ corticalis cerebri.

The author gives a detailed description of the most prominent phenomena characterizing each of the above-mentioned symptomatic stages. We shall content ourselves with pointing out a few of those which indicate the first approaches of the insidious malady in question.

The symptoms of the first stage of the disease may be divided into those affecting the mind and those indicating impairment of the motor powers. The former are conveniently subdivided into alterations of humour, of the moral faculties, of energy, and of intelligence. Among the phenomena they present are irritability, melancholy, indifference (a state in which the patient may be said to conduct himself "*leniter in re, sed fortiter in modo*"), perversion of the moral faculties, indecision, and a yielding to outward circumstances, momentary absence of mind, forgetfulness of recent occurrences, morbid restlessness, *ennui;* thickness of speech, indistinctness of articulation, a difficulty in pronouncing more complicated words and those which abound in consonants [so much does the patient feel this difficulty, that to avoid such words, he will sometimes choose others, even though they may not so clearly convey his meaning], uncertainty of gait, a feeble step, an occasional difficulty in estimating distances, alteration of handwriting (the style becoming less rounded), involuntary twitchings in the muscles of the face, particularly about the angles of the mouth and eyes and in the upper lip. From such a commencement the patient passes [unless the disease be arrested in its incipient form: and much may be done, by due attention to sleep, diet, air, exercise, occupation, &c., at least to postpone the evil day] through the second and third stages, until, in the fourth he is found in the highest possible degree of human degeneration: a living corpse, breathing and assimilating, and nothing more. "The wreck" [the author borrows our English word] "of the unhappy man lies dumb and motionless as a sack of flesh.* He is in the fullest sense of the term '*out of his mind.*'" [Once more Dr. Salomon draws, though not quite so appositely, upon our language, adding in a parenthesis, = crack-brained.] The paralytic symptoms now attain their climax. Speech is gone; the patient cannot alter his position, but lies motionless upon his back; the muscles of mastication and of deglutition are paralysed; so are those of the trunk, rendering respiration extremely slow; the motions of the thorax are scarcely perceptible; the impulse of the heart is feeble in the extreme; the food often passes into the larynx, producing suffocation, or in consequence of paralysis of the muscles of the pharynx, remains behind the root of the tongue; the temperature of the skin falls; gangrenous sores sink deeply, often reaching even to the subjacent bony parts; finally, death, long wished for by those around him, closes the miserable scene.

In cases where the paralytic symptoms are not at first fully recognised, insanity with paralysis is distinguished from *other forms of mental disease* by its progressive character. From *apoplexy*, with the slighter forms of which, where the tongue is affected, it may also be confounded, it is diagnosed by the more or less unilateral character of the latter. The principal feature of the differential diagnosis between it and *alcoholismus chronicus*, is drawn from the difference in the starting-points of the two diseases. General paresis proceeds from a morbid process in the delicate membranes of the brain; chronic alcoholism from a general intoxication. In the former, the psychical symptoms first present themselves; the degeneration of the mind tends to that of the body. In the latter, the paralytic symptoms occupy the first place; the general intoxication of the body leads to the mental degeneration. The disease under consideration has also been confounded with paralysis from *muscular atrophy*. In both diseases we have progressive paralysis of the muscular system. In other respects the two affections are entirely dissimilar. In one the seat of lesion is in the brain, in the other it is in the muscles. Paretic individuals can, under the influence of their delirium, employ their muscles in a very violent manner: they may dash in pieces the door of the room in which they are confined. A person, on the contrary, labouring under paralysis from muscular atrophy, does not rave, and in consequence of the degenerated state of the muscles, never can be violent.

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* "Comme une masse inerte."—GUISLAIN.

The honour of having demonstrated the anatomical changes in insanity with paralysis belongs to the Vienna school (Rokitansky, Wedl). We have already mentioned what the pathological condition corresponding to each symptomatic stage consists in. In patients who die in the first stage the pia mater is found to be in a state of inflammation. The periencephalitis of the second stage is recognised by an increase of volume of the cortical portion of the brain. The degeneration of the third stage has been demonstrated by Rokitansky. When the patient has survived to the commencement of the fourth stage, the most superficial part of the cortical substance, corresponding to the lamina nervea in the healthy state, is converted into cicatricial tissue, so that when we feel with the tip of the finger the surface of the now nearly obliterated convolutions, the outer portion gives a sensation of firmness, while a certain degree of fluctuation is observed from the subjacent dissolved cortical substance of the brain.

The author, in conclusion, gives a *résumé* of the pathological observations of Rokitansky and Wedl, and closes his clear and comprehensive essay with the deduction, that "in the present state of science it would appear that the disease consists essentially in a degenerative process in the adventitious membrane of the vessels of the pia mater, and in the connective tissue surrounding the elements of the cortical portion of the brain, which degeneration, in its development, gives rise to the change of the grey cerebral cells into an inert mass."

Treatment Dr. Salomon does not touch upon. Perhaps he considers it superfluous to allude to it in connexion with so hopeless a malady. Nevertheless, we believe that, as we have already taken the opportunity of suggesting, much may in its early stages be done in the way of indefinitely postponing the progress of the disease, by inculcating early rising, or more correctly restriction of the hours of sleep, attention to diet, exercise in the open air, occupation, cheerful society, and regulation of the bowels by food rather than by medicine, for in no other affection with which we are acquainted would anything approaching to hypercatharsis prove more injurious than in this. The employment of derivative means, as an issue, seton, or open blister in the back of the neck, is also of importance.

ART. VI.—*A Manual of the Practice of Medicine.* By T. HAWKES TANNER, M.D., F.R.C.P., Assistant-Physician for the Diseases of Women and Children to King's College Hospital. Fourth Edition.—*London*, 1861. pp. 704.

THE present edition of this "libellus" exceeds the former one (of which we gave a notice in our number for January, 1858) by above two hundred and fifty pages. Whilst enlarging it by the incorporation of much recent information on the various subjects treated of, the author has kept sight of the chief qualifications which we remarked as conspicuous in the work which he has executed—viz., clearness and conciseness. He sometimes, however, falls into a fault which, unless very ample care is taken, is necessarily attendant, as it appears to us, on the construction of manuals, and that is, diminished fulness and "*exactness.*" We quote one example only, which will be found at page 610, where allusion is made to the alteration of the pupil of the eye in certain cases of aneurysm. The author observes that the "contraction (of the pupil) is partially due to irritation of the sympathetic." Here, the distinction is not observed between the effects of "irritation" and of "paralysis" of the sympathetic. When the branches or trunks of this nerve are "*irritated,*" as by slight pressure, then we look for dilatation of the pupil, owing to over-action of those fibres of the iris which are supplied by this nerve. But the contraction of the pupil in cases of aneurysm would surely be caused by "*paralysis*" of those muscular fibres of the iris which are supplied by the sympathetic, those fibres, that is, which, radiating from the edge of the pupil, produce by their action dilatation of the pupil; and would only arise when the aneurysmal pressure was extreme, and was greater than sufficed to produce mere *irritation* of the sympathetic.

ART. VII.—*Recent Works issued by the New Sydenham Society.*

WHEN commenting, just a year past, upon the fruitful energy displayed by the Council of the above-named Society in furnishing its members with most excellent and practical works, and that at an astonishingly cheap rate, we failed to observe a misprint which entirely subverted the meaning of a sentence to which we would now allude.

At page 184 of the number for July, 1861, a passage runs thus: "Perhaps also the Council would do well to give no more translations of short foreign communications." In this paragraph the "no" ought to have been "us," for we intended to suggest that such translations WOULD BE most acceptable and useful. It is, then, with much satisfaction that we now find that the members of the Society have had placed in their hands during the past year not only a "Year Book" for 1860, the first fasciculus of an Atlas of Portraits of Skin Diseases, the first volume of a translation by Dr. Balfour of Edinburgh of 'Casper's Handbook of the Practice of Forensic Medicine' (the original of which was noticed in our number for October, 1857), but also a volume containing the translations of five foreign communications of great importance: these are, Czermak 'On the Practical Use of the Laryngoscope,' translated by Dr. Gibb; Dusch 'On Thrombosis of the Cerebral Sinuses,' by Dr. Whitley; Schroeder Van der Kolk 'On Atrophy of the Brain,' by Dr. Moore, of Dublin; a Paper by Radicke on the Application of Statistics to Medical Inquiries,' by Dr. Bond; and Esmarch 'On the Uses of Cold in Surgical Practice,' by Dr. Montgomery, of St. Thomas's Hospital, London.

All of these communications are evidently of high practical and scientific value, but as it so happens that we are in hope ere long of presenting to our readers separate papers in which most of these subjects will pass under consideration, we forbear entering upon any critical examination of them at the present time.

The skin portraits we hope soon to notice in a review of the teaching upon skin diseases now afforded at Vienna, and the translation of 'Casper's Jurisprudence.'

As regards the 'Year Book,' which on a former occasion provoked much criticism and free handling, we find that its editors have availed themselves of various suggestions thrown out for their consideration. The present 'Year Book,' owing to a smaller type having been used, contains one-third more matter than the one did for 1859, and also differs from it in the arrangement of the material used. When abstracts are given, these follow immediately the titles of the papers alluded to, which are grouped together as much as possible according to the subjects on which they treat, in preference to being arranged alphabetically. The abstracts are improved by being on the whole much fuller than in the preceding volume, though individual ones are not so lengthy and verbose; and the final index is also rendered more valuable by being more complete.

ART. VIII.—*Ready Rules for Operations in Surgery.* By ALLAN WEBB, M.D., &c. &c.—*London*, 1861.

IN performing operations at a military hospital, Dr. Allan Webb has been in the habit of writing out on a black board in Hindustani and English the necessary directions for his native assistants, who are changed every three months. These short descriptions of operations became very popular in India, and were published in a book, the second edition of which is now before us; the first, as we are informed, having been out of print for some time. Dr. Webb rather oddly remarks in his preface, that he believes his plan to be good—a superfluous observation, since otherwise he would not have followed it; and we believe so, too, under the circumstances that he describes—viz., when it is necessary to operate with inexperienced assistants. But a far better plan, of course, is to secure the assistance of a sufficient number of persons practically acquainted with the operation contemplated; and then no definite rules for their conduct are necessary, since each man understands at a word what he has to do, and knows how he ought to do it. But for exigencies such as those of military practice, or for the guidance of students learning operative surgery, we are disposed to think Dr. Webb's book a very useful aid. He gives one table to the description of each operation. On one side are the rules for the operator, printed in black ink; on the other, a list of the instruments required, and the rules for the various assistants, printed in red ink. Forty-nine operations are described, including most of those usually required in English practice, and some operations for elephantiasis scroti, which are peculiar to tropical practice. In one of these operations the tumour is supposed to be of such a size that Dr. Webb directs it to be suspended by means of a pully and piece of sailcloth from the ceiling! It is fortunate for our European patients that such operations are not required in this country, otherwise, even with the practised assistance that is always at hand, not a few of them would infallibly sink under the hæmorrhage; but as Hindoos bear operations better, it is possible that sufficient success may attend on such proceedings to justify the risk.

We could point out a good many minute points on which we should be inclined to recommend different directions to those which our author gives; but, after all, these are matters of opinion. We would complain also that here and there, in the laudable attempt to be brief, Dr. Webb becomes somewhat obscure. On the whole, however, we can have no hesitation in expressing our high sense of the value of this little work. The rules show a practised and bold operator, and will be eminently useful to army surgeons and those who are qualifying for that branch of the profession. May we suggest, however, to Dr. Webb that the best test of all rules is their success; and that with the extensive experience which he seems to have had in operations on the natives of India, he would be doing a good service to the profession which he follows with so much zeal, if he could give us some satisfactory and reliable statistics of the results of operations on them.

Art. IX.—*A Practical Treatise on Military Surgery.* By Frank Hastings Hamilton, M.D., &c. &c.—*New York*, 1861. pp. 234.

This work probably owes its appearance to the late unhappy events which have converted so many of the peaceful citizens of the United States into soldiers, and which have, of course, turned a corresponding number of country doctors into army surgeons. To the latter class Dr. Hamilton's small treatise will probably prove useful. It contains information on every subject which can occupy their attention, from the making of Irish stew to amputation at the hip-joint. Examination of recruits, general hygiene of troops, their accommodation in camp, military hospitals, preparation for the field, the management of men on march, the conveyance of the wounded, the specialities of military surgery (gun-shot wounds), amputations, the use of anæsthetics in warfare, hospital gangrene, dysentery, and scurvy—each of these important subjects forms the text of a separate chapter; besides which, an appendix gives a mass of miscellaneous information calculated to be useful to army surgeons in general, and to those serving the United

States in particular. This is a goodly list; but when so many important matters have to be treated in the compass of some two hundred and thirty pages, it is inevitable that the style of treatment should be a little superficial. This is eminently the case with the section on gun-shot wounds, which is hardly equal to Professor Hamilton's well-earned reputation. Perhaps, however, the author may have thought that, as his readers would have access to other and more elaborate surgical treatises, it was the less necessary to go deeply into the matters there spoken of. We are glad to see that Dr. Hamilton renders justice to the superiority of the medical service of the English army over the much vaunted system of our French allies in the Crimea. It is so much the habit of our newspaper correspondents to echo the cuckoo-cry of "They manage these things better in France," that the public has been deeply imbued with the idea that our military surgery is far inferior to that of the French army—the very reverse of which is the fact. An American is not likely (especially, alas! writing in the year 1861) to err from British partiality in any comparison with things French. Our author speaks as follows:

> "The French army sanitary system is exceedingly complicated, and its details are made out in the most elaborate manner; nothing is left to conjecture; every duty is defined so exactly that there can be no chance of error. . . . . In the British service the system is much less elaborate, and there is much less precision in the rules which govern its details; so that, to the casual observer, it seems imperfect, and contrasts unfavourably with the French system; but the British surgeons are permitted to exercise a certain amount of authority over their own department, such as is not allowed to the French surgeons, &c. The result, fairly traceable to these apparently insignificant, but, as every medical man knows them to be, important practical differences, was, that the English army closed its campaign with a loss, by death or invaliding, of less than one-third of the troops, while the French had lost more than half of their whole number." (p. 32.)

Dr. Hamilton has ranged himself among those military surgeons who decry the use of anæsthetics in warfare, although his opposition to the employment of these inestimable medicines is not so violent as that of the determined opponents of chloroform. Still, however cautious may be the manner in which he speaks, we cannot but think that it is a grievous error to revive in any degree the spirit of Dr. Hall's famous order to his medical subordinates at the outset of the Crimean campaign. Since that period chloroform has been abundantly tested both at Sebastopol and in Italy, and the theory of the stimulating effects attributed, in the document above referred to, to a lusty roar under the knife, has been pretty well exploded. Nor has the notion, to which Dr. Hamilton seems to incline, that anæsthetics exercise some unexplained influence on the tissues of wounds, indisposing them to union by first intention, although frequently propounded, received the smallest support from direct evidence. Dr. Hamilton seems to rely a good deal on the following fact, which he quotes from McLeod's well-known book:—

"Says McLeod: 'I never saw one amongst our most numerous amputations in which primary adhesion took place throughout the whole surface of the flaps.' Is not this a most unparalleled experience? Do the annals of surgery furnish another such example?" (p. 185.)

In case they do not, we will endeavour to supply one by confessing that, in a hospital experience extending now to a great number of years, we never saw the wound of an amputation, nor any other wound of anything like equal size, whether made under the influence of chloroform or not, in which primary adhesion took place throughout its whole extent, nor do we believe that such a phenomenon has ever occurred in Professor Hamilton's or any other surgeon's practice; but we have noticed quite as favourable examples of primary union in wounds made under anæsthetics as in any others. If this were not so, why have not our surgeons long since discovered that chloroform would be fatal to their reputation? We trust that the benefits of the greatest discovery of modern times will not be denied to men when they most require it, on the strength of such flimsy reasoning as this.

But, however we may differ from some of Professor Hamilton's opinions, we can recommend his work as a sensible, useful, and concise treatise on the subject which he has undertaken; and in taking leave of him, we can only express a fervent hope that our military surgeons may not have the opportunity forced upon them of giving practical evidence on American soil of their proficiency in the lessons which he inculcates.

Art. X.—*Die Krankheiten der Handwerker.* Von Professor Adolph Hannover.

The Diseases of Artisans. By Professor Hannover.

This essay appears in the fifth, sixth, and seventh numbers of the 'Monatsblatt für Medicinische Statistik und Offentliche Gesundheitspflege,' for May, June, and July, 1861, and is primarily concerned with the statistics of the diseases of the working classes in Copenhagen. The author's observations are founded on the statistics of the large General Hospital, and in the present paper apply only to internal or medical diseases.

At the outset of his paper he remarks on the difficulties surrounding the inquiry he has taken in hand, except, indeed, in the instance of artisans who use in their trade well-known poisons whose operation is fully understood—such as mercury, lead, and phosphorus. Among others, the causes of disease dependent on their occupation are obscure, and to be sought in the nature and character of their work, and in the external circumstances associated with it. The only method to arrive at a correct knowledge of the immediate influence of the several trades on health would be by taking a certain number of artisans, of about the same age and conformation, and of sound constitution originally, and to note the duration of their healthy condition and the consequences of the favourable or unfavourable circumstances under which they have subsisted.

Professor Hannover proceeds with some preliminary remarks on the character of the population of Copenhagen, and on the relative proportion of those who are, and of those who are not engaged in some handicraft, and then by means of a table shows the relative numbers of persons of the different trades in one thousand admissions into the hospital, rightly commenting, at the same time, on various circumstances which dispose, or otherwise deter the operatives in the several trades availing themselves of hospital treatment. In subsequent tables he exhibits the numbers in different trades who have been labouring under one or other internal diseases, and then taking each principal variety of disease, considers the special relations between it and the occupation of the patients who are its victims. The statistics of the mortality of diseases in connexion with the several trades are also well set forth, and some interesting supplementary tables respecting the prevalence of insanity among different classes, are added on the authority of Dr. Funder, of St. John's Hospital at Bidstrup.

We hope to have an opportunity ere long to discuss at large the conclusions of Professor Hannover, along with those arrived at by other investigators of medical statistics.

ART. XI.—*Transactions of the Ethnological Society of London.* Vol. I. New Series.—*London*, 1861.

THOUGH not a handmaid of practical medicine, yet there is an intimate relationship between ethnology and anatomy and physiology, for on these sciences the stability of the first-named science mainly rests. Indeed, the rise and progress of ethnological science are greatly indebted to members of the medical profession, and particularly to Dr. Prichard, whose book on the 'Races of Men' is still a standard work on the subject. Even in the volume before us we find valuable contributions from medical men—from Mr. Greenhow, Dr. R. G. Latham, Dr. Beddoe, Dr. Knox, Mr. Lockhart, Mr. Busk, and Mr. Dunn ; but we regret to learn from a most valuable paper by the last-named surgeon, 'On the Psychological and Physiological Evidence in Support of the Unity of the Human Species,' that the Ethnological Society is at the present time not so well supported by the medical profession as it was when first established. We would desire to enlist the co-operation of medical men, not only on the ground of the connexion between ethnology and those subjects, such as physiology, with which they have a special acquaintance, but also on account of the good it would do them to mix among non-professional scientific men, and to get those angularities rubbed off which are otherwise apt to grow too luxuriantly among purely medical acquaintance and medical societies. And looking only to the contents of the volume before us, we can assure them that the subjects discussed at the Ethnological Society are eminently calculated to arouse their interest and attention.

The prominence which has lately been given to M. du Chaillu's African researches will lend additional importance to a paper by that gentleman in the present volume, "On the People of Western Equatorial Africa," and to the "Ethnological Notes on M. du Chaillu's Explorations and Adventures in Equatorial Africa," by that hardy, venturesome traveller, Capt. R. Burton.

In conclusion, we would recommend to our readers the perusal of this volume of ethnological essays (which we should have noticed in detail had we sufficient space at our disposal), and at the same time congratulate the Society which publishes them on the excellence of the work they accomplish.

Art. XII.—*Elements of Medical Zoology.* By A. Moquin Tandon, Member of the Academy of Sciences, &c. Translated and edited by R. T. Hulme, M.R.C.S.E., F.L.S., Lecturer on Dental Surgery, &c. pp. 423. 1861.

The thanks of the medical profession are due to Mr. Hulme for introducing M. Moquin Tandon's work to their notice in an English garb. In the wide range of English medical literature there was no book which comprised all the information afforded by his treatise. Besides a complete account of all animals furnishing substances to which curative properties have been attributed, it presents a full, although at the same time terse and concise, zoological description, together with a relation of the action and effects of all noxious and poisonous animals, and of human epizoa and entozoa. The classification which the author proposes for the purposes of medicine is not founded upon zoological affinities, but rather upon the characters of the animal in its medico-zoological relations. Such an arrangement has, of course, no pretensions to be scientific; it is simple and practical, and adapted to the wants of the practitioner of medicine. Thus, for instance, all poisonous animals are treated of together, and are divided into two sections. The first includes those which convey their poison by the mouth, comprising poisonous animals with fangs—serpents; and poisonous animals armed with antennæ in the form of claws or with foot-jaws—spiders, scolopendra. The second comprises animals which inoculate their poison by means of a special organ—viz., the ornythorynchus, scorpions, and the hymenoptera. The account of the human internal parasites leaves in the present state of knowledge little to be added. Mr. Hulme has not only performed a translator's part well, but he has exercised the editorial function for the benefit of the reader by interblending with the original text much new matter. Amongst the interpolations we notice the late experiments of Virchow and Leuckart on the propagation of trichinæ, a notice of the Distoma Buskii, and of Griesinger's account of the peculiar form of chlorosis designated by him Egyptian, and the production of which he refers to the Ancylostomum duodenale.

PART THIRD.

Original Communications.

Art. I.

The Diseases of St. Kilda. By John E. Morgan, M.A., M.B. (Oxon Member of the Royal College of Physicans, Manchester.

Examples selected either from the vegetable or animal kingdoms testify to the differences in organic development which may, even in a comparatively short space of time, result as a consequence of the varying physical conditions to which any particular species may happen to be exposed. The same truth is daily illustrated in the science of medicine. Diseases designated indeed by one term, nevertheless appear under so many phases, dependent upon the circumstances which gave them birth, that it is frequently no easy matter to determine either the order under which they are to be classed, or the causes on which they depend; and if this holds true even where difference in respect to such questions as employment, and diet, and race are insignificant, much more may it be looked for among those persons who, in respect to all these points of comparison, differ very materially from their neighbours. Viewed in this light there is, I think, much in such an island as St. Kilda which more especially calls for observation. It is situated, as my readers are doubtless aware, some sixty miles to the west of the group of islands known as the Hebrides; while three sides are walled by precipitous cliffs, the fourth descends with a gentle slope towards the sea, its two extreme points embracing between their arms the intervening bay which serves as a harbour, with a holding-ground and shelter sufficiently insecure. At a short distance from this bay lived, and for upwards of four hundred years have lived, that small and secluded society who together constitute the people of St. Kilda. To the population of Great Britain and Ireland they contributed at the taking of the late census seventy-eight souls—thirty-three males and forty-five females, divided among twenty families, domiciled in twenty houses. The exact time at which the island was first colonized is a somewhat disputed and not very important question. Dean Monro, who travelled through the Hebrides in the year 1549, refers to it under the name of Hirta, and distinctly alludes to its being inhabited at that time. During the whole period of its history the people have remained singularly stationary, neither, except under very peculiar circumstances, encouraging emigration, nor admitting strangers to dwell among them. Their occupation, which is neither agricultural nor piscatorial, might almost, were it not their recognised calling in life, be termed acrobatic, a great portion of their time being passed rather in the air than on either land or water. In the pursuit of the sea-fowl, especially the fulmars, a species of petrel, a great portion of the male population may be seen suspended by ropes around the cliffs, as composedly applying themselves to their work as the farmer to the cultivation of his soil. Their food—to modern ears, at least—has little to recommend it, and seems but few degrees more natural than their calling. It consists to a great extent of sea-birds, the flesh and eggs eaten fresh in summer, while no less than twelve thousand, or one hundred and fifty for each man, woman, and child, are salted down for winter consumption. A considerable admixture of oleaginous food appears to be required to render such a diet either palatable or digestible, inasmuch as St. Kilda historians speak of the necessity of making a free use of a peculiar preparation termed "the Giben." This consists of the fat with which the breasts of the young solan geese are thickly padded, melted down, and stored up in stomachs obtained from the old gannets, and preserved like bladders of hogs' lard. A small quantity of coarse meal, potatoes, and milk, complete their dietary table. Such is the mode of living among these people as regards their occupation and diet; to the construction of their dwellings, another important element in the causes which influence the origin and propagation of disease, I shall have occasion hereafter to revert.

In the course of June, 1860, during a cruise among the Western Isles, I happened to call at St. Kilda. As the difficulties of visiting the island, in consequence of the absence of all public means of communication, are considerable, and comparatively few persons have inquired into its condition, it appeared to me that it might prove neither uninteresting nor unprofitable to investigate, as far as lay in my power, into the history, causes, and symptoms of one or two very rare affections which have long been associated with its shores. To the prosecution of this task I was further invited, by the discovery while there of a register of the number and causes of death which occurred on the island between July, 1830, and September, 1840. It was kept by a missionary resident at the place during the years over which it extends, who from the careful manner in which he fulfilled his task, and the remarks he had interspersed in reference to the causes of death, was evidently a well educated and intelligent man, far better entitled to form an opinion than the generality of Highland registrars. At this time the aggregate population amounted to about one hundred and five persons, or twenty-eight more than at the time of the late returns of the census; but nevertheless, as thirty-five natives have emigrated since 1851, the island cannot be spoken of as becoming "depopulated" in consequence of its peculiar diseases. During the period of time included in the ten-year register, 65 births and 64 deaths are respectively recorded. Of the latter, no less than 33 were due to a disease termed by the inhabitants the "eight-day sickness;" among medical writers, the trismus neonatorum, or infantile lockjaw. Neither term is strictly correct, inasmuch as the infant by no means invariably dies on the eighth day, while in many cases the jaw can at no time of the malady be spoken of as locked. The first notice of the trismus at St. Kilda which I have been able to discover, is contained in a description of the island by the Rev. Kenneth Macaulay, a missionary sent there by the Society for Propagating Christian Knowledge in Scotland, and long resident at the place. In this work, published in 1764, he remarks—

> "The St. Kilda infants are peculiarly subject to an extraordinary kind of sickness; on the fourth or fifth day after their birth many of them give up sucking, on the seventh their gums are so clenched together that it is impossible to get anything down their throats. Soon after this symptom appears they are seized with convulsive fits, and after struggling against excessive torments till their strength is exhausted, die generally on the eighth day."

Though the fact of the great prevalence of this affection has been repeatedly confirmed by visitors to the island, among others by Mr. J. Wilson and Mr. M'Lean, nevertheless, the extent to which it occurs, and the circumstances under which it originates, have not been brought forward with the exactness they deserve.

First, then, as regards the symptoms: I had not myself an opportunity of observing a case of the disease, an infant of the required age certainly existed at the time of my visit, though it showed no disposition to satisfy my curiosity. Its invasion and progress were, however, fully and even graphically described to me by the nurse of the isle's-women, who during the last thirty years has probably witnessed almost every attack of the trismus, besides having its fatal effects brought home to herself by the loss of twelve out of fourteen children born alive. She emphatically asserted that at the time of birth there was no appreciable physical inferiority on the part of those infants who were so prematurely and suddenly selected as a prey. "They were all proper bairns," and so continued till about the fifth or sixth day. The mother's eye might then not infrequently observe on the part of her child a strange indisposition to take the breast. The import of this first premonitory symptom is well understood; sooner or later it is succeeded by great restlessness, while all the symptoms are exacerbated when the child attempts to swallow. At the same time involuntary twitchings along the course of the muscles, sudden and violent starts, and in certain cases a peculiar and piercing shriek, often heard as the child seems dozing, plainly indicate a serious disturbance of the nervous system. The lower extremities are usually the next to become involved, the legs being violently and spasmodically drawn up, and the toes flexed. The back is sometimes rigid and stiff, while in other cases it is affected with sudden and acute opisthotonic seizures. The state of the jaw is equally variable, in some instances it may be literally spoken of as locked, and when this is the case the disease is much less interrupted by violent and often-recurring spasmodic paroxysms, than in other and more numerous cases in which this organ seems relaxed, its directing nervous influence paralyzed and dead, while the muscles feel soft and flabby. To this relaxed condition of the jaw my informant apparently attached marked significance, for on referring to it she used these words, "When once the jaw falls we lose hope, I have never seen a child come round when that happened." In one of the spasms which in so remarkable a degree attend this particular state of the jaw, the strength of the little sufferer fails, and death closes the scene in from thirty to seventy hours after the first premonitory symptoms.

In the foregoing description of the trismus I have, on the authority of my informants, directed especial attention to the fall of the jaw. From what has been stated above, there are, it would appear, in infantile trismus, two conditions of this organ more or less distinct—the one rigid and locked, the other relaxed and accompanied by paroxysms. According to some authors, the former of these two is far the more common, and indeed the only one referred to. Thus, Dr. West, in his work 'On the Diseases of Infancy,' speaks of the jaw in cases of trismus as "at first slightly open, and the corners of the mouth drawn downwards and backwards, but as the disease advances the jaws become quite closed, the corners of the mouth even more drawn down, and the lips firmly compressed against the gums." On the other hand, what was stated at St. Kilda, coupled with a description of the disease as it occurred in the West Indies (where we are informed, on the authority of Dr. Morrison, who practised in Demerara for eight years, that the disorder is commonly called "jawfall"), permit us to infer that the opposite state is by no means unusual. These two conditions are probably to be explained on the assumption that in proportion as one or other of them is the more marked, so are either the spinal nerves or the reflex system more directly involved, the spasmodic paroxysms pointing to a disordered state of the latter, while the permanent rigidity denotes the exalted action of the former. The influence severally exercised by one or other of these systems of nerves is at times observable in certain cases of traumatic tetanus. I may refer for an illustration to the case of a girl, a patient of Mr. Lane's, in St. Mary's Hospital, in the spring of 1861, suffering from trismus induced by a burn, which I had an opportunity of carefully observing. After various premonitory symptoms, such as pain and stiffness along the back of the neck and over the region of the temporal muscle, the lower jaw at length became firmly locked, though the teeth were separated by an interval of nearly one inch. During five days the distance which intervened between the two sets of incisors kept continually growing less, until on the day of her death it was reduced to about a quarter of an inch. At no time were the lower teeth rigidly compressed against the upper. This particular state of the jaw, however, merely continued so long as the patient was awake and remained conscious. As sleep crept over her features, the sardonic grin which rudely distorted no ill-favoured face gradually disappeared, and the expression, after becoming for a moment natural, passed on to a look which was unmeaning and vacant, and in proportion as the sleep appeared more sound so did the jaw more completely hang down, while the tongue at the same time protruded from between the lips. With this change in the mouth the spasmodic paroxysms which had before occurred at long intervals, and with comparatively slight severity, became far more violent, insomuch that the nurse was under the necessity of guarding the tongue from becoming injured, as the teeth kept suddenly gnashing together. Such a case is, I think, instructive in more ways than one; it both illustrates how, in the course of the same disease, the state of any particular organ may differ during the hours of sleep and wakefulness; and it further shows how, when the spinal nerves are for a time released from the restraint exercised over them by the mind, a sort of usurped authority seems taken up by the reflex system. Experiments on animals in which, after division of the spinal cord, the reflex movements have been more readily excited, together with similar pathological phenomena observed in cases of paralytic seizure, both speak to the same truth.

As regards the extent to which the trismus occurs, I am in a position to speak with some degree of accuracy. It has been asserted by a gentleman who visited St. Kilda in 1838, and on his authority repeated in the fifth volume of this Review, that it proves fatal to eight out of every ten infants who are born alive. In the ten-year register to which I have already refered, as extending from 1830 to 1840, and which consequently includes the year 1838, out of sixty-four deaths, thirty-three, or about one-half, are assigned to the trismus. During the last five years likewise, from 1856 to 1861, in which a register has likewise been employed for chronicling the deaths, out of the total of seventeen, eight are entered under this disease. I may further mention as the result of a calculation based upon a statement elicited from the different families in the island, respecting the number of children they had severally lost and succeeded in rearing, that the former amounted in the present generation to eighty-five, and the latter to forty-four. Of these eighty-five, however, probably one-third would be due to various other causes of death incidental to childhood. On striking an average from these figures, we are probably not far from the truth in affirming, that since the year 1830 five infants out of every nine born alive have fallen victims to the ravages of the trismus. In the great majority of the cases, death occurred between the fifth and the tenth day, though in one instance, considered most unusual, it is true, life was prolonged to the seventeenth day. Great as this tetanic mortality undoubtedly is, it has been far exceeded in the case of the Westmann Isles, off the coast of Iceland. Sir Henry Holland, in an appendix to Sir George Mackenzie's travels in Iceland, gives an interesting description of the disease as it occurred in one of those islands at the beginning of the present century. Thus, in speaking of Heimacy, one of the group, he remarks that the population, which at that time did not amount to two hundred, was "almost entirely supported by emigration from the mainland, scarcely a single instance having been known, during the twenty years preceding his visit, of a child surviving the period of infancy." He supplies a table showing the mortality consequent upon the disease, and the days upon which death occurred. We there learn that out of one hundred and eighty-five deaths, seventy-five took place on the seventh day, twenty-two on the sixth, eighteen on the ninth, and sixteen on the fifth and eighth. In this list the second and twenty-first days are respectively the earliest and latest on which the disease terminated fatally.

Let us now turn to inquire into the nature of the causes at work at St. Kilda to which this heavy mortality may be attributed. It has been assigned, both here and at the Westmann Isles, to the filthy manner of life which prevails among the people. Such expressions are, however, somewhat vague, and do not convey any very definite idea respecting the nature of those particularly offensive customs which both generate the disorder here, and might be expected to do so wherever they prevailed. An inquiry into the history of the disease, by the apparently opposite circumstances under which it occurred, tends rather to confuse the mind than assists it in arriving at any well-grounded conclusion. Thus, for example, eighty years ago we observe it prevalent in an apparently well ordered lying-in hospital in Dublin, carrying off vast numbers of negro infants in our West Indian colonies, checking all increase of population at St. Kilda, and the complete scourge of the Westmann Isles. Few, indeed, seemed the points of resemblance between these several localities. In respect to race, diet, occupation, and climate, they more or less differed from each other. In one respect, and in one only, may a certain similarity be traced between them.

The occurrence of infantile lockjaw has often been ascribed to one or other of the following causes: mismanagement of the umbilical cord at the time of birth, exposure of the infants to sudden alternations of temperature, and a vitiated atmosphere dependent upon deficient ventilation. Whatever influence may be due to the first in inducing the disorder in other places, it certainly cannot be held responsible for it at St. Kilda; inasmuch as both the operation itself and the treatment subsequently adopted, are, as far as I could learn, in no way different from that generally practised along the west coast of Scotland, where no such untoward circumstances follow as a result. Neither are we, I think, justified in attributing more weight to the effects of sudden changes of temperature. Among the Hebrides the climate is remarkably equable, and the examination of meteorological tables taken at places not far removed from St. Kilda in respect to their latitude, justify me in saying that there are few parts of Great Britain where either the extremes of temperature between day and night, or summer and winter, are subject to such trifling variations. The vitiated atmosphere, however, the third cause frequently adduced, cannot be considered as innocuous as the other two; but to render this part of my subject intelligible, it is necessary to point out one or two important distinctions between an ordinary Highland bothie and a St. Kilda hut. Viewed from their exterior, indeed, there is little appreciable difference, and what there is is decidedly in favour of the latter. It is to this similarity in the appearance of the two dwellings that the cause of the trismus on the island has probably escaped observation. In the Hebrides and along the north-west coast of Scotland, the cabin is usually constructed of unhewn stones loosely piled together, without the addition of mortar. The roof is closely thatched, excepting at one corner, where a hole, some eighteen inches in diameter, as far as practicable sheltered from the wind, may generally be observed. At this hole the smoke, which freely rises from a peat fire in the centre of the room, is allowed to escape. This aperture in the roof being uncovered both by day and night, contributes in no small degree towards keeping up a very efficient system of ventilation. In the St. Kilda huts this smoke-hole is dispensed with; and further, on carefully examining into the construction of the dwellings, I found that the walls, instead of being loosely put together, and to a great extent pervious to the atmosphere, consisted of a double stone dyke, separated by an interval of eighteen or twenty inches, completely filled with layers of peat and sods, whereby all ingress of fresh air is altogether excluded. The reason assigned by the people for keeping their houses thus hermetically closed, is the great scarcity of manure. In other parts of the Hebrides the long sea-weed, which is cut and collected at the time of the spring-tides, is employed for fertilizing the soil. In St. Kilda, on the other hand, in consequence of this source of supply being not very accessible, with a view of obtaining a substitute, the inhabitants strive as far as possible to prevent the escape of the smoke. Thus it is that the soot is thickly deposited on the inside of the cottage roof. With the return of spring this richly coated layer is carefully removed, and its place supplied by a corresponding addition to the outer thatch. I believe it is principally to this in-door manufacture of soot, the conditions for the successful preparation of which are so directly at variance with all sanitary requirements, that we must assign the vast prevalence of the trismus. Let philanthropists calculate the amount of health and life at which, during many generations, these poor people have been content to purchase a few loads of manure!

In support of this defective system of ventilation being the chief cause of the infantile mortality, I may mention the fact that in almost every place where the trismus has more especially prevailed, the ventilation has been found proportionately defective. Thus, for example, in Dublin Lying-in Hospital, to which I have before referred in connexion with this subject, the establishment of a complete system of ventilation gradually reduced the mortality from 17 to little more than 1½ per cent. In our West Indian colonies likewise, at the time the disease there was of such frequent occurrence, repeated reference is made to the close and confined apartments in which the women were lodged during, and for the first few days after, the birth of their children. Thus Dr. Maxwell states that on these occasions, "the air was carefully excluded from the apartment, and a fire kindled." Dr. Morrison also, who himself practised in the West Indies, speaks of "confined and smoky houses as a probable cause of the disease." I have not been able to discover what is the exact construction of the dwellings in the Westmann Isles, as regards the requirements of ventilation; whether, in fact, they differ as much from the Icelandic huts, as the St. Kilda do from the generality of Highland bothies; between the inhabitants of these islands, however, and the St. Kildeans, we find a curious point of resemblance in respect to their diet, to which it is right that I should allude. It consists in the consumption among both people of a great number of sea-fowl, and especially fulmars, which abound around their coasts. That such a diet, especially when the salted birds are eaten, should prove detrimental to the free secretion of milk, is readily to be credited; while at Kilda likewise, I was informed that during the first four or five days after their confinement, the mothers can but to a very small extent contribute to the sustenance of their infants. The latter are consequently, during this period, principally hand-fed. Whether there is anything in the flesh of the fulmars which renders their use peculiarly injurious, I am not in a position to say. I may observe, however, as some slight support of such a view, that in taking the birds by means of a noose, which is cast around their necks, great care is necessary on the part of the fowler in guarding his eyes against a remarkably pungent oil, which the fulmar, when disturbed, is wont to eject at the intruder. Possibly, some principle of an equally irritating nature may exist in the flesh, and so, through the mother, act specifically on the infant. In the hand-feeding, independent of the mother's nursing, there seems no ground for complaint, inasmuch as it is conducted in every way on the most approved principles. Though I have deemed it right to make this allusion to the possible consequences of partaking of a fulmar diet, I am bound to admit that I do not myself attach so much weight to any influence it may exercise, as to that of the very deficient ventilation, and I am the rather confirmed in this opinion by the fact that Sir Henry Holland seems to imply that the fulmar is eaten in parts of Iceland in the neighbourhood of the Westmann Isles, without the supervention of the trismus.

Besides the lock-jaw, there is yet another very singular affection associated with St. Kilda, which happened, at the time of my visit, to be rife among the islanders; it is familiarly known by the name "boat cough;" the term denoting what is universally believed in the island, that the arrival of a vessel from the Hebrides has now been so frequently followed by an outbreak of this disorder, that the two necessarily stand to each other in the relation of cause and effect. As many of my readers may doubtless consider that in giving credence to a story of this kind, I am dealing in the marvellous, I propose briefly to consider the nature and weight of the evidence upon which my remarks are founded, that evidence being altogether unconnected with the popular belief of the islanders, and in several instances but tardily conceded. The first notice of the disorder which I have been able to meet with, is contained in Martin's account of his visit to St. Kilda in 1692. His description of the place appears to have found favour at the time at which he wrote, inasmuch as the following references are taken from the fourth edition of that work. He enjoyed a good reputation among his contemporaries, and has generally been looked upon as a trustworthy guide on the subjects of which he treated. His curiosity appears to have been aroused by the description he had heard of this "boat cough," and he determined to spare no pains in thoroughly investigating the matter by interrogating the inhabitants in everything relating to its history and symptoms. He found their testimony unanimous in all that related to the disease, and they further assured him that they not only became affected after holding direct intercourse with strangers who landed on their shores, but likewise in consequence of the arrival of packages of foreign goods. He himself evidently was at first disposed "to look upon all this notion of infection as a mere fancy," till he found the disease break out apparently as a consequence of his own arrival. This circumstance, together with the result of his inquiries, and general observations on the subject, led him to give his assent to the prevailing belief of the islanders. Some fifty years after Martin's visit, the Rev. Kenneth Macaulay was sent as missionary to St. Kilda. He informs us that he received especial instructions from his employers to investigate the whole question of the "boat cough." He appears to have set out with a determination not to allow himself to be imposed upon by mere hearsay, but to be guided by what he might see. For such an inquiry his residence on the island as missionary afforded him peculiar facilities. After speaking of the general immunity of the inhabitants from all cases of illness at the time he arrived, he remarks, "I began to conclude with pleasure that my visit would do them no injury, but I concealed my suspicions, that I might not tempt them to impose upon me. . . . . But my doubts and suspicions were soon removed. On the third day after I landed, some of the inhabitants discovered evident symptoms of a violent cold, such as hoarseness, coughing, discharging of phlegm, &c., and in eight days, they were all infected with an uncommon disease, attended in some cases with severe headache and feverish disorder, so that without rejecting the most convincing of all evidence, the evidence of the senses, I was not able to suspect that their complaints at that time were either feigned or imaginary." As an example of the manner in which mere residents on the island, though not themselves natives, may after a time become infected, he relates an anecdote regarding a Mrs. McLeod, a widow of a former missionary, which was confirmed to him by herself. She asserted that for three years after going to St. Kilda she escaped the general infection, but after that time was a sufferer like the rest of the islanders. Mr. Macaulay further remarks that the natives complain of the company of strangers being for some time very offensive to them, and speaks of their "finding a difficulty in breathing a light sharp air when they are near you." The next writer who refers to the subject is Mr. James Wilson, in his 'Voyage Round the Coast of Scotland.' In this work he gives an interesting description of St. Kilda, and its missionary Mr. Mackenzie (the gentleman to whose industry I am indebted for the ten-year register), who he assures us "confirmed the account which he had formerly read and disbelieved, of the natives being almost always attacked by influenza after the arrival of a boat from the Long Island, the eastern range with which they have necessarily most frequent intercourse." During my cruise among the Hebrides in the summer of 1860, I fell in, at

Barra, with an intelligent old man, who had for a period of many years held the office of steward to the proprietor of St. Kilda. In the discharge of this office, it was his duty to visit the island once or twice annually, to collect the rent, which is paid in feathers, oil, &c. My informant was upwards of ninety years of age, and spoke of having made the voyage nearly eighty times in the course of his life. The result of his long acquaintance with the island was the conviction on his mind that the St. Kildeans seem rarely capable of associating with strangers without experiencing an attack of influenza. He was at a loss to assign any reason for the occurrence, but considered that his experience of the people justified him in vouching for the fact. At the time I myself landed at St. Kilda, I felt so sceptical in regard to everything relating to this "boat cough," that had not a chance circumstance presented it to my notice, I should scarcely have made it a subject of inquiry. The circumstance to which I allude was the visit of H.M.S. *Porcupine*, Captain Otter, R.N., some ten days previously, to which the inhabitants referred a most severe attack of influenza from which they were at that time suffering. One of the most remarkable peculiarities in connexion with this visitation of sickness, however it arose, was the extent to which the whole population appeared to be affected. There was a look of extreme depression and lassitude about every person we saw, and the short hacking cough heard on every side resembled the monotonous and gloomy sounds which issue from the wards of a consumption hospital. Even supposing for a moment that these attacks of influenza are in no way connected with the arrival of a foreign vessel, it nevertheless appears singular that a whole community, small though it be, should be so frequently and suddenly prostrated by a debilitating, and in some cases even a dangerous, disorder, as we witness in this case. The symptoms of the disease in the particular outbreak which I happened to witness were much more severe than those of ordinary catarrh. In the ten-year register, three deaths out of sixty-four are attributed to this cause, nearly as large a proportion, though the numbers are too small for admitting such an inference, as fall victims to phthisis in some districts in England and Wales. An extreme feeling of prostration, as in severe epidemics of ordinary influenza, forms a marked feature in the disorder, and is often preceded by great febrile disturbance. The supervention of hæmoptysis, likewise, is spoken of as an occasional occurrence, and is referred to by one of the missionaries as having fallen under his observation.

On the foregoing evidence, so far at least as I have been able to discover, the existence of the "boat cough" more especially depends. If we consider it sufficient to point to some mysterious connexion between the arrival of strangers and the outbreak of an epidemic, to account for the coincidence is by no means very easy. The commencement of the disorder has been sometimes attributed to the fact that the visit of a vessel often leads a portion of the inhabitants to expose themselves to the liability of taking cold in rendering assistance on landing. The deficient shelter of the harbour causes a heavy sea to break upon the shore; hence, as is almost an established custom of the island, whenever a boat is seen approaching the land, some eight or ten of the natives, with a view of rendering assistance, join hand to hand, walk out into the breakers, catch her as she rises on a wave, and haul her high and dry on the beach. There are those who would see in this exposure a severe attack of cold of an infectious character. The hardships to which these people are habituated in the prosecution of their calling as fowlers, forbid our admitting such an explanation as this. Moreover, the disease has on several occasions occurred when either the antipathy entertained towards the intruders, or the moderate state of the weather, has prevented their extending the customary aid. In the opinion of others, the affection is looked upon as a periodical epidemic, dependent upon easterly winds, and only so far connected with the arrival of a vessel, as the wind from that quarter would favour her course. This theory, however, fails to account for several visitations—as, for example, that which followed the visit of Martin, whose party, in consequence of the opposite direction of the wind, was compelled to resort to their oars. The *Porcupine*, likewise, as a Government steamer, would be freed from the necessity of waiting for the wind.

There are few questions connected with the science of medicine involved in greater obscurity than the origin of contagious diseases, and the causes upon which that property depends. Many disorders of this nature are prevalent at the present time, but found no place in the nosology of the ancients. What combination of influences favourable to their development impressed on these several varieties their distinctive features, and what are the causes which still tend to keep them separate and defined? Why, again, do we see such varying degrees of susceptibility on the part of individuals to the poison of the different infectious disorders, that one person in constant attendance upon a fever patient escapes unscathed, while another merely entering his room is selected as a prey? These are questions which, in the present state of our knowledge of disease, neither have been, nor admit of being satisfactorily answered, though they testify to the subtile nature of the different morbid poisons, both in respect to the manner in which their influence is diffused, and their origin masked in obscurity. Is there anything in this boat cough which might lead us to suppose that susceptibility to certain disorders, at any rate, is, in a great degree, dependent upon the particular circumstances under which a community may happen to live, and so throw some light on the possible origin of influenzas? Is it supposable, from what has been said in regard to the suddenness of its attack, that when men live for a length of time entirely shut out from the rest of the world, under conditions altogether exceptional, both as respects occupation and diet, the mere fact of their coming in contact with strangers should exercise a sort of infectious influence on the more susceptible of the two? I have related above how the people in certain cases complain of experiencing a certain strange feeling of uneasiness in the company of visitors, a feeling we can imagine similar to what is often felt in the presence of persons suffering from ordinary catarrh, and which seems to justify such expressions as the being *sensible* of having contracted a cold from another. That susceptibility to certain contagious diseases has a natural tendency to increase, and the diseases themselves, when they do appear, to prove more malignant in proportion as their outbreak is the longer delayed, the observation of individual cases, no less than the history of epidemics, as seen among secluded societies, permit us, I think, to infer. Illustrations of the former, we may consider as in some degree adduced, by observing the greater severity of such disorders when contracted by adults as compared with children; while the latter is exemplified, in their still intensified fatality, when occurring at distant intervals of time, in such islands as Iceland and St. Kilda. Thus, for instance, we find that upwards of one hundred and thirty years ago, the small-pox was introduced among the inhabitants of the latter island, and before its ravages had ceased, carried off more than half the population. The fatal severity of several irruptions of the same disease, recurring after being long unknown, in the island of Iceland, may be found recorded in its history. In the year 1846, measles, after being absent from the Faroe Isles for sixty-five years, were at length introduced. The disease proved extremely severe, both in respect to the number of persons attacked, and the proportion of the cases which terminated fatally. A few years ago, the same disease, after a long period of time, made its appearance among the inhabitants of a small island situated to the west of the Long Island, and out of a population but little exceeding one hundred persons, carried off thirteen. May we not explain the accumulated fatality in all these cases by supposing that in the same manner as the different cereals flourish best when planted in a virgin soil, or at long intervals of time, so it is with infectious diseases?—the more distant their visitation, the richer the pabulum supplied for the epidemic.

Among the other causes of death recorded in the ten-year register, those from dysentery are the most numerous; no less than six being ascribed to its ravages, and two more to "green sickness." If we consider that the population at this time was only one hundred and five, we shall be struck by the large proportion, and see in this circumstance a proof of the severe intestinal irritation which is apt to follow the continued use of such a diet as salted sea-birds and coarsely-ground oatmeal.

The dysentery I should feel disposed to attribute more especially to the use of the sea-fowl, inasmuch as in many districts of the Highlands, at particular times of the year, the food of the people consists so exclusively of oatmeal, that the effects of its constant employment may be readily seen. In the majority of instances they will be found to consist either of well-defined cases of pyrosis, or some other modification of dyspepsia, associated more especially with the upper portion of the alimentary canal. Under these circumstances tenderness over the epigastrium, and a burning sensation along the course of the œsophagus, are symptoms repeatedly dwelt on. Two more deaths are entered in the words, "went over the rocks," or fell victims to their hazardous calling of snaring the sea-fowl. I was informed on the island that during the last thirty-five years five men have experienced a similar fate. Two women are said to have died in childbirth. In the report of the Registrar-General for Scotland published in the spring of 1861, the proportional number of deaths attributed to this cause in the insular districts of Scotland, is one in forty-eight, while in the case of women residing on the mainland it is not more than one in one hundred and forty-five. The frequent occurrence of death from this cause among a population in other respects healthy and remarkably long-lived, and the numerous instances in which, even though the result proved favourable, instrumental interference was called for, repeatedly struck me while residing in the Hebrides. I could never divest myself of the idea that it was mainly to be attributed to the heavy creels of peat, potatoes, and seaweed, which it falls to the lot of the women to carry on their backs. The chief weight of this creel or wicker basket rests upon the lower portion of the spine, which the rope passing round the chest rather assists in balancing, than materially lessens. If we consider the early age at which the young women apply themselves to these burdens, before the ossification of the pelvic bones is yet completed, it seems not unnatural to suppose that the bones may to some extent yield, causing a mechanical obstruction, and so contributing to the difficulties of parturition.

Another curious fact connected with life at St. Kilda, which again speaks to the influence of employment on health, is the remarkable disproportionate longevity between the male and female inhabitants. It has been frequently remarked that in the case of the men sixty years is a limit of life but rarely passed, while the women not infrequently attain an age of fourscore years and upwards. The old factor to whom I have before referred as being long connected with St. Kilda, informed me that during his prolonged acquaintance with the island he could not, with one or two exceptions, recal to mind any males who succeeded in reaching the far side of sixty years, while a green old age on the part of the women was of not infrequent occurrence. The food and dwellings common to the two sexes would naturally be the same; hence, however objectionable, they would not necessarily appear a bar to length of days when once the constitution had become hardened to their influence. The difference, therefore, must probably be set down to the calling of the men, in the prosecution of which they are frequently suspended during several hours by a rope attached round their waist. That the continued strain, both bodily and mental, to which the system is thus exposed, should tend to congestion, and eventually to organic disease of some of the more important internal organs, may readily be believed. While on the island, moreover, I myself observed in the case of two men, but little past the prime of life, marked indications of dropsical effusions, and was informed that similar indications of disease were by no means rare among the men. From tubercular diseases the inhabitants enjoy, as it appears, a remarkable immunity. One woman indeed, seventy years of age, is registered as having died of consumption, though her death might probably with more truth be attributed to chronic bronchitis. The answers I received to my inquiries respecting the occurrence of true tubercular phthisis from the more intelligent persons, convinced me that the few cases they associated with the symptoms of that disease were confined to the old and invalided. I may here remark, in reference to pulmonary consumption, the extreme rarity of which disease among the Hebrides I endeavoured to point out in an article in this 'Review' for October, 1860, that the word by which it is expressed in the Gaelic language literally signifies a "wasting" or "decay," and hence, though all cases of phthisis are probably entered in the register under that heading, numerous other pulmonary disorders are included in so general a term. We may therefore, I think, infer that favourable as is the testimony of the lately-published returns of the Registrar-General for Scotland respecting the prevalence of phthisis in this portion of the kingdom, they may still be considered as unavoidably overdrawn. Of the remaining deaths two are attributed to croup, a frequent and fatal disease among these islands; and one to cancer. Of the latter disease, a second fatal case had occurred immediately prior to my visit to the island.

Before bringing my observations on the ten-year register to a close, I would direct a passing remark on the apparently trivial circumstances to which, among rude states of society, fatal results may be traced. In many respects, on a cursory survey, at any rate, the habits of life which prevail at the Faroe Isles are not very dissimilar to those at St. Kilda. From the fact, however, of the inhabitants of the former being well supplied with materials for fertilizing their soil out of doors, the necessity of preparing a substitute by closing all the apertures in their houses, was never forced on their minds. The consequence is that they breathe both by day and by night a wholesome and renovating air; and from this cause, probably, more than any other, are, in spite of their nauseous food, and the open sewers round their huts, the longest-lived community with which we are acquainted, their annual rate of mortality being only 12·5 in every one thousand persons. In St. Kilda, on the other hand, the poisonous atmosphere tells with such fatal effects, that out of a population of 105, in the course of the ten years to which I have so often alluded, 64 deaths occurred, exhibiting a yearly death-rate of 60·9 in the 1000; nearly twice as great as that observed in the most unhealthy manufacturing districts in England and Wales.

I have thus run over some of the principal causes of death which I found recorded in the ten-year register, with a few passing remarks on the circumstances under which they arose. They contain, I think, considerable interest, as occurring spontaneously from natural though preventible causes, and as being altogether unconnected with that great family of disease to which man, in proportion as he goes more out into the world, and comes into more frequent contact with his brother man, appears to render himself obnoxious. Since the outbreak of the small-pox at St. Kilda, to which I have referred, neither that disease, nor measles, nor scarlatina have, as far at least as I was able to discover, been introduced upon the island. In regard to diseases of this class, though not directly connected with my subject, I may perhaps be allowed to remark that peculiar facilities are afforded in islands for observing the circumstances under which they arise. In large towns, and even in country districts, in many instances, it is obviously impossible to say how far any particular outbreak of sickness is due to directly contagious influences, or arises from cosmical or malarious exhalations. The assertions of patients suffering from one or other of these affections, that they have not come in contact with persons similarly indisposed, are all of comparatively little value. In large cities especially, the street-cabs, the "four-wheeled fever-traps" of the metropolis, after depositing one passenger at a hospital, and the next at a ball, add largely, it may be readily believed, to the tale of infectious disease. In islands, on the other hand, and especially in such groups of islands as the Hebrides, the advantages of isolation are aided by the fears entertained among simple societies towards the sufferers from contagious disorders. Hence it is frequently possible to estimate with a degree of certainty unknown in more populous places, how far any disease is strictly contagious, in other words, communicable from man to man, or originates in malaria or the variable conditions of the atmosphere; and this inquiry is the more important at the present time, inasmuch as there is, I think, among many sanitary reformers, a tendency to look upon almost all diseases as preventible, and the most specific contagious poisons as capable of being artificially produced. Thus, for example, among some writers, even such a disease as small-pox is spoken of as capable of being spontaneously generated where the requirements of cleanliness are systematically violated. That such, however, is not the case is both proved by the fact that this, and several other now prevalent disorders, were unknown to the ancients, among whom, could they originate in filth, the conditions favourable to their development most undoubtedly existed; and further, by their non-appearance, except when accidentally introduced, among the inhabitants of islands. I may refer to the island of Raasay, in which I myself resided for the greater portion of upwards of seven years, for an illustration to the point. It is situated immediately to the east of the Isle of Skye, and at one point the width of the sea which separates it from the latter is not more than three-quarters of a mile. During the last fifty years, as I have ascertained from elderly persons resident at Raasay, no single case of either measles or scarlatina has occurred in that island, while both these affections have, at various intervals of time, been prevalent in Skye. Moreover, some of the hamlets in the latter island, in which the disease more especially prevailed, have been those immediately opposite to Raasay. But while scarlatina and measles were, during all this time, absent from the island, sporadic cases of hooping-cough, even to my own personal knowledge, might be often observed: nevertheless, these three disorders are frequently associated in one category in respect to the nature of the poisons which may lead to their production.

Whether the causes for the trismus and heavy general mortality I have ventured to suggest be correct, may soon be satisfactorily tested.

By the visit of the *Porcupine* in the spring of 1860, the condition of the island and the manner of life which prevails among the people were more prominently brought out. A violent hurricane in the early part of the following October swept into the sea the roofs of the houses and destroyed the greater part of the winter stock of food. It was fortunate for the inhabitants that this visitation was witnessed by Captain Otter, R.N., who was stationed off the island at the time it occurred. His influence and representations caused the loss the poor people had sustained to be replaced by other and more wholesome provisions. The improvement of the landing-place likewise, carried out under his supervision, owing to the greater facility afforded for fishing, opened a new channel for employment and food. Whether as a consequence of these changes or not, I am unable to state, but from inquiries I have lately made, I find that since the first visit of the *Porcupine*, the "boat cough" has not reappeared on the island. It is not improbable that the days of the trismus are in like manner drawing to a close. During the summer of 1861, by the direction of the proprietor, the old cabins were being rapidly demolished, and neatly-constructed felt-roofed cottages rising in their stead. With all these changes we may reasonably expect that the frequent occurrence of this once fatal disease may soon be altogether stayed, and its ravages known only as a thing of the past.

Art. II.

An Analysis of Two Hundred and Twenty Cases of Pulmonary Consumption. By William Robinson Hill, M.D. Edin., Physician to the Eastern Dispensary, Bath.

(*Concluded from p. 512 in previous number.*)

Treatment.—In the treatment of cases of consumption there are two main points to be borne in mind; 1st, the general treatment of the disease, and the fulfilment of those conditions which are likely to restore and maintain the health and strength of the invalid; and 2ndly, the special treatment of those more prominent and dangerous symptoms or occasional accidents of the disease which arise in certain cases.

The first point will be attained according as we become acquainted with the hygiene and therapeutics of phthisis, and according to the accuracy of our knowledge with reference to its causes.

It is clear that those supposed to be hereditarily or otherwise predisposed to pulmonary phthisis should be protected by adequate clothing, avoiding exposure to sudden cold or wet; and hence it is manifest that those climates which are least liable to changes in temperature and weather, are the most suitable for such invalids. They should also endeavour to avoid everything of a mentally depressing nature, such as attendance upon a sick room and numerous other things that might be mentioned; and, finally, should abstain from everything that is physically debilitating, whether this consist in irregularities of life and lack of out-of-door exercise, or whether it depends upon insufficient food and the breathing of a pestilential atmosphere; the latter causes, alas! far less easy to obviate in most cases among the humbler class of patients than the former.

With regard to the therapeutics of the disease, no specific has yet been discovered for pulmonary phthisis, nor, as I anticipate, ever will be. A variety of therapeutic agents have been vaunted and tried from time to time, as if possessing peculiar efficacy in the arrest or cure of the disease, one of the most recent of which is the substance phosphorus in combination with potash and soda or lime, as the hypophosphites of these bases, which has afforded an example of the danger of the *à priori* mode of reasoning in medicine, always praiseworthy in the extreme, if correct, but prejudicial and hazardous in its results, if blindly carried out when based upon data of a merely ideal character.

The very doubtful efficacy of these salts in the cure or even amelioration of phthisical patients, has been demonstrated by the impartial investigations of Dr. Quain.* The results at which he arrived show that of 22 cases only 6 experienced amelioration, whilst in *five* of these six the improvement was merely temporary.

There being, then, at present no drug known to exert a specific influence over the disease, there would seem, as is proved by general experience, to be an indication for the administration of those which are known to invigorate the system, increase the appetite, and aid the digestive powers, and for this purpose iron, quinine, and vegetable bitter infusions will be found suitable. From general observation, I believe alkalis to be more beneficial in the majority of cases than acids. Small doses in conjunction with hydrocyanic acid and infusion of gentian, will be found of marked service in the dyspepsia of phthisis.

Cod-liver oil has now for a long time maintained a high place in the professional and popular mind for its efficacy in consumption, and deservedly so. Whether its action is therapeutic or merely dietetic, I do not undertake to say. It is remarkable that fatty and oily substances generally are not only disliked by consumptive patients, but they for the most part disagree with them, and yet this oil, after the first few doses, usually agrees well and is even relished by them, and thus the necessary element of fat, which is avoided in their ordinary dietary, is by this means supplied to the system.

The beneficial effects of the oil will be satisfactorily seen in the analysis of my cases, which may, I think, be looked upon as being very reliable, inasmuch as the oil was not expressly given with the view of testing its merits, but was supplied indiscriminately to one batch of cases, whilst it was withheld indiscriminately from another batch, a note being merely taken of the fact at the time; and on looking over the 174 cases in which such a note was taken, and comparing the general result after a certain period of treatment, averaging about three months, I find that the proportion of those who derived positive benefit during the time they were under treatment is decidedly the greatest amongst those who were treated with the cod-liver oil. What gives these observations greater weight, is the fact that the cases which had no oil, had everything else in their favour in the way of tonic medicines, sedatives, good food, &c., in common with the oil cases; in short, with the exception of the oil, they were treated to all intents and purposes in exactly the same way; and therefore I think it may be fairly argued that the administration of cod-liver oil increases the probabilities of the improvement of a consumptive patient. Thus, of 174 male and female cases, 103 had oil, and 71 had none—

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* See Lancet, March, 1860.

Of the 103, 64, or 62·13 per cent., improved.
" 71, 29, or 40·84 " "

So in examining the male and female cases separately, the only noticeable difference is that the proportion of females benefited without oil exceeds that of the males; whilst among those that took oil, the same preponderating excess of improvement is observable in each; thus—

Of 83 males that took oil 52, or 62·65 per cent., improved.
Of 20 females " 12, or 60 " "
Of 47 males that had no oil 16, or 34·05 " "
Of 24 females " 13, or 54·15 " "

This I conceive to be the most true and satisfactory method of testing the value of a remedy, and, in generalizations such as this, of course the larger the number of cases observed, the more correct the result may be presumed to be; therefore it is possible that from a larger number of facts these ratios might be somewhat altered or liable to a degree of variation under some change of conditions; but I think the numbers are sufficiently large to give good reason for presuming a tolerably near approach to the truth.

To assume anything regarding the value of any particular remedy in any particular disease from a limited number of cases, must be hazardous, and will probably prove fallacious in most cases; for putting aside the bias of the investigator, who will look at symptoms through the coloured spectacles of his own theory, there are so many other sources of error in the investigation of therapeutic actions, that the *post hoc propter hoc* argument should be adopted only with the very greatest caution and circumspection. Thus, if I were to select a few cases and argue from them, I should probably show that the most satisfactory cases of improvement were at least as frequent without oil as with it.

The male cases have also been examined with the view to ascertain whether the cod-liver oil seemed to be more effective in one stage of the disease than in another, with the following result—

Patients treated with oil—

Of those in the 1st stage, 75 per cent. improved.
" 2nd " 80 " "
" 3rd " 41·66 " "

Patients treated without oil—

Of those in the 1st stage, 50 per cent. improved.
" 2nd " 50 " "
" 3rd " 36·2 " "

. It will from this table be observed, that in every stage of disease the preponderance in favour of improvement lies with those that take oil, but that the probabilities of improvement are greatly increased in an early stage. It is clear, therefore, that if cases were selected, the benefit of cod-liver oil would appear to much greater effect than where it is given indiscriminately to all able to take it. It is scarcely necessary to add that the oil can be taken either alone or in any fluid, as it best suits the patient, and it will often be found that the stomach which ordinarily refused it, will retain it if administered in a small dose about half an hour after a meal.

With regard to the special treatment for certain of the more prominent individual features of phthisis, I shall not enter into any minute details, but merely give general results as they appeared to be developed in the observation of cases. The dyspepsia of phthisis, in addition to the remedy above mentioned, was in a few cases notably relieved by pepsine, and in several, very manifestly so, by scruple doses of bismuth, most advantageously when administered a short while before mealtime.

The pains so constantly complained of at some time or another in the chest, are generally removed by mustard or hot linseed poultices, and also the latter spread over with extract of conium dissolved in a little warm water, will be found to possess a marked topical anodyne effect. The "dragging" pains often experienced under the clavicles are most effectually removed by counter-irritation, and for that purpose a tolerably saturated spirituous solution of iodine seems to be peculiarly suited, partly because the prejudice against the weakening effect of blisters is removed, and principally because the iodine acts more immediately and with comparatively greater temporary intensity on the skin, thus being a more true and more speedy counter-irritant; and in addition, the application of the iodine has often been noticed to be followed by diminution in the frequency of the cough, by relief of dyspnœa, and in many cases by removal of the sense of tightness across the chest which is often experienced.

The profuse night-sweats to which consumptive patients are liable, and for the arrest of which the medical attendant is frequently appealed to, have not appeared to me to be amenable to the direct influence of any drug. The tinct. ferri sesquichlorid. is recommended by some physicians as the most efficacious, but the only testimony I can render on that head is that it will often be seen to fail. The oxide of zinc also will be found a very uncertain remedy, if indeed it does possess any effect in that way. What I think I have seen most benefit from is the administration of five or ten grains of gallic or tannic acid at bedtime.

Sponging of the chest with common salt and water on retiring to rest will also in many cases arrest the night-sweats most completely and satisfactorily; but as a rule, if the patient improves generally in health and strength, the night-sweats will cease without the exhibition of any drug for the purpose, indicating apparently that they depend upon general weakness, and perhaps an atonic condition of the skin, which would explain the beneficial action of the slight stimulation imparted by salt and water.

In hæmoptysis, when the amount of blood brought up is copious, it is important that the patient should maintain the upright posture in bed, and sometimes placing the feet in hot water over the side of the bed will appear, theoretically at least, to cause derivation of the blood from the chest. The sudden application of cold to the chest would seem to be injurious, by driving the blood from the surface upon the lungs, causing greater congestion of them, though it is sometimes followed by immediate cessation of the hæmoptysis, acting possibly in this way, that the slight shock causes sympathetic contraction of the bleeding vessel. Dry cupping of the chest, in cases able to bear it, is often followed by immediate arrest of hæmorrhage, acting possibly by derivation; and in one case I have seen the operation of an emetic attended by complete cessation of the hæmoptysis—a severe experiment, however, which one would be unwilling to try in many cases.

When the hæmoptysis continues in small quantities every now and then, or with every cough, sedatives are useful in allaying excitement of the circulation; and astringents, such as alum and dilute sulphuric acid, are supposed to act directly on the blood, increasing its coagulability, and should be administered; but there are some cases in which, without excessive disease of the lungs, the hæmoptysis will continue for days, baffling every device of the practitioner, and dragging even the robust-looking patient ever downwards into a state of weakness not to be recovered from, and ending only in death; very mournful cases these for the medical attendant, making him feel his insufficiency and inability to cope with disease.

The diarrhœa of phthisis, which is considered by Virchow to depend upon catarrh of the intestinal mucous membrane, and not upon the tuberculous ulceration, must be treated, according to circumstances, with opium, astringents, and alkalis.

As regards the complications usually met with in pulmonary phthisis, the following tabular statement will indicate the proportion in which they occurred among the two hundred and twenty patients:

Pneumothorax	6
Laryngeal affection	17
Fistula in ano	5
Tubercular meningitis	3[*]
Tubercular peritonitis	2
Tubercle in intestines	5[†]
Albuminuria	3
Diseased bones	3
Diseased supra-renal capsules	1

Pneumothorax.—This is a complication of comparatively rare occurrence, there having been only six instances in the 220 cases, giving a per-centage of 2·72, which nevertheless ought doubtless to be computed at rather a higher figure, as allowance must be made for the majority of the 220 having been under notice only a limited time. The symptoms leading to suspicion of its occurrence were in all the cases pretty characteristic, and may be enumerated as sudden and sharp pain tolerably localized in some part of the thorax, generally after violent coughing, attended by a marked increase of dyspnœa, quickened pulse, an anxious expression of countenance, and some degree of feverishness.

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[*] These cases have been published in detail in the British Medical Journal, Oct., 1860.

[†] Including only those cases in which the lesion was demonstrated by autopsy.

These general or rational symptoms are in the majority of instances accompanied by tolerably characteristic physical signs, consisting of a tympanitic or drum-like percussion, with often marked fulness of the intercostal spaces, and absence, sometimes complete, of the auscultatory sounds, whether natural or morbid, over a more or less extended surface. Additional evidence may also be obtained by the occurrence of metallic tinkling, or by an amphoric character being imparted to vocal resonance or to distant respiratory sounds.

Easy as the diagnosis of pneumothorax is in a typical case, there are difficulties sometimes presented to the statement of a decided opinion —as, for instance, when, by reason of neighbouring pleuritic adhesions, the cavity in the pleura containing air is very circumscribed; and, on the other hand, a large pulmonary vomica will often yield tympanitic percussion and amphoric phenomena, compelling the physician to form his opinion according to general symptoms.

As to its cause there can be but one opinion, that it is produced by the rupture of a vomica in consequence of some sudden strain or jerk, causing the distending and confined air to seek a ready exit through its thinned walls where unsupported by pleuritic adhesions, the abundance and firm nature of the latter in pulmonary phthisis being the great safeguard against its more frequent occurrence.

Of the six patients, four died; one on the third day, one on the eleventh, and two about two months after the attack; one left the hospital about two months after his attack with very small prospect of a prolonged survival, whilst the sixth recovered not only from the immediate danger of the pneumothorax, but left the hospital after six weeks considerably improved in general health.

In all there had been physical evidence of pulmonary tubercular softening, and in four, cavities were known to exist before pneumothorax occurred. The pneumothorax was on the left side in five of the cases, and on the right in the remaining one, with one exception corresponding with the side on which there was evidence of the greatest amount of disease. As to *sex*, four were males and two were females.

In three of the four fatal cases the diagnosis was confirmed by autopsy, and the fourth lived two months after the occurrence of the attack and left the hospital three days before death, so that no examination was obtained. The case that made such a good recovery is sufficiently interesting to warrant a more particular notice, showing, as it does, the complete recovery from an accident of an usually fatal nature to phthisical subjects. The patient was a female, aged twenty-six, admitted with all the general symptoms of consumption; cough, night-sweats, loss of weight and strength. Pulse 130. Respirations, 28 per minute, with the physical signs of softening at the left apex, the right lung being healthy. Six weeks after admission she complained of pain under the left scapula, which was not relieved by a blister; and on examination, the percussion was found to be quite tympanitic over the left chest anteriorly, the respiratory sounds were distant, and distinct metallic tinkling was heard under the left clavicle. The pulse was 140 per minute. She gradually recovered from this condition, the tympanitic and amphoric phenomena disappearing in the course of six weeks, whilst faint respiration was heard all over the left chest, without any crepitation in any part. During the latter half of this period she gained six pounds in weight, and finally left the hospital, declaring she felt quite well. There remained an occasional slight hacking cough without expectoration; no dyspnœa, even when ascending stairs, and the pulse was reduced to 108, the respirations being 20 per minute.

The most appropriate treatment in cases of pneumothorax seems to consist in a local application of warm cataplasms and sinapisms, with opiate draughts, combined with small doses of antimony, if much fever exists. Puncture of the chest will in some cases afford great temporary relief by permitting the escape of the air contained in the pleura, thus diminishing the dyspnœa so much that a patient will sometimes beg for its repetition. It can only, however, be regarded as a palliative measure.

Laryngeal phthisis.—This may be regarded as one of the most unfavourable complications of pulmonary phthisis, offering, as it often does, yet another obstacle to the maintenance of the patient's strength, by mechanically preventing the ingestion of nutriment, in addition to its own morbid influence on the constitution. I have had the opportunity of observing this affection in fourteen males and three females, giving a per-centage of the frequency of its occurrence, in the two sexes together, of 7·7; whilst taken separately, it would seem that the male subject is more liable to its attack than the female—the per-centage of cases in the former sex being 9·2, and of the latter only 4·4. It is also a complication that manifests itself for the most part in the latter stage of pulmonary disease, for of the seventeen cases there were only two in which the laryngeal affection appeared to have commenced before softening had occurred in the lungs. In the great majority the pulmonary cavities had already been formed, and in *all* they were probably formed within a year of the commencement of aphonia. Within the same period, also, about one half, or fifty per cent., of the laryngeal cases completed their term of existence, showing that the duration of pulmonary cases is considerably abbreviated by the supervention of laryngeal phthisis. The rapidity with which the laryngeal structures are destroyed is evidenced by two cases, in both of which the pulmonary lesions were tolerably extensive. In one, the aphonia commenced only three weeks before death, and the post-mortem inspection showed thickening of the laryngeal mucous membrane and ulceration of the left vocal chord. In the other, according to the patient's account, the voice had been affected only four weeks previous to the time I saw her, and a week after, the autopsy revealed considerable ulceration of the epiglottis and upper part of the larynx, with rough and thickened vocal chords.

Of the non-fatal cases, some received slight benefit from treatment, but only two were really improved in a satisfactory manner. One was a man with a vomica in the right lung, who had had pulmonary affection probably for five years. He was treated with cod-liver oil internally, and the application of strong solution of nitrate of silver to the fauces and upper part of the larynx. After eleven weeks' treatment the voice was improved, the patient had gained 6½ lbs. in weight, and the vomica became quite dry. The other was a young man who had had pulmonary disease for two or three years, and partial aphonia for a year. The application of blisters and strong solution of iodine externally removed all the soreness of the throat, and the voice improved considerably. Of internal local applications, I believe that the solution of nitrate of silver, useful as it is in cases of chronic inflamed fauces, possesses little virtue in real laryngeal ulceration, partly perhaps because in nine cases out of ten it never reaches the diseased part, though it may be supposed to do so. A free application of tannic or gallic acid dissolved in glycerine is often of service in allaying irritation and soreness; but if any permanent good can be done, I feel confident that the most effectual and easiest mode of obtaining it is by the application of blisters or a blistering solution of iodine to the throat externally on each side of the pomum Adami; and sometimes the application of two or three leeches exerts the most marked benefit, even in extreme cases, as is evidenced by the following case:—G. O., aged twenty-nine, with a vomica in the right lung, and evidence of tubercles in the left, had had hoarseness of voice for two months, and was getting worse. Solution of nitrate of silver was diligently applied internally without relief being obtained; the disease gradually advanced in the larynx, which became so painful that the patient was finally unable to swallow even liquids. He lost strength rapidly, and was confined to bed from weakness. Two or three leeches were then applied to the throat externally, with the result of relieving the pain immediately, and next day he was able to swallow. From that period he began to gain strength, and in a month's time could walk out for two hours daily.

Fistula in ano.—On the subject of fistula I have only to remark, that whilst in surgical books it is said to be very frequently associated with pulmonary phthisis, it will be seen by the above table that such is the case only in a very small proportion of phthisical patients. Dr. Cotton's observations have also led him to the same conclusion, that "the common opinion regarding its frequency is incorrect." At the same time it may be equally true, that of the fistula cases that come under the observation of the surgeon, a considerable proportion of them are victims of phthisis, though this does not warrant the converse statement, that fistula in ano is a frequent complication of pulmonary consumption. All the five cases were male patients.

Prognosis.—In conclusion, I have a few observations to make on this subject, the practical importance of which must be fully apparent, as it is the one thing on which information is most anxiously and eagerly demanded from the medical attendant, and on which in many cases much may depend upon the correctness of the opinion given. It is a subject which demands considerable study from the very nature of the difficulties it presents, as seen by the observation of the great diversity in many points that exists in the course of any two cases; hence accuracy in prognosis can only be obtained in the way of generalizations, from which a certain ratio of divergence must always be expected, that will continually diminish according as we attain to perfection of knowledge and certainty on the subjects of the etiology and pathology of the disease in its different modes of manifestation.

The value, therefore, of any facts obtained from this analysis of cases must be estimated according as they assist in the development of this special subject.

Under this head, the points which it is desirable to determine are, the average or usual duration of a case of pulmonary phthisis, the conditions under which prolongation of life is attained, and those under which patients succumb before reaching the ordinary period of duration; and finally, how far the symptoms in any individual case enable us to predict with an approach to accuracy its probable duration and prospect of present improvement.

As regards the duration of a case, it is well known that the extreme limits are separated by a wide interval. That form which commences as an attack of pneumonia and terminates in the pathological condition of "cheesy hepatization," may bring its victim to the grave within a very short period. Cases also of "acute tuberculosis," and those which are hastened to their end by any fatal intercurrent attack—such as meningitis—may of course be of much shorter duration than ordinary cases, and therefore should be omitted in endeavouring to ascertain the average duration of the latter class, of which the shortest that has come under my notice is that of a lad, aged nineteen, who, as far as could be ascertained, was in good health and quite free from cough four months before his death. One of the most protracted cases I have observed was that of a man, aged fifty-one, who stated he had had cough for twenty years. This, of course, is no decisive evidence of tubercular pulmonary affection having existed so long; but he stated he had had an attack of hæmoptysis at least ten years before I saw him, and the pathological appearances at the autopsy confirmed the opinion of the distant date of the commencement of the disease; and in another case I have positive evidence of at least ten years' duration of the pulmonary affection.

In estimating the *average* duration of a case of phthisis, a difference must of course be made between those who succumb only from the mere constitutional affection and pulmonary lesion, and those whose end is accelerated by the concurrence of some additional tubercular or other disease or accident—such as laryngitis, pneumothorax, hæmoptysis, &c. Of all the cases I have seen to their termination, an average of 1·93, or just two years, is yielded, which is increased by half a year when only those uncomplicated cases which run a natural course are taken into account. The average of two years for all cases taken together is the figure arrived at by most investigators, as may be ascertained by reference to the abstract of Dr. Pollock's paper on Prognosis in Phthisis, in the 'Medical Times and Gazette' for September, 1859. Dr. Pollock himself inclines to place it higher, on the ground that cases are not always recognised at their commencement, an opinion with which I quite agree; and when it is remembered that in most cases we are dependent on the patient's own account merely in fixing the date of the commencement of his illness, I think that an average of two years and a half, as above mentioned, may be considered as the lowest figure by which to indicate the duration of uncomplicated pulmonary phthisis.

With regard to the conditions under which consumption assumes the chronic form, on examination of the notes of 48 such male cases, I find that more than half had passed the age of thirty, whereas, of all taken together, not much more than one-third had attained that age; 67 per cent. were free from hereditary taint, whereas the ordinary per-centage of such is only 58 among the male cases generally, and in 57 per cent. the pulse was below 100 beats per minute; therefore, a case may be expected to be chronic in a person of a more advanced age, in those free from hereditary tendency to the disease, and in those where there is an absence of hectic symptoms. On the other hand, cases may be expected to have an early termination where the reverse of these conditions obtains, and also when complications supervene—such as laryngeal disease, tubercular peritonitis and meningitis, copious hæmoptysis or pneumothorax, which in the 220 cases occurred to the amount of fifteen per cent., and which must hence be considered as reducing in the same proportion the probabilities of a case attaining to the, in other respects, natural period of duration.

In a practical point of view, the most important thing for the medical attendant to be well acquainted with, is the probability of present improvement in any individual case at any time that it may present itself to his notice, to assist him in which, amongst other things, the chief elements for consideration may be enumerated as the question of emaciation, whether the patient have been losing or gaining weight recently, whether night-sweats are present or absent, and the condition of the pulse. How far the latter may be considered as a guide will be seen by the following statement: of 108 patients (male and female) in whom the pulse was above 100 at the time they came under observation, only 32·4 per cent. improved during the time they were under treatment; and of 96 in whom the pulse was at or below 100 per minute, 63·6 per cent. improved. Profuse nocturnal diaphoresis will usually be found in concurrence with rapidity of pulse, and indicates in the same manner in a general way advance of the pulmonary affection. Sixteen such cases, in which the diaphoresis continued unchecked in spite of treatment, got rapidly worse or died within a comparatively short period, whilst in nearly every case of cessation of the night-sweats a corresponding improvement in other respects took place.

The advance or arrest of emaciation may, as a rule, be looked upon as a very good indication of the condition of the patient, a gain betokening improvement; and a loss of weight indicating in almost every case advance of pulmonary disease. The emaciation so constantly attendant upon tubercular disease of the lungs is probably due to two circumstances—1st, defective nutrition; on account of the accompanying vitiated state of the digestive organs, and the retention of fat by the liver, evidenced by that fatty condition of the organ so frequently found in connexion with pulmonary phthisis, which is the result of resorption and retention of fat, and not of a fatty degeneration in its ordinary sense;* and, 2ndly, the decreased pulmonary surface available for the aëration of the blood: for if in the healthy body a certain amount of lung-tissue is provided, through which the blood circulates in a given time, in order to maintain the due interchange of elements necessary to the waste and repair of a certain weight of organized tissues, we may fairly conclude that a decrease in the weight of the body is necessarily attendant upon a decrease in the amount of permeable pulmonary tissue. To show how important and useful a criterion we have in the observation of the emaciation or otherwise of the patient, I may mention that of 46 patients who experienced a loss in weight of from two pounds and upwards, only 5 showed any symptoms of improvement generally, all the rest having become worse, in most instances with a degree of rapidity commensurate with the loss of weight; whereas, in 63 patients who gained weight to any marked amount, there were decided symptoms of general improvement in all, with more or less disappearance of physical signs in 38. In the other 25 the physical signs advanced, notwithstanding improvement in general health—that is, passed from the first to the second stage, or from the second to the third stage—which circumstance leads me again to notice the error of considering these conditions of lung as stages of the disease; for if a patient has sufficient strength of constitution to sustain the demand during the process of excavation, he is oftentimes, after the formation of a cavity, placed in a condition not only of greater ease and freedom from cough, but also much more favourable to the prolongation of life.

The occurrence of hæmoptysis is also said to be a symptom of chronicity, and such would appear to be the case from the fact, that of 40 male and female patients in whom it took place, in some even to a considerable extent, an average duration of 2·6 years is yielded; whilst of an equal number who never had hæmoptyis, an average of only 1·8 was the result. These statements may, however, be interpreted inversely, that those in whom the disease has continued longest are in the same proportion more likely to have had an attack of hæmoptysis at some period or other, though I think that in most of the hæmoptysis cases this symptom usually shows itself at an early stage of the disease, and therefore the idea of attendant chronicity may be lawfully used to allay the excitement and terror of those who are the subjects of these hæmorrhagic attacks.

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* See Virchow's Cellular Pathologie, &c., p. 300 et infra.

In females we have an additional element in the catamenial functions, which we can call to our aid in forming an opinion as to the probable issue of a case, and its importance in prognosis will be seen by the accompanying groups of cases. Of nine patients in whom the disease had existed for a period of twelve months or more, and in whom the menstrual functions continued in a normal state, only two got worse whilst under treatment, an average of only about 22· per cent. Of twenty-five patients in whom there had been evidence of pulmonary affection for half a year and onwards, with absence of the catamenial discharge for at least three or four months, twenty lost ground with more or less rapidity, an average of 80· per cent.; and the most unfavourable cases, or those least amenable to treatment, will be found amongst those in whom the catamenia disappears nearly at the same time, or very shortly after, the pulmonary affection is manifested. Its reappearance in any case will, I believe, be always found coincident with some general re-establishment of health; for though its absence must not be considered incompatible with improvement, recovery under these circumstances very seldom occurs in any marked degree.

From these facts, then, we may conclude that for any individual case free from the more dangerous complications, the average duration of two and a half years may be expected; and this period will be more or less prolonged, and a degree of present improvement may be anticipated, according as the case in question presents those symptoms which are indicated above as warranting a favourable prognosis.

Art. III.

Vegetable Morphology: its History and Present Condition. By Maxwell T. Masters, F.L.S., M.R.C.S., &c., Lecturer on Botany, St. George's Hospital.

The basis of botanical science, the most important of all its subdivisions, that one, indeed, upon which all the rest depend, is morphology, or the accurate investigation and observation of the plant and its varied parts, in order to obtain an insight into their mutual relations and significance, and by these means to discover the principles of their construction. The great value of some hypothesis as furnishing a centre round which the ever-increasing host of newly observed facts may be gathered, and their import explained, leads us to add a word of caution which may perhaps appear superfluous, but which the experience of every day proves is not so. We have no right confidently to assume that in laying down so-called "laws," philosophers have actually discovered the plan by which it has pleased the Creator to build the universe or fashion its inhabitants. It more befits our ignorance to acknowledge that such expressions as unity of type, special adaptation, and the like, are merely relative; that they serve our purpose of collating facts and rendering them intelligible, and that by their means we do really arrive at a clearer insight into the truth. Mr. Buckle, in his 'History of Civilization in England,' vol. i.,

p. 28, alludes to this in the following pertinent words:—"A law of nature being merely a generalization of relations, and having no existence except in the mind, is essentially intangible; and therefore, however small the law may be, it can never admit of exceptions, though its operation may admit of innumerable exceptions. Hence, as Dugald Stewart rightly says, we can only refer to the laws of Nature by a sort of figure or metaphor; this is constantly lost sight of, even by authors of repute, some of whom speak of laws as if they were causes, and therefore liable to interruption by larger causes, while other writers pronounce them to be delegated agencies from the Deity."

The principal points in the doctrine of vegetable morphology are so perfectly well-known that it is unnecessary in this place to dwell at any length upon them, but there are many circumstances connected with the history of vegetable morphology which seem to be imperfectly known even to professed naturalists. It is hoped, therefore, that a short account of the progress of the doctrine, embodying likewise some of the facts connected with its present condition, may not be entirely unacceptable, even though no pretensions be made to completeness in these respects.

The first indications of what we now call morphology are doubtless to be met with in the writings of Aristotle and Theophrastus; take for instance the following passage from Aristotle—"As a general rule, a plant possesses potentially both root and stem in every part;" or this from Theophrastus—"Some organs exist only according to analogy, and others, though the same, yet exist in a different manner." Perhaps the most remarkable passage of this sort is one derived from the writings of Nicholas of Damascus, who was probably indebted to Aristotle for the idea. Nicholas was a poet, historian, and statesman, and was sent in the latter capacity as ambassador to Augustus by Herod the Great, B.C. 5. He wrote two books on plants, compiled chiefly from older authors, especially Aristotle, to whom, indeed, until disproved by Meyer, the work had always been attributed, and as such had been commented on by Albertus Magnus. "The wise men among the antients," says Nicholas, "regarded all leaves as fruits, but they thought the amount of moisture was too great to allow of their attaining maturity and solidity by the influence of external heat and evaporation produced by the sun. The undigested moisture therefore takes the form of leaves, and the objects for which the leaves are produced are merely that the sun may draw off the moisture through them, and that they may protect the fruit from his heat. Thus the leaves are also, properly speaking, fruits, and as said before, it is only the moisture ascending through them which converts them into leaves. This is the explanation of the often fruitless olive-trees, for as soon as digestion is effected, the undigested moisture separates first of all from the more delicate portion and forms leaves, while the digested portion becomes flowers, and when in the autumn this part is matured, the fruit is produced and makes its appearance at the end of the stem at the place appointed for it by Nature." This, which Meyer calls an anticipation of the metamorphosis, is of interest in a purely physiological point of view. Not only is there here inferred an intrinsic identity between the leaf and the fruit, but that identity is asserted for the physiological reason that the sap, instead of being retarded and reserved in the tree for the purpose of aiding in the formation of the fruit, is hurried into the leaves, and there evaporated by the action of the sun. The general principles here involved are strikingly in accordance with those received and promulgated by modern physiologists. If evaporation and transpiration be allowed to go on to too great an extent, the leaves indeed may become larger and more numerous, but at the expense of the fruit. Check the undue exercise of the functions of the leaf, retard the flow of the sap by removing a ring of bark, and the sap is thereby accumulated and concentrated, and hence serves to increase either the quantity or the quality of the fruit, or it may be both. Thus, in this noteworthy passage the morphological assertion is corroborated by physiological observation and reasoning, a method of procedure of the highest possible value, and as such laid down as a canon to be followed whenever it is possible to do so, by De Candolle, Jussieu, and all eminent botanists. From this time up to the time of Albertus Magnus there was little or no progress, but on the contrary, much retrogression in botany, as in other branches of learning. Albert, a Dominican friar, who died in 1280, was, for the age in which he lived, an intellectual giant, and possessed enlarged ideas on botanical as on other subjects. Speaking of life as manifested in plants, he says—"It is only evidenced in the functions of growth, of imbibing nourishment, and of reproduction, and with these circumscribed limits correspond the nearly homologous nature both of the external and internal parts of plants, and the powers which they possess of reproducing their kind from any part whatsoever, as well as by seed."*

It is not necessary here to do more than mention the numerous systematic writers of the sixteenth and seventeenth centuries, as the way in which they treated their subject has little bearing on the theory of morphology. It will suffice merely to say, that they in general speak of what we now call petals as "*folia.*"

Joachim Jung, professor at Hamburgh, who died in 1657, has shown in his works that he possessed opinions on the subject of morphology which would meet with acceptance in the present day, in evidence of which may be cited the following extracts from his 'Doxoscopia." He thus defines the stem—"*Quicquid florem fert aut fructum est caulis.*" In alluding to what we should now call compound flowers with tubular florets, he says, "*Sunt et flores quidam falso staminei dicti, qui rectius ex flosculis cavis sive fistulosis arcte farcti, sive tubulosi, dicerentur, staminei dici possunt qui solis staminibus constant.*" In the chapter on the flower, after stating that leaves are not only flat, and have definite breadth, but are occasionally hollow and cylindrical, he continues—"*Ita quoque inter folia florea recenseri possunt et recensentur etiam quæ interminatam sive in se recurrentem habent latitudinem.*" So also in the 'Isagoge Phytoscopica," published after the author's decease by J. Vagetus, the distinction between root and stem, the difference between leaves and foliaceous branches, the transition from the ordinary leaves to the "*folia floris,*" and the true nature of the involucre, are all clearly explained. As for the flower, it consists, says he—1mis. *Vel ex meris planis foliis, figurâ ac situ similibus uti flos Tulipæ.* 2dis. *Ex foliis meris planis, sed figurâ ac situ diversis ut flos Iridum Gladioli.* 3tiis. *Ex foliis quasi planis et corniculis ut in Aquilegiâ.*"

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* The preceding quotations have been derived for the most part from Meyer's History of Botany—a most valuable contribution to the History of Botany.

After this time, and until 1759, when Wolf published his 'Theoria Generationis,' there seems to be little worthy of record in the history of morphology. The very remarkable works of Wolf have been so strangely neglected, that their great merits are even now hardly appreciated. The most noteworthy facts relating to the treatise just mentioned, are the researches into the development of the flower; herein he opened up a new line of investigation the full importance of which has not been sufficiently recognised till comparatively recently. He describes the internal structure of buds as consisting of a cellular substance to which the rudiments of the leaves are to be considered as appendicular organs. He attributes the formation of the flower to an arrest of growth arising from diminished vegetative action. The order of development in the successive whorls of the flower is explained by Wolf in a manner not quite in accordance with modern researches on this subject; nor is his hypothesis, that the stamens are to be considered as buds axillary to the petals, at all consonant with their true position with reference to the petals. This notion, however, somewhat modified, has of late years been supported by Agardh and Endlicher.

In reference to the metamorphosis of plants, neither Linnæus nor Goethe have expressed themselves so clearly as does Wolf in an essay on the Development of the Intestinal Canal in the Chick, published in the Commentaries of the St. Petersburg Academy of Sciences, 1766. After speaking of the homologous nature of the leaves, the sepals and petals, an homology consequent on their similarity of structure and identity of origin, he goes on to state that the "pericarp is manifestly composed of several leaves as in the calyx, with this difference only, that the leaves which are merely placed in close contact in the calyx, are here united together;" a view which he corroborates by referring to the manner in which many capsules open and separate "into their leaves." The seeds, too, he looks upon as consisting of leaves in close combination. His reasons for considering the petals and stamens as homologous with leaves, are based upon the same facts as those which led Linnæus, and, many years afterwards, Goethe, to the same conclusion. "In a word," says Wolf, "we see nothing in the whole plant, whose parts at first sight differ so remarkably from each other, but leaves and stem, to which latter the root is referrible." "If," he continues, "the organs of a plant, with the exception of the stalk, are thus referrible to the leaf, and are mere modifications of it, a theory, showing the manner in which plants are generated is obviously not a very difficult one to form, and at the same time the course is indicated which we must follow in propounding it. It must first be ascertained by observation in what way the ordinary leaves are formed, or in other words, how ordinary vegetation takes place; on what basis it rests, and by means of what powers it is brought into existence. Having gained this knowledge, we must investigate the causes which so modify the general mode of growth as to produce, in the place of leaves, the parts of the flower." The basis of all these modifications he attributes to a gradual diminution in the powers of vegetation. It may here be remarked that during the interval between the publication of the 'Theoria Generationis' and that of the essay in the St. Petersburg 'Transactions,' to which reference has just been made, Wolf seems to have abandoned the notion that the stamens were buds peculiar to the corolline leaves, for in the latter essay he refers the stamens to leaves also; and it is worthy of notice that while the 'Theoria Generationis' was published one year before the 'Prolepsis Plantarum' of Linnæus appeared, Wolf's essay in the St. Petersburg 'Transactions' was not printed till six years after the publication of the 'Prolepsis.' These facts render it rather difficult to assign the priority either to Wolf or to Linnæus; but when we consider that Wolf's first essay was published before the 'Prolepsis,' that his second essay was an expansion of the first, that there is no proof that he was under any obligation to Linnæus, or had even perused the 'Prolepsis,' and when we further consider (as all physiologists will admit) the far higher scientific merits of Wolf's essays on this subject than of that of Linnæus, we need not hesitate to give Wolf the merit of having been at once the pioneer and the exponent of the metamorphosis.

Of Linnæus' essay in the 'Prolepsis,' it is not necessary to say much, as it has been more read and is more generally known than the writings of Wolf. The 'Prolepsis Plantarum' was published at Upsal in 1760 among the 'Amœnitates Academicæ.' In this essay, published in the name of his pupil Ullmark, Linnæus refers all the parts of the flower to leaves, and this view is established by the consideration of numerous instances in natural as well as in monstrous flowers, where the parts of the flower are either like ordinary leaves, or are replaced by them, which could not be the case were the two organs not homologous, for, says he, "the liver cannot become the heart, nor the heart the stomach." The greater stress is laid on these particulars, because it has been said that Linnæus made no investigations in this subject; and his essay in consequence has been unjustly depreciated. The truth is, the essay is based upon original researches and incontrovertible facts, and by their aid a similar result was arrived at as Wolf had attained to, from the study of progressive development, over and above the observations that he made of a similar nature to those of Linnæus. But Linnæus associated with his facts, hypotheses which could not be satisfactorily borne out, hypotheses which Wolf, with his knowledge of the internal structure of plants, and his enlarged ideas on physiology, would never have originated. The petals, for instance, were considered to be buds axillary to the sepals, the stamens, again, as buds axillary to the petals, and so on. But the members of each successive whorl of the flower are not axillary one to the other—to say nothing of the improbability of one single leaf being the representative of a bud. The asserted relationship, too, between the whorls of the flower and the cortical and woody layers of the stem is equally untenable. The whole subject is, moreover, complicated and obscured by the fanciful theory of anticipation, by which he supposed a flower to be a shoot, modified as to its leaves and hurried on in its growth, so that the growth of five years was, in the case of the modified shoot or flower, compressed into one year. From these impeding circumstances is it, no doubt, that so little attention was paid to this essay, and the foliar nature of the floral whorls, although pointed out by Wolf and again by Linnæus, was not received as an admitted fact till after the publication of Goethe's essay, to be presently mentioned. Before quitting the subject of the 'Prolepsis,' it may be mentioned that Linnæus, speaking of the buds, compared them to so many distinct generations, as in the similar instance of the *volvox globator*, thus expressing an opinion quite in accordance with the modern doctrine of metagenesis.

Goethe's famous essay on the metamorphosis of plants was first published in 1790, thirty years after the publication of the essays of Wolf and Linnæus. Mr. Buckle has, however, drawn attention to a passage in the 'Italianische Reise,' which shows that Goethe had glimpses of the discovery in or before 1786. Much misapprehension has arisen as to what degree of merit is really due to Goethe, for while some unhesitatingly ascribe to him the merit of being the foremost labourer in this field, others as unjustly deny him all praise, and say that he was forestalled by Linnæus. It is indeed true that in many points he was anticipated by previous writers, especially by Wolf, but no one can peruse Goethe's essay without acknowledging that with him the idea was an original one. From Linnæus he directly derived but little, from Wolf nothing. In justice to Linnæus, however, it must be stated that Goethe acknowledged that Linnæus had had a greater influence on his mind than any one save Shakspeare and Spinosa, and that he not only prefixed to his essay a quotation from the 'Prolepsis,' but also devoted a chapter to its consideration. A perusal of this chapter will bear out what is above alleged regarding the originality of the idea with Goethe, as may be further seen in the historical sketch of his botanical studies, which he published many years after the original publication of the essay. Of course we cannot overlook the indirect influence which the writings of Linnæus and other botanists during the thirty years' interval which has been mentioned, must have exerted on Goethe's mind, still it is evident, from his lack of early scientific training, as well as from an impartial consideration of the essay itself, that Goethe framed his theory from original independent research and thought, and it may indeed have been as Turpin remarked, that the freedom from the dust of schools may have contributed to the development of the idea of the organic unity in plants, because its originator was unshackled by the details of a multitude of ever-varying forms, and by a terminology often superfluous, because expressing the same thing under different names. We may safely admit that had it not been for Goethe's clear enunciation of what Schleiden terms the only really scientific principle which botany can be said at present to possess, neither the essays of Linnæus nor those of Wolf would have sufficed to establish the theory on so firm a basis as that on which it now stands.

It is to the elder De Candolle that science is peculiarly indebted for demonstrating the value of Goethe's essay. No reader of this treatise will fail to recognise how much that has been written on the subject in our own time, by authors of far greater botanical knowledge than Goethe, has been anticipated by the great poet. Compare, for instance, the alternate expansion and contraction of the lateral organs of the flower, on which Goethe lays so much stress, with A. Braun's theory of the rejuvenescence of plants, and with the series of vibrations in the metamorphosis of which he speaks. Many similar instances might be adduced. Goethe, too, distinctly recognised the true nature of the so-called nectaries, when he stated them to be intermediate stages in the passage of petals into stamens, and explained in a similar way the "corona" of passion-flowers, of *Narcissus*, and other organs not distinctly referrible to the corolline or to the staminal whorls.

This notion is opposed to that of Schleiden, who believes these formations to be secondary productions from the petals, and not independent foliar organs.* And this is no doubt true in some cases, as shown by organogenic researches, but it does not invalidate the truth of Goethe's opinion in other cases, as witness the following instances which seem to favour Goethe's views. The flowers of *Narcissus montanus* growing in the Botanic Garden at Oxford constantly produce anthers on the margin of the *corona*, and in some instances the *corona* is divided into distinct filaments, each surmounted with an anther, a deviation from the ordinary arrangement which seems to show clearly that the *corona* is composed of a number of confluent petaloïd filaments whose anthers are generally suppressed; an opinion first enunciated by Dr. Lindley, and which is surely confirmed by the close affinity between *Narcissus* and *Pancratium*, in the flowers of which latter plant the stamens are connected together at their bases by a petaloïd expansion. The petaloïd scales of *Brodiæa*, of *Vellozia*, and the petaloïd filaments of *Allium*, may all be cited in support of this notion. Some of the rays of the crown of the passion-flower have been likewise observed to be replaced by anthers, while in *Passiflora murucuja* the rays are actually combined into a cup like that of *Narcissus* or of *Melia*. In the case of *Saponaria*, the writer has shown that the scales on the petals of these flowers are in reality referrible to the adhesion of two antherless filaments.†

Goethe says, in support of his opinion—"If the formation of the petals is the result of expansion, that of the *corona* is due to contraction, as is the case with the stamens." Goethe also clearly showed the nature of the so-called nectaries of *Aconite* and *Nigella*, when he referred them to the petals, but in this he was, as has been already stated, forestalled by Jung.

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* A similar opinion is held by M. J. Gay, the most recent writer on this subject.—Ann. des Sc. Nat., 1859.

† Journal of Proceedings, Linnæan Society, vol. i. p. 159.

Goethe's explanation of the formation of the fruit is so well known that little need be said concerning it, especially as from De Candolle downward the writers on the fruit have compiled their systems of classification on the basis laid down by Goethe. It is here necessary to remark, that Wolf's explanation of the structure of the compound fruit is quite as explicit as that of Goethe, and based upon exactly similar facts, but the writings of Wolf were not known to Goethe till long after the first publication of his essay; and although Linnæus had asserted the foliar nature of the *Pistillum*, from having seen the style of *Carduus* replaced by two green serrated leaflets, there is nothing in the writings of Linnæus so explicit as to the constuction of compound fruits as may be found in Goethe's memoir. The nature of buds, their homologies with seeds, the phenomena of vegetative reproduction and growth as evinced in the formation of buds, and the successive production of node after node, are all clearly explained by Goethe, who on this account also may be considered as the pioneer of that theory of rejuvenescence which Braun has brought to such perfection. There are certain objections which have from time to time been raised against Goethe's theory, as well as to those of other writers before and after him; but it will be more convenient to mention these in conjunction with the present condition of vegetable morphology than separately.

A very simple method of grouping the various modifications met with in the conformation of plants in general, though one not usually adopted, is to classify the several organs or parts of plants into groups, according as they belong to the alimentary, the tegumentary, the reproductive, the fibro-vascular, and the appendicular systems. A very slight acquaintance with plants will suffice to show that many of their organs might as well be included in one as in another of these groups, and in truth might without impropriety be placed in both; nevertheless the arrangement will be found a convenient one, and probably as little liable to objection as any arrangement of the kind can be in the present state of our knowledge, or rather ignorance of the relations between structure and functions in plants.

The alimentary system contains the organs devoted to the nutrition and growth of plants, including multiplication by "*gemmæ*" or buds. The tegumentary or cortical system is sufficiently explained by its title, it corresponds to the exo-skeleton of animals. The reproductive system needs no explanation, the fibro-vascular system corresponds to the endo-skeleton of animals and might be merged with the first group; the last group, the appendicular, is one inserted here more in deference to generally received opinion than from a conviction of the real necessity of establishing such a system as distinct. We shall endeavour to show as we proceed, how little ground there is for retaining such a distinction, and how the organs so classed might more justly be referred to some of the other groups just mentioned.

The groups are arranged according to their relative frequency and importance; thus, all plants, however simple, have an alimentary system; in fact, the simplest conception we can frame of a plant is, as a gelatinous mass of matter endowed with a mysterious principle, vital or physical, it matters not which for our present purpose, manifesting itself in the nutritive functions performed by the humble organism. The zoospores of some of the cryptogamic plants are at first mere masses of protoplasm—e.g., *Vaucheria*, which have a separate existence for a time, an existence too marked by the function of active locomotion, a function denied to plants higher in the scale. These zoospores speedily become invested by a membrane which completes the "cell" by forming the cell wall. Indeed, it is doubtful whether a cell wall of membrane, differing in chemical constitution, and notably in physical and vital endowments from the primary nitrogenous mass, be not essential to all plants in their adult condition.* The zoospore does not fulfil its ultimate purpose, the formation of a new plant, till it has become invested by the cell wall. Hence it may be assumed that the alimentary system and the integumentary system are represented in all plants in their adult condition, although perchance in some of the lowest plants, for a time, the organism may live without an integumentary cell-wall.

The integumentary system which is universally present in plants, presents itself, as we have seen, in the lowest plants, simply as the cell-wall. Even in this state it undergoes morphological changes; in some cases it becomes soft and gelatinous, and in those cases where several cells are aggregated together, the walls of the cells become inseparably fused, so as to form an integumentary layer common to the whole plant. The peculiar markings on the *Desmidieæ*, according to Mr. Tuffen West, are due to the bulging of the cell-wall at regular intervals in the same manner as the simpler kinds of hairs, and the velvet-like surface of the petals of flowering plants are produced. In flowering plants in general, we have the integumentary system represented by the epidermis and by the cortical layers, structures of much greater complexity than the integumentary system of Thallogens; but even in *Algæ* we have a remarkable foreshadowing of the cortical layers of higher plants, while it can hardly be doubted that a relation of strict homology exists between the gelatinous envelope of some *Algæ*, such as *Palmella* before mentioned, and the cuticle, which overlies the epidermis of flowering plants, and which brings to mind the basement membrane on which the epithelial cells rest, save that the position of the latter with reference to the membrane is reversed, so that the comparison between the anterior elastic lamina of the cornea and the cuticle of plants, would be more apt, though it must be distinctly understood that nothing more than an apparent relation is intended in the comparison. The variations and increased complexity of the integumentary system are perhaps due to, or at least they are co-existent with, the altered circumstances under which these plants have to exist.

For anything we yet know to the contrary, the physiological process of reproduction is essentially the same in the highest as in the lowest plants. In the latter, fructification takes place in any or all the cells, apparently indifferently, but as we proceed higher in the scale we find certain cells specially assigned for this process, and even special organs placed in more or less determinate positions, adapted for the due performance of this function. This distinction in function and local position is one of the first links in the chain reaching from the lowest to the most highly endowed plants. But although in the last mentioned plants there is greater complexity of structural arrangement, and the newly-formed plant partakes also of a higher degree of complexity, yet the formation of a spore and its fecundation by the spermatozoïds, does not appear to differ essentially from the formation of a germinal vesicle and its fecundation by the agency of the pollen cells. In speaking of the reproductive system, it will of course be understood that reproduction by sexual agency is alone intended, as the numerous methods of multiplication by means of buds and the like, appertain more closely to the processes of nutrition and growth than to those of reproduction properly so called.

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* Cf. Beale's Archives of Medicine, 1861.

The fibro-vascular framework is merely a modification of the cellular system, and like it no doubt takes part in the nutritive processes, at least in its young condition, before it becomes blocked up by secondary deposits. It is, however, in its morphological aspect that we here consider it, and as forming a sort of endo-skeleton. Co-existent with it, leaving out of consideration a very small number of exceptions, is the presence of an *axis*. When the plant continued in its primitive cellular state we had a *thallus*, now when fibro-vascular tissue is added we have an "axis," a distinction of as great practical importance as that between vertebrate and invertebrate animals. This fibro-vascular system is foreshadowed among certain *Algæ*, having elongated cells in their centre, surrounded by horizontal cortical cells, while in *Vaucheria*, *Bryopsis*, the stalk supporting the frustule of some *Diatoms*, and many other instances we have no vague representation of the *axis*.

Hardly is the fibro-vascular system developed, before indeed it is perfectly so, than the appendicular system becomes represented by leaves. In *Riella heliocophylla* the leaves are even placed spirally. *Jungermanniæ* have two parallel rows of leaves. Mosses have their leaves arranged spirally, and so we pass through Ferns and Lycopods, where stem and leaf are well marked. All these plants produce spores. And now we reach the higher groups of plants characterised by the formation of flowers and producing seeds, in which an embryo is formed in contrast to the spore of so-called flowerless plants. The flowers being merely modifications of the axis and of the leaves derived from it at that point, while the seed may be regarded as homologous with the leaf bud, so very generally found at the junction between the leaf and the axis or stem.

To recapitulate then, it may be affirmed that all plants have alimentary, tegumentary, and reproductive systems, and the most lowly organized plants, such as *Thallogens*, have no others. All plants but Thallogens have, in addition, a fibro-vascular system forming an axis or stem, divided for the most part into two portions, an ascending one or stem, a descending one or root, co-existent with which is the presence of an appendicular system, if such really be a distinct system, represented by leaves or leaf-like organs. Lastly, in true flowering plants we have in addition to the other systems, the appendicular system more highly developed, and forming what are called the parts of the flower, calyx, corolla, stamens, pistils, &c., and particularly we have a formation of seeds containing an embryo plant. In Thallogens, the humblest of plants, the nutritive function is inseparably conjoined with the circulatory, the respiratory, and the vegetative functions, and this is the case also, though to a less extent, in more highly organized plants, but as there seems, so far as we can yet see, no special organism even in the higher plants set apart for the fulfilment of any one of these processes to the exclusion of the others, and as they may all be included under the general head of nutrition, so here the term alimentary system is intended to comprise all those processes which in the animal kingdom are carried on by distinct organs or sets of organs specially adapted for the purpose. The functions of nutrition therefore which in the lowest plants are carried on in the cells, the only organs such plants possess are in the higher plants carried on in the roots, the stem, the leaves, &c., but still it is by means of cells that the functions are carried on, by cells essentially differing but little from those constituting the entire plant in less highly developed organisms, so that these organs of the higher plants are hardly comparable to the organs of animals in which difference of structure is accompanied by a corresponding diversity of function. If then, physiologically speaking, all the processes of life may be performed by one or more cells, why not, morphologically speaking, may we not have but one organ as we have, indeed, in purely cellular plants—the single cell or the aggregate of many such in the thallus? Thus much every one will admit; but in the higher plants, as they are termed, where conjoined with an increased complexity and diversity of minute anatomical structure, though still purely cellular in its nature, we have an increased number of parts, such as roots, leaves, and flowers, it is assumed that more than one organ is present, as indicated by the terms axis and appendages, the latter being too often looked upon as distinct parts appended to the pre-existent axis; but there is strong evidence to show that, morphologically speaking, there is only one primary organ, call it thallus, axis, frond, or what you will, this one organ purely cellular, being even in its simplest condition quite capable of fulfilling all the essential vital processes. One can imagine such a plant fulfilling all the conditions necessary for its own existence, a very selfish existence it is true, and an existence which Nature as we see is by no means contented with. Plants, like all other works of the Creator, do not live merely for themselves but for others; they carry out the maxim, "*Nemo sibi vivat,*" on principles that are not always to the individual equitable principles, and to this end it becomes necessary that they should be endowed with properties which would not be essential, were the life of a plant only of use to itself. Not only are new properties conferred, but the power of existing under very varied conditions, the power of struggling against adversaries. How is all this effected? not by any real change in internal structure, but by slight modifications of it, not by any addition of new organs, but by the adaptation of the existing fabric to suit the altered circumstances. We know too little at present to dogmatize on these matters however, and it behoves us to be cautious in setting down as a consequence what may merely be a coincidence, although the relation of cause and effect is much more perceptible among plants than among animals. Hence, then, on physiological, no less than on morphological grounds, it is quite consistent with our present knowledge to affirm the existence of but one morphological element in plants, represented by the cell, the thallus, the stem or the leaf in the various groups of plants, and to consider that in the higher groups at least we have this primordial element subjected to various and complex modifications; thus, on this view in the higher plants, the axis and the leaf are considered as parts of one and the same organ co-existing in the majority of cases, and both subjected to those modifications included under the term metamorphosis, while in other cases the one part predominates over the other to its partial or complete suppression. According to this theory of vegetable construction, those otherwise anomalous transitional forms between leaf and stem may be explained.

It would hardly be proper in this place to enter into many details in support of the opinion just expressed; a few instances may suffice to show that there is in reality no such defined limit between axis and appendages as has been attempted to be laid down. Thus, the commonly expressed opinion that the axis increases by the addition of new cells to the extremities and to parts already formed, while nothing is added to the upper part of the appendages; in other words, that they grow from the base, although undoubtedly true in many cases, is by no means invariably so, as the researches of that excellent phytotomist Trécul fully show.*

In plants that consist of mere congeries of cells, as in Thallogens, we frequently have those cells multiplying in such a manner as to produce a leaf-like expansion, as in *Ulva*, for instance. A similar tendency is manifested in higher Cryptogamous plants, as in *Marchantia*, or in the pro embryos that result from the germination of Ferns, &c., where the leaf-form evidently precedes that of the axis. In flowering plants the predominance of the primary leaf-formation is shown in such cases as *Lemna*, and even in the embryo of dicotyledonous plants; the cotyledons are but little in arrear of the axis as regards their development, and may often be considered at the period of germination in advance of the axis, as well as in many monocotyledonous plants—for instance, *Tulipa*† *Allium*.

Moreover, the opinion that in some instances the stem and young shoots of plants are formed from the decurrence and fusion of the leaves is by no means unsupported by facts. Some of the foregoing instances may perhaps be cited as merely leaf-like modifications of the axis, and may thus be considered like the leaf-like branches of *Opuntia*, *Xylophylla*, *Ruscus*, *Pterisanthes*, *Podostomaceæ*, &c., as instances of analogy rather than of homology; but before the truth of this objection can be admitted, the difference between the axis and its appendages must be more distinctly defined than it seems to us can be done at present.

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* Ann. Sc. Nat., troisième Ser. Bot., tome xx. p. 211, &c.

† Germain de St. Pierre: Bulletin de la Soc. Bot. de France, 1855, pp. 96, 159.

It has been denied by many who contend for the distinctness of the axis and its appendages, and of the necessary pre-existence of the former, and consequent absolute dependence of the leaf-like organs on the axis, that no intermediate stages between stem and leaf exist. If the previously cited cases be not sufficient to impugn this statement, what shall we say to cases such as those afforded by the leaves of *Guarea* and *Trichilia*,* where the leaves after a time assume the condition of branches and develop young leaflets from their free extremities, a process less perfectly seen in some of the pinnate-leaved kinds of *Berberis* or *Mahonia* to be found in almost every shrubbery?†

In the animal kingdom it is stated that there is no real homology between the organs of one great class and those of another, but simply a relation of analogy; and the same distinction is considered to exist in the sister kingdom, but, as it appears with far less reason. The organs of nutrition for instance of a Fungus, are to all intents and purposes the same as in a Rose, although the structure is more complicated and modified in the one than in the other. There is no proof that the cells of the lower classes of plants are different organs from those of the higher; wherein does the difference consist? In origin they are alike, true they are modified in the higher classes of plants, but never to such an extent as to conceal their true nature, and those cells whose nutritive functions are most active are absolutely similar to those in the lower plants, and remain so as long as their functions are active; in origin then, in structure, in function, the nutritive cells are identical; why, then, say that there is no relation of homology, but only the more remote one of analogy?

The difference between the reproductive organs in the lower and higher groups of plants, and the different manner in which their functions are fulfilled, do indeed present much greater obstacles to the notion of their essentially homologous nature, obstacles arising from our imperfect knowledge; that such obstacles are far from fatal is shown, however, by the identity in origin and primary structure of the reproductive organs in the various groups of plants, and we may confidently look to future investigations into this intricate and difficult subject to overcome the difficulties now in the way of the hypothesis, to establish bonds of connexion between the various groups where none now can be shown to exist, and to demonstrate among the Cryptogamic groups especially what has been done so successfully among the Phænerogamic—the truth of the principle, Unity in Variety.

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* Dr. Alexander: Proceedings of the Linnæan Society, May 6th, 1852. Also Dr. Grisebach: Flora of West Indies, *Guarea*.

† Still more conclusive are the fronds of some of the *Podostemaceæ*—e.g., *Lophogyne*, etc.—*See* Tulasne: Monogr. Podostem. Paris, 1852.

Many have been led by the acknowledged inadequacy of the doctrine of final causes to account satisfactorily for the existence of this principle, and by the confessedly entirely hypothetical existence of "ideal types" to make deeper researches into the circumstances that bring about this general harmony of structure. In the number of this Journal for October, 1858, Mr. Hinton concludes an able paper on this subject by an assertion "that organic form is the result of motion in the direction of least resistance;" and similar conclusions are arrived at by Mr. Spencer, who in a paper on the "Law of Organic Form," published in this Journal in January, 1859, says the forms of all organisms are dependent on their relation to incident forces, including under this title those forces to which they are passively subject, and those which they experience as the result of their own action. There cannot be the slightest doubt of the general truth of these statements, impossible though it be at present to reconcile all the known facts relating to the growth of plants with them. Such, for instance, as the division of the cells in one direction or another, the direction of roots and stems, the different ways in which the same object is effected under apparently similar conditions, the influence exerted by the quantity, quality, and locality of the nutriment required by the growing and living organism. These are a few among other instances where the form is affected more or less by the agencies just referred to, but probably to a greater degree by causes of which we know at present little or nothing. Mr. Spencer, indeed, does not rely wholly on such agency, for he says, "conjoined with the law of hereditary transmission this may be the principle underlying all morphology"—a view harmonizing in some degree with that of Mr. Darwin, who says, "we have merely to conceive an ancient progenitor, plant, or animal constructed on the existing general pattern, but whose descendants have become subjected to successive slight modifications, each modification being profitable in some way to the modified form, but often affecting by co-relation of growth other parts of the organism." "The general pattern of an organ might," he continues, "become so much obscured as to be finally lost by the atrophy, and ultimately by the complete abortion of certain parts, by the soldering together of other parts, and by the doubling or multiplication of others, variations which are within the limits of possibility."

Upon morphology of necessity depend the various systems of classification in plants, whether they be professedly artificial, or, when a higher end is aimed at, as in the so-called natural system. In the one, the object is to detect with facility the species of any given plant; in the other, to learn as much as can be learned of its nature and its relationship to other plants; thus, the increased difficulty of the natural system is amply compensated for by the amount and value of the information gained in its prosecution. In both plans the object is the same—to throw together individual plants into groups called species, these again into higher groups, such as genera, orders, &c.

The true nature and limits of species, nay, even the fact of their very existence or the reverse have furnished a constant bone of contention. With one writer, the twist of a petal, the notching of a leaf, if tolerably constant, constitutes a species, while another overlooks slight variations, and masses into one readily definable group a host of plants presenting considerable diversity in form and appearance. Another says species are merely arbitrary creations of the botanist and have no real existence in nature. In questions of this kind the opinions and evidence of those naturalists, who devote their whole attention to this subject, who have the opportunity of examining and comparing large suites of specimens from every possible locality, and grown too under every possible variety of external conditions, whose minds are trained to the careful philosophical investigation of what features are of importance, and what not, of facts which in one group are of the highest value for classificatory purposes, in another of no value at all, is of greater weight than that of other naturalists, whose views are less philosophic, and whose studies range over fewer plants, confined to more limited areas and subjected to fewer variations in external conditions. If the opinions of the latter be nearest to the truth, we shall be obliged almost to assume that there is little or no variation in species, these are the observers, to use Hudibrastic phrase,

"Who can distinguish and divide
A hair, 'twixt south and south-west side."

and thus, what their opponents would consider as trifling variations, are considered by the hair-splitters to form distinct species. But if the views and the practice of the former class of observers be most consistent with the facts of the case, then we must assume, as most of us have done, until the advent of Mr. Darwin's wonderful book, that each species is liable to vary within certain limits as yet not defined, and probably very different in degree in different species. But whether or not Mr. Darwin's conclusions be just, there cannot be the slightest doubt, that he has done good service in lessening the breach between the two classes of observers before referred to, and by showing how truly valuable and mutually important are their observations, conducted though they may be on somewhat opposite principles. It is unnecessary here to enter at any length into Mr. Darwin's views, as they have been already commented on in this journal, and his notions as to the origin of, and relationship of existing species are generally known. For a clear exposition, however, of the rules and methods employed by systematists in framing their modes of classification, the thirteenth chapter of Mr. Darwin's book may be with great profit consulted. In his eyes, "the natural system is genealogical in its arrangement, with the grades of difference between the descendants from a common parent expressed by the terms genera, families, orders, &c." An illustration that he gives of this view of classification is so apt and embraces so many of the views held and maintained by him with equal learning and candour, that it may with much propriety be here inserted.

"If we possessed a perfect pedigree of mankind, a genealogical arrangement of the races of men would afford the best classification of the various languages now spoken throughout the world; and if all extinct languages, and all intermediate and slowly-changing dialects had to be included, such an arrangement would, I think, be the only possible one. Yet it might be, that some very ancient language had altered little, and had given rise to few new languages, whilst others (owing to the spreading and subsequent isolation and states of civilization of the several races descended from a common race) had altered much, and had given rise to many new languages and dialects. The various degrees of difference in the languages from the same stock, would have to be expressed by groups subordinate to groups; but the proper or even only possible arrangement would still be genealogical; and this would be strictly natural, as it would connect together all languages, extinct and modern, by the closest affinities, and would give the filiation and origin of each tongue."*

Before quitting this subject we may be allowed to add a few words on the value of botany, especially of morphology, and classification in the preliminary education of the medical student. It seems to us that the value of botany in this point of view has been hitherto almost overlooked, and students have been, and still are required by some of the examining bodies, to attend a short course of lectures on this subject in their first summer session, when their time should be occupied with subjects of a more practical nature. The result is, in the great majority of cases, that a listless attendance is given by the students, who are utterly unprepared for such instruction, and who find it impossible without previous elementary acquaintance with the subject to get more than a superficial smattering of botanical science, which is speedily forgotten, and conduces to no good result. Of late, however, there have been symptoms of an improvement in this respect, as manifested by the regulations of the University of London, which, in place of demanding an examination in this subject at the first M.B. examination, now very properly requires it to be taken up at the preliminary scientific one. The College of Surgeons allows students at their option to be examined in botany and zoology at their preliminary examination, in subjects of general education. The two great requisites for the successful physician, says Dr. Watson, are "skill in observing and skill in acting;" without the former, the latter is but shameless empiricism; without the latter, the former is but unprofitable pedantry; this being so, it becomes a question how best to foster that faculty of observation possessed by all in very varying degrees. Herein lies the pre-eminent value of a training in some one branch at least of natural history, as preparatory to the effective study of disease at the bedside. It may be asked why a training in mathematics or logic, or why an intelligent study of the principles of language, should not be equally as advantageous to the student of medicine as a knowledge of the principal facts in some one or more branches of natural history. This is not the place to enter at any length into this question; all that need be said is, that both the methods of observation and the methods of reasoning in use among naturalists, are far more nearly akin to those used by physicians in the study and treatment of disease, than they are to those employed by the mathematician or the philologist, and this must of necessity be so, seeing that medicine is but a branch of natural, or as it may be called, of vital science, and which cannot be subjected to the same rules and tests as the mathematical or the physical sciences. In support of our opinions, we would especially call attention to the mode of describing plants now employed by botanists. The object is to convey in as terse a manner as possible, a correct description of a plant, omitting nothing essential, inserting nothing superfluous; to accomplish this satisfactorily, much practical knowledge is indispensable, great precision in the use of language, great discrimination in order not to confound things which appear alike, but are in reality different, and equal care not to set down as diverse, things which are essentially the same. In few branches of knowledge is the adage, "*Nimium ne crede colori*," more constantly brought to mind; in few, is there greater necessity for the student not to be led away by the immense variety of form and appearance from the great principle of unity in variety. We would recommend those who have not considered this subject to compare the mode of correctly describing plants as given in a little pamphlet of Dr. Lindley, entitled 'Descriptive Botany,'* with the ordinary mode of reporting cases in hospitals or in the public prints. If such a comparison be made, we feel assured that there will not be many dissentients from the opinion we have ventured to express as to the value of a training in botanical science as preliminary to the more strictly professional branches of education.

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* Darwin: Origin of Species, p. 422.

* Bradbury and Evans. London, 1860.

PART FOURTH.

Chronicle of Medical Science

(CHIEFLY FOREIGN AND CONTEMPORARY).

HALF-YEARLY REPORT ON PHYSIOLOGY.

By Hermann Weber, M.D.
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I. General Physiology.

1. *Force.* (Cornhill Magazine, No. xxi., p. 409, 1861.
2. J. H. Bennett: *On the Molecular Theory of Organization.* (Proceed. of the Roy. Soc. of Edinburgh, April 1st, 1861.)
3. P. Broca: *On the Phenomena of Hybridity in the Human Genus.* (Brown-Séquard's Journal de la Physiol., vol. iii., p. 392, 1860.)
4. M. Boudin: *On the Non-Cosmopolitism of the Human Races.* (Brown-Séquard's Journ. de la Physiol., vol. iii., p. 363, 1861.)
5. Thomas Graham: *Liquid Diffusion applied to Analysis.* (Proc. Roy. Society, vol. xi., p. 243, 1861.)

1. The author of 'Force' gives a lucid explanation of the scientific signification of this term, and of the nature and characters of force. He shows that the different forces, such as mechanical force, gravity, cohesion, light, heat, electricity, magnetism, chemical affinity, and vital force, are not separate powers, but are only conditions of matter; that, for instance, heat, light, and electricity are the common results of motion, which itself is not a thing, but only a state. As the different forces are not entities, but only conditions, the author generalizes them under the common idea of "force," including under this term "all the active conditions of matter, of whatever kind they may be." "The total amount of these active conditions," he continues, "is the total amount of force. The differences are differences merely of form or mode; essentially all are the same." In elucidation of this view, the author demonstrates that we are unable to isolate any one of the forces, except for some temporary purpose; that they are constantly passing off the one into the other. "Motion resolves itself in sound and heat; heat flies off in motion, in chemical or electric change; electricity is lost in sparks of light, in magnetism, in mechanical disruptions, in the production of chemical power; chemical power no sooner acts than it is no more chemical, and must be recognised in explosions, in electric currents, in heat." Motion thus may be transformed into heat, this into light, thence into magnetism and chemical attraction, and may at last appear again as motion. The author further points out the *quantitative* relation in the conversion of one kind of force into another; and the existence of force under two forms—the active and the passive (the tense and the relaxed bow). Natural bodies with *active* force are ready to operate—such as bent springs, which contain mechanical force; vapour, which contains

heat; a charged electric battery; plants and animals, which are likewise "force-containing groups of bodies." Substances with *passive* force are more ready to receive than to give out force—such as springs that are unbent, an electric battery that has been discharged. The author compares the equivalence of force in nature with the phenomenon of *vibration* in mechanics, as every change "must consist of two equal and opposite actions—the ceasing of force in one relation, and its operation in another."

2. Bennett opposes by the "molecular theory" the law of *omnis cellula e cellulâ*. He understands by *molecules* minute bodies "varying in size from the four-thousandth of an inch down to a scarcely visible point;" he gives a description of the physical and chemical characters of such molecules, and divides them into two kinds—1st, *histogenetic molecules*, formed either from the union of two simple organic fluids, or from precipitations occurring in formative fluids, holding various substances in solution; 2nd, *histolytic molecules*, as the result of the disintegration of previously-formed tissues. The author expresses the opinion that histolytic molecules are frequently transformed into histogenetic elements; or, in his own words, "that molecular matter formed from the process of disintegration may, when placed under peculiar circumstances, become the basis of matter which undergoes development." Bennett points out that under the influence of a *molecular force* these molecules exhibit a variety of movements, and combine in definite ways; he considers the molecular force as altogether independent of cell, nucleus, or other form of structure. The author's researches lead him to the enunciation of the following molecular law of growth—viz., "That the development and growth of organic tissues is primarily owing to the successive formation of histogenetic and histolytic molecules." Bennett believes that the first step in the development of textures is the formation of molecules, that these unite to form nuclei and cells; that these become disintegrated to produce a secondary mass of molecules; that these again unite into nuclei and cells; and that the same process is repeated more or less often in various developments until the animal tissue is formed.

3. Broca has by careful researches been led to the view that the various races of man are different species of the same genus. He exposes as inconclusive the principal arguments adduced in favour of the unity-theory, and dwells especially on the incorrectness of the assertion generally made by the defenders of this theory with regard to the phenomena of hybridity in the various races of man, and in various species of animals belonging to the same genus. It has been set forth as the most irrefutable proof of the unity of the human species, that hybrid procreations of two different races of man are perfectly able to propagate themselves, while hybrid procreations of two species of animals of the same genus are said to be unable to do so; Broca, however, shows, 1st, that some of the hybrid productions from different races of man, possess only a limited power of propagation; and, secondly, that some hybrid species of animals, as those from the crossing of the dog and the wolf, the goat and the sheep, the dromedary and the camel, the hare and the rabbit, are perfectly able to propagate their species.

4. Boudin concludes from his researches on the manner in which the different races of man bear the climates of the various zones, that these races exhibit great differences with regard to the faculty of becoming acclimatized, and that, in general, man cannot be acclimatized to all zones. The European cannot live and propagate his race as an agricultural labourer in the hot countries of the northern hemisphere, but he can do so in many of the hottest parts of the southern hemisphere; he bears, in general, the migration to cold countries better than that to hot zones. The negro cannot exist in the south of Europe, and even not in the north of Africa, where his race can be maintained only by constant immigrations; it is doubtful whether he can become

thoroughly acclimatized to the West Indies, to Bourbon, Mauritius, and Ceylon, though these islands belong to the tropical zones; the negro appears to be able to live in the south of the so-called United States, while his mortality in the Northern States is great, where he is, in addition, very liable to insanity. The Hebrew nation appears to bear migration to all countries, and offers very favourable statistics of births and deaths wherever it exists.

5. Graham's communications, although they are not strictly physiological, demand our careful attention, as the means of analysis and the phenomena pointed out by him are likely to become highly important in the study of the vital phenomena of the animal organism. We refer for accurate information to the 'Philosophical Transactions,' mentioning here only that the author divides all chemical substances into two great groups—*crystalloïds* and *colloïds*, and studies their distinctive properties by means of diffusion. The *crystalloïds* are highly diffusive, their reactions are energetic and quickly affected, they are marked by their tendency to crystallize, either alone or in combination with water. The *colloïds*, on the contrary, are of low diffusibility, have little (if any) tendency to crystallize, and appear to be typified by animal gelatine. "The colloïd," Graham remarks, "possesses *energia*. It may be looked upon as the probable primary source of the force appearing in the phenomena of vitality, as living matter without form. To the gradual manner also in which colloïd changes take place (for they always demand time as an element) may the chronic nature and periodicity of vital phenomena be ultimately referred."

II. FOOD AND DIGESTION.

1. E. von Bibra: *Cereals and Bread.* (Nürnbrug, 1860; and Canstatt's Bericht, loc. cit., p. 214.)
2. C. Bödeker: *Communications from the Chemical Laboratory of the Physiological Institution at Göttingen—The Composition of Woman's Milk.* (Zeitschr. f. Rat. Med., vol. x. p. 162, 1860.)
3. E. Metzler: *Contribution to the Knowledge of the Digestion of Gelatine, Gelatine-forming Tissues and Cartilage.* (Giessen, 1860; and G. Meissner's Bericht über Physiologie in 1860, in Zeitschr. f. rat. Medic., vol. xiii., p. 257, 1861.)

1. Von Bibra gives a detailed description of the cereals and the bread. After an elaborate historical account of the culture of cereals by various nations, he enters on the chemical constitution of the different cereals. The author then devotes a chapter to the manufacture of bread; he gives analyses of the principal kinds of bread used in his country and abroad, and finally communicates some researches on the hygroscopic characters of cereals and bread.

2. Bödeker gives the analysis of the milk of a healthy woman on the fourteenth day after delivery. The following table contains the proportions of the different constituents found by the author, compared with the analyses of some other chemists :—

	Bödeker.	Griffith.	Doyère.	Simon.	Henry and Chevallier.
Water	882·2 ...	875·00 ...	873·8 ...	861·0—914·0 ...	879·5
Sugar of milk . . .	64·6 ...	61·76 ...	70·0 ...	39·2— 62·4 ...	65·0
Fat	31·0 ...	25·41 ...	38·0 ...	8·0— 54·0 ...	35·5
Proteinaceous substances . . .	19·0 ...	12 68 ...	16·4 ...	19·6— 45·0 ...	15·2
Salts.	3·2 ...	1·55 ...	1·8 ...	1·6— 2·7 ...	4·5

Bödeker proposes the following mixture as a substitution for woman's milk : 200 cubic centimetres of cow's milk, 50 cubic centimetres of cream, 150 cubic centimetres of water, and 15 grammes of sugar of milk. Or approximatively in other figures: 8 ounces of cow's milk, 2 ounces of cream, 6 ounces of water, ½ ounce of sugar of milk.

3. Metzler has examined the action of gastric juice ("an acidulated infusion of the mucous membrane of the pig's stomach") on gelatine and gelatinous substances. Solutions of gelatine, when digested in blood-heat with gastric juice, lost their property of coagulating. Even small quantities of gastric juice sufficed to deprive large quantities of gelatine of its coagulability, requiring, however, more time to effect this than larger quantities. Gastric juice, previously boiled, did not exercise this influence. Tendons, which the digestion with diluted hydrochloric acid left unchanged, were by the action of the gastric juice transformed into a syrupy fluid, offering similar properties as the solution of gelatine by gastric juice. Elastic tissue seemed to be as little influenced by the digestion with gastric juice as by that with diluted hydrochloric acid.

III. CIRCULATION AND RESPIRATION.

1. F. GOLTZ: *On the Interpretation of the so-called Automatic Movements of the Cut-out Frog's Heart.* (Virchow's Archiv, vol xxi. p. 191, 1861.)
2. M. SCHAEFER: *On the Auscultation of the Normal Sounds of the Heart.* (Archiv d. Vereins für Wissenschafth Heilkunde, vol. v. p. 137.)
3. WALLACH: *On the Mechanism of the Second Sound of the Heart.* (Gaz. Méd. de Lyon, 1860, No. 4, p. 89; and Canstatt's Jahresbericht für 1860, vol. i. p. 122, 1861.
4. EINBRODT (MOSKAU): *On the Influence of the Respiratory Movements on the Pulsation of the Heart, and the Pressure of the Blood.* (Moleschott's Untersuchungen, vol. vii. p. 265,1860.)
5. J. P. SUCQUET: *On the Circulation of the Blood in the Limbs and in the Head in Man.* (Paris, 1860; and Canstatt's Bericht, loc. cit. p. 122.)

1. Goltz endeavours to prove that the experiments of Stannius* on the frog's heart have as yet not been satisfactorily interpreted, and that they are in discord with the theory that the heart is a self-moving apparatus. The principal facts discovered by Stannius, it will be remembered, are—a ligature placed round the bulbous entrance of the venæ cavæ into the right auricle stops the action of the whole heart in the state of diastole; a ligature placed round any portion of the auricles, arrests the action of the heart below the ligature, while the portion above the ligature, together with the venæ cavæ, continues to pulsate; if a ligature is placed exactly between the auricles and ventricles, both continue to pulsate, but the ventricles more slowly; if first a ligature is placed round the entrance of the cavæ, and then another one round the commencement of the ventricles, the latter begin again to pulsate, the auricles remaining motionless. These phenomena have been explained by assuming that the ligature acts as a stimulus on the fibres of the pneumogastric nerve, and excites its inhibitory influence. Goltz, on the contrary, thinks that the ligature acts, not by stimulating, but by dividing, the nerve-fibres. The objection, that the substitution of section instead of the ligature, in the experiments of Stannius, has not the same effect as the latter, is met by a modification of the original experiment—viz.: by performing the section while the heart is immersed under oil, in which case really the same effect is obtained as by ligature; while as soon as the heart is taken out of the oil, and exposed to the stimulus of the atmospheric air, the pulsation begins as if the section had been made in the air. The author finds a farther reason for the view that the ligature acts by division and not by irritation, in the fact, that after removal of the ligature, the contractions

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* Two Series of Physiological Experiments: Müller's Archiv, 1852, p. 85.

do not again begin, or if they do, at all events not sooner than if the ligature had remained. Goltz infers from these experiments, that the heart, with its ganglia, has no self-moving power in the usual sense, but that its so-called automatic action depends on stimuli, and that it is from the bulbous dilatation of the right auricle, formed by the entrance of the cavæ, that the natural impulse is given to the heart's actions; as soon as this source of stimulus is removed, the heart remains quiet, until it is again excited by artificial impulse. For the description of the experiments, and for the author's reasoning, we refer to his essay.

2. Schäfer communicates the results of the careful examination of fifty healthy subjects with regard to the sounds of the heart; he has made his examination, together with Professor Seitz, at Giessen. The authors treat on the quality of the two sounds of the heart, and on the manner in which they are heard on the different parts of the chest. Concerning the phenomenon of reduplication or rather division (*Spaltung*) of the sounds of the heart, the authors never met with it in the first sound, but in twenty-nine cases out of fifty in the second sound; they found it especially when the second sound was loud, and in general at the spot of its greatest intensity—viz., in the fourth and fifth left intercostal space, close to the sternum; it was in the same person not constantly heard, but almost only at the end of the inspiration, and during the expiration. The authors thus confirm Bamberger's view that the reduplication or division of the second sound does not indicate anything abnormal.

3. Wallach has likewise examined the subject of the division of the second sound, and has met with it as well in animals as in man. He explains it by a want of synchronism in the closure of the semilunar valves of the aorta and those of the pulmonary artery; this may, according to Wallach, be caused by a difference in the length of the two arteries, in the structure of their walls, in the propulsive force of the blood streams, and in the resistance of the tissue surrounding the arteries; the latter circumstance especially being influenced by the act of respiration.

4. Einbrodt has, at the suggestion of Ludwig, examined the influence of the respiratory movements on the rhythm of the pulsations of the heart, and on the pressure of the blood. The majority of the observations were made on dogs, some, however, also on men. The author has especially studied the influence of artificially increased and diminished respiratory pressure on dogs. We must, of course, refer to the essay itself for the description of the apparatus used, and of the experiments performed.

The *increased respiratory* pressure during its rise from 0 to its maximum, causes an increase in the pressure of the blood in the arterial system; during its duration it, *a*, renders the respiratory movements more or less difficult; the inspiratory movements being short and rapid, the expiratory movements heavy and protracted, complete pauses of several minutes' duration occurring, when the pressure rises beyond twenty millimetres mercury; *b*, it impedes the afflux of the blood to the heart, diminishes the propulsive power of the heart, and reduces the pressure of the blood in the arterial system in proportion to the amount of the respiratory pressure; *c*, it influences the rhythm of the heart's action in a twofold manner—viz., by direct irritation of the heart, and by irritation of the pneumogastric nerves, the former causing increased, the latter diminished frequency of contractions; the rhythm therefore varies according to the predominance of the one or other of these elements; *d*, it causes cerebral pressure through prevented reflux of the blood from the cerebral veins, to which circumstance the irritation of the pneumogastric nerves is ascribed. The phenomena caused by *cessation of the increased respiratory pressure* offer nothing characteristic.

The *diminished respiratory pressure* renders the respiratory movements likewise difficult, especially the inspiration, through the difference between the pressure on the internal surface of the lungs, and that on the walls of the chest; but if this difference does not exceed fifty millimetres mercury, the respiratory movements are still able to effect a change of the air in the lungs. The arterial pressure of the blood rises with every expiration, and sinks during the inspiration; the greater the difference between the respiratory pressure and the pressure on the chest, the greater is the rise of arterial pressure during the expiration. The mean pressure of the blood becomes increased through the diminished respiratory pressure, the heart containing more blood, and propelling a larger quantity with every contraction. The heart's contractions are, in general, rendered less frequent, partly through irritation of the pneumogastric nerves, partly also through commencing paralysis of the heart from the long continued effort of the diminished pressure.

The results of the observations on the effect of the *respiratory movements* under ordinary pressure, may be related as follows:—1. The rhythm of the heart's contractions and the pressure of the blood are usually not affected, if the respiratory movements are rapid. 2. If the respiratory movements are extensive the inspiration causes, in general, an increased, the expiration a diminished frequency of the pulse, the latter effect being attributed to the central irritation of the pneumogastric nerves, the former to the diminished irritation of these nerves. The pressure of the blood becomes in the beginning of the inspiration diminished, it rises during the progress of the inspiration, and reaches its maximum at the commencement of the expiration, when it again begins to sink. The author has observed analogous phenomena on some human subjects, while in others the respiratory movements appear to have no influence on the rhythm of the heart.

5. Sucquet points out the existence of two manners of communication between the arteries and veins of the limbs and of the head—viz., that through the capillary system, and an immediate transition from the small arteries into small veins. In the arms the basilic and cephalic veins form the channels for the blood passing directly from the arteries into the veins, in the legs the venæ saphenæ and some deeper veins, in the head the venæ facialis, auricularis, and ophthalmica. Sucquet adopts the view that these immediate communications between arteries and veins have the function of carrying off the blood not required for the nutrition of the part, that they form therefore a derivatory system. The fact that the blood contained in these derivatory veins is of dark colour, although it has not passed through the capillaries, is explained by the supposition that the absence of pulsation is sufficient to cause the dark colour; the arterial colour of the blood of venesections, when this has lasted so long as to cause pulsation of the vein, is adduced in favour of this supposition.

These views of Sucquet's are treated of also in a preceding paper on the "Cerebral Circulation," see page 112.

IV. Lymph; Chyle; Blood.

1. C. Schmidt: *On the Chemical Constitution and Process of Formation of Lymph and Chyle.* (Bulletin de St. Petersburg, 1861, vol. iii. p. 355; Chemisch. Centralblatt, 1861, No. 27; and Meissner's Bericht, l. c. p. 258.)
2. J. Setschenow: *Contributions to the Pneumatology of the Blood.* (Zeitsch. f. Rat. Med., vol. x. p. 101, 1860.)
3. J. Setschenow: *Pneumatological Notes.* (Zeits. f. rat. Med., vol. x. p. 285, 1860.)
4. Lussana: *On the Doctrine of Beltrami regarding the Fibrin of the Blood.* (Lettera del Dott. Lussana a Giusto Liebig; Gaz. Med. Ital. Lombardia, Nro. 10—13, 21, 22, 24, 25, 1860; and Canstatt, loc. cit., p. 229.)

1 C. Schmidt has again studied the composition and formation of the lymph and chyle. He found that in foals well fed with hay, the quantity formed

within twenty-four hours in the head and neck amounts to about 144 grammes for 1 kilogramme of head and neck of the animal; or in other words, to about 14 per cent. of the total weight of the parts. The following table shows the constitution of the lymph, which was in the two cases examined, almost the same.

	1000 parts of lymph contain		1000 parts of serum.	1000 parts of clot.
	955·17 serum,	44·83 clot.		
Water	914·68	40·68	957·61	907·32
Fibrin	—	2·18	—	48·66
Albumen	30·59	1·54	32·02	34·36
Fat and fatty acids	1·17		1·23	
Other organic matters	1·69		1·78	
Mineral salts	7·04	0·43	7·36	9·66
Chloride of sodium	5·40	0·27	5·65	6·07
Soda	1·24	0·03	1·30	0·60
Potash	0·11	0·05	0·11	1·07
Sulphuric acid	0·08	0·01		
Phosphoric acid combined			0·08	0·18
with alkali	0·02		0·02	0·15
Phosphate of lime	0·19	0·07	0·20	1·59
Phosphate of magnesia				

From experiments on two other foals, the author concludes that the chyle passing within twenty-four hours through the cervical portion of the thoracic duct amounts to about 6·6 per cent. of the weight of the body. The composition of the chyle collected several hours after a meal consisting of hay and meal-pap, differed only very slightly from the lymph obtained from the lymphatic vessel of the neck; the author ascribes the principal difference, consisting in the presence of iron in the chyle, to the admixture of hæmatosin. For the analysis of the chyle we refer to the essay. The quantity of chyle obtainable from the thoracic duct was almost doubled—viz., 13 per cent. of the weight of body in twenty-four hours, when the animal was fed on milk.

The comparison of the constitution of the food consumed with that of the fluid passing through the thoracic duct within twenty-four hours, shows that almost one-half of this fluid has its origin not in the chyme, is, therefore, not chyle proper, but is derived from the blood, and is consequently lymph, while the other half is chyle proper. The examination of the blood of the same animal leads to the inference, that at least half of the total amount of blood, and, in particular, its intercellular fluid, passes over in the course of twenty-four hours into the lymphatic system. The quantity of chyle and the per-centage of salts carried in twenty-four hours through the thoracic duct, is nearly equal to the total amount of the intercellular fluid of the blood. The formation of lymph from the blood, according to Schmidt's view, is represented by the following table:

	Water.	Solids.	Fat and fatty acids.	Fibrin.	Albumen, hæmatosin, sugar, &c.	Salts.
1000 grms. of blood contain	805·49	194·51	2·64	3·31	150·37	7·56
483 grms. of lymph contain	461·70	21·57	0·63	1·05	16·28	3·61
516·7 grms. of blood-globules, together with the remainder of the intercellular fluid carried on in the bloodvessels	343·79	172·94	2·01	2·26	134·09	3·95

2 & 3. Setschenow has obtained from 100 volumes of venous blood of dogs 35·21 carbonic acid, 4·10 oxygen, 0·78 nitrogen. The author found the quan-

tity of simply-diffused carbonic acid very small, that of loosely-bound carbonic acid so considerable as to allow only a small proportion of it to be accountable as derived from the bicarbonated alkali of the blood; he therefore considers the greater portion of the acid as loosely combined with phosphate of soda, a view which has been pronounced already by Fernet.

4. Lussana endeavours to prove by his own, and by Bettrami's researches, that fibrin is a product of the retrogressive metamorphosis, a view which had been adopted by many physiologists already, before the publication of the author's letter to Liebig. Lussana has further devoted much study to the establishment of the hypothesis that fibrin has its origin in the used-up muscular fibre.

THE Report on Nutrition, Metamorphosis of Tissue, Secretion, Nervous System, and Senses, has been postponed to the July number.

The following papers are likewise of interest, but for want of space could not be analysed:—

R. Dunn: Some Observations on the Tegumentary Differences among the Races of Man. (Journal of the Ethnological Society, vol. iv. p. 33.)
R. Dunn: Physiological and Psychological Evidence in support of the Unity of the Human Species. (From the Ethnological Transactions, July 20th, 1859.)
Thomas Graham: On Liquid Transpiration in Relation to Chemical Composition. (Proc. Roy. Soc., vol. xi. p. 381, 1861.)
A. Heynsius: On the Diffusion of Albumen. (Archiv von Donders und Berlin, vol. ii.; and Canstatt's Jahresbericht für 1860, vol. i. p. 1, 1861.)
Gunning: On the Imbibition of Animal Membranes. (Archiv v. Donders und Berlin, vol. ii; and Canstatt, loc. cit., p. 1.)
Eckhard: On the Celerity of Diffusion through Animal Membranes. (Eckhard's Beiträge zur Anatom. und Physiologie, vol. ii. p. 159; and Canstatt, loc. cit., p. 1.)
L. Ordenstein: On the Parotid Saliva of Man. (Giessen, 1859; and Canstatt, loc. cit., p. 115.)
Planer: The Gases of the Digestive Tube, and their Relations to the Blood. (Sitzungsberichte d. Wiener Akademie, vol. xlii. p. 307; and Meissner's Bericht, loc. cit., p. 258.)
M. Schiff: Report on the Experiments performed in Prof. Schiff's Physiological Laboratory in the Course of 1860. Digestion. (Archiv d. Heilkunde Jahrgang, 1861, p. 229.)
W. Brinton: Experiments and Observations on the Structure and Function of the Stomach in the Vertebrate Class. (Proc. of the Roy. Soc., vol. xi. p. 357, 1861.)
J. Basslinger: Rhythmic Contractions on the Cardia of the Stomach of Rabbits (Cardia-pulse). (Moleschott's Untersuchungen, vol. vii. p. 359, 1860.)
F. Martin: The Peristaltic Movements of the Intestinal Tube. (Giessen, 1859; and Canstatt, loc. cit., p. 112.)
Luschka, Prof.: On the Peritoneal Covering of the Cæcum, and on the Fossa Ilio-Cæcalis. (Virchow's Archiv, vol. xxi. p. 285, 1861.)
F. Cohn: On Proteinaceous Crystals in Potatoes. (Erdmann's Journal, vol. lxxx. p. 129; and Canstatt, loc. cit., p. 229.)
A. Chauveau: Experiments Performed at Alfort on the Sounds of the Heart. (Gaz. des Hôpit., 1860, No. 125.)
H. Jacobson: Contribution to the Hæmodynamik. (Archiv für Anatom. und Physiol., 1860, p. 80.)
Poiseuille: On the Pressure of the Blood in the Arterial System. (Compt. Rend., vol. l. Août, 1860.)

J. Marey: Researches on the State of the Circulation, from the Characters of the Pulse, furnished by a New Sphygmographion. (Brown-Séquard's Journ. de la Physiol., vol. iii. p. 241, 1860.)

Moilin: Note on the Physiology of the Pulse. (Gaz. Med. de Paris, 1860, No. 18.)

C. Gerhardt: The Position of the Diaphragm. Tübingen, 1860.

A. H. Shoemaker: On the Action of the Intercostal Muscles. (Archiv f. d. Hollaend. Beiträge, vol. ii. p. 197, 1860; and Canstatt's Bericht, loc. cit., p. 131.)

W. Koster: On the Action of the Respiratory Muscles, especially the Intercostal Muscles. (Arch. f. d. Hollaend. Beit., vol. ii. p. 408.)

C. Bacumler: Observations and Historical Notices on the Actions of the Intercostal Muscles. (Erlangen, 1860.)

Traube: On the Entrance of Small Particles of Carbon into the Interior of the Respiratory Apparatus. (Deutsche Klinik, 1860, Nos. 49 and 50.)

L. Pouchet: Researches on Substances Introduced with the Air into the Respiratory Organs of Animals. (Compt. Rend., vol. l. p. 1121, 1860; and Canstatt, loc. cit., p. 131.)

Pettenkofer: On the Apparatus for the Study of the Phenomena of Respiration and Perspiration in the Physiological Institution at Munich. (Sitzungober. der. Bayer. Akad., 1860, p. 296; and Canstatt's Bericht, loc. cit., p. 131.)

G. Valentin: Contributions to the Knowledge of the Hybernation of Marmots, Contractions of the Heart and Respiratory Movements. (Moleschott's Untersuchungen, vol. ix. p. 39, 1860.)

A. Schöffer: On the Carbonic Acid of the Blood, and its Excretion through the Lungs. (Zeitsch. f. Rat. Med., vol. xi. p. 89, 1861.)

C. Meder: On the Lymphatic System. (Zeits. f. Rat. Med., vol. x. p. 323, 1860.)

A. Schmidt: On Fibrin and the Cause of its Coagulation. (Chemisch. Centralblatt, 1861, p. 403; and Meissner's Bericht., loc. cit., p. 289.)

Botkin: Researches on the Diffusion of Organic Substances. (Virchow's Archiv, vol. xx. p. 26.)

H. A. von Haxthausen: On the Phosphoric Acid in the Urine and Fæcal Excretions. (Dissert. Inaugur. Halac, Saxon, 1860; and Virchow's Archiv, vol. xxi. p. 366.)

A. Friedleben: Contribution to the Knowledge of the Physical and Chemical Constitution of Growing and of Rhachitic Bones of the Earliest Infancy. (Jahrb. f. Kinderheilk. vol. iii. p. 61 and p. 147.)

A. Friedleben: On the Importance of the Determination of Water in Osseous Tissue. (Archiv f. Heilkunde, vol. ii. p. 139.)

A. Milne-Edwards: Chemical and Physiological Researches on Bones. (Annales des Sciences Natur., IV. Série, vol. xiii. p. 113; and Meissner's Bericht, loc. cit., p. 298.)

C. Eylerts: Chemical Examination of the Marrow-Fat of Bones. (Archiv der Pharmac., 1860, pp. 104, 129; and Meissner's Ber., loc. cit., p. 298.)

L. Ollier: Experimental Researches on Osseous Grafts. (Brown-Séquard's Journal de la Phys., vol. iii. p. 88.)

L. Ollier: On the Transplantation of Bone Taken from Animals some Time after Death. (Compt. Rend., vol. l. 1860, p. 163.)

Bourguet: Note on Osseous Regeneration. (Compt. Rend., vol. li., p. 208.)

E. Brown-Séquard: On a Case of Osseous Graft. (Brown-Séquard's Journ. de la Phys., vol. iii. p. 108.)

M. Wilkens: On the Chemical Constitution of Cartilaginous Tissue. (Siebold and Kolliker's Gatsche, vol. x., p. 467; and Canstatt, loc. cit., p. 141.)

Schoonbroodt: Note on the Transformation of Sugar into Albuminous Substance. (Compt. Rend., 1860, vol. l. p. 856; and Meissner, loc. cit., p. 298.)

A. Rollet: On the Albuminous Substances of the Connective Tissue. (Sitzungsber. d. Wiener Akademie, vol. xxxix; and Meissner's Bericht, loc. cit., p. 298.)

G. Fröhde: Contribution to the Knowledge of Albuminous Substances. (Journ. f. Pract. Chem., vol. lxxix. p. 483; and Meissner's Bericht, loc. cit., p. 298.)

J. Sterry Hunt: On the Relations between Amyloid and Albuminous Substances. (Compt. Rend., 1860, vol. l., p. 1186.)

M. Traube (of Ratibor): On the Heat of Combustion of Alimentary Substances. (Virchow's Archiv, vol. xxi. p. 414, 1861.)

C. Ludwig: New Experiments on the Temperature of the Saliva. (Wiener Wochenschrift, 1860, No. 28.)

B. J. Stockvis: Contribution to the Physiology of Uric Acid. (Archiv f. d. Holland. Beiträge, vol. ii. p. 260.)

C. Bödeker: Contribution to the Knowledge of the Tissue-Change in the Healthy Organism. (Zeitsch. f. Rat. Med., vol. x. p. 153.)

C. Neubauer: Contributions to the Analysis of Urine. (Archiv d. Ver. f. wissensch. Heilk., vol. iv. p. 228.)

J. Kaulich: On the Formation of Aceton in the Animal Organism. (Prager Vierteljahrschr. 1860, vol. iii. p. 58.)

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J. Lockhart Clarke: Notes of Researches on the Intimate Structure of the Brain. Second Series. (Proceed. Roy. Soc., vol. xi. p. 359, 1861.)

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F. Arnold: On the Nerves of the Dura Mater. (Oester. Med. Jahrb. 1861, p. 26.)

L. Meyer: On the Nature of the Pacchionian Glands. (Virchow's Archiv, vol. xix. p. 171 and p. 288.)

M. Schiff: New Researches on the Influence of the Pneumogastric Nerve on the Action of the Stomach. Bern, 1860.

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L. Joseph: Contribution to the History of the Physiology of the Pneumogastric Nerve. (Virchow's Archiv, vol. xviii. p. 368, 1860.)

C. E. E. Hoffmann: Contributions to the Anatomy and Physiology of the Pneumogastric Nerve in Fishes. (Giessen, 1860.)

J. Moleschott: Researches on the Influence of the Irritation of the Pneumogastric Nerves on the Frequency of the Heart's Contractions. (Moleschott's Untersuchungen, vol. vii. p. 401.)

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A. Chauveau: Theory of the Physiological Effects produced by Electricity on the Animal Organism, as well in the Form of the Instantaneous as also of the Continued Current. (Brown-Séquard's Journal de la Phys., vol. iii. pp. 52, 274, 458, and 534.)

J. Budge: On the Difference in the Irritability of one and the same Nerve, and on the Value of Pflüger's Electrotonus. (Virchow's Archiv, vol. xviii. p. 457; and Canstatt, l. c. p. 180.)

R. Remak: Centripetal Action of the Constant Galvanic Current on the Nerves of Man. (Compt. Rend., vol. li. p. 327, 1860.)

C. Matteucci: Results of Researches on the Electric Function of the Torpedo. (Proceed. Roy. Soc., vol. x. p. 576, 1860.)

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A. von Bezold: On some Relations of Time in Connexion with the Electrical Irritation of Nerves. (Moleschott's Untersuchungen, vol. vii. p. 581, 1860.)

A. von Bezold: Researches on the Influence of the American Arrow-Poison (Curare or Woorara). (Archiv f. Anatom. und Phys., Jahrg. 1860, pp. 168 and 387.)

W. Wundt: On the Influence of Curare Poison on Nerves and Muscles. (Verhandl. d. Naturh. Ver. in Heidelberg, 1860; and Canstatt, l. c. p. 181.)

W. Kühne: On the Action of the American Arrow-Poison. (Archiv für Anatom. und Physiol., Jahrg. 1860, p. 517.)

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L. Vella: On the Antagonism which exists between Strychnine and Curare; or the Neutralization of the Tetanic Effects of Strychnine by Curare. (Compt. Rend., vol. li. p. 353, 1860.)

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HALF-YEARLY REPORT ON MATERIA MEDICA AND THERAPEUTICS.

By Robert Hunter Semple, M.D.

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I. *On the Treatment of Glanders (in the Horse) by the Arsenite of Strychnia.* (Journal des Vétérinaires de Toulouse, June, 1861.)

MM. Ercolani and Bassi, Professors of the Veterinary School of Turin, have ascertained that a gramme (about fifteen grains) of arsenite of strychnia administered to a horse of full size, produced in twenty minutes the convulsions which are peculiar to poisoning by strychnia, and that with sixty centigrammes (a centigramme is ·1543 of a grain) a slight muscular contraction was the only result. In cases of glanders, they therefore administered a dose of twenty centigrammes, which they gradually raised to sixty and eighty centigrammes. The drug is given with bread in the form of pills. The experiments proved that the binarsenite was more efficacious than the arsenite. Of forty horses affected with glanders, the results of treatment were ascertained in thirty. Of these thirty, eighteen were cured by the administration of the arsenite of strychnia, but the remaining twelve were not improved by the treatment, and they were killed as being incurable. These results require confirmation, but in France, where the arsenite of strychnia has never been employed, the cure of glanders in horses has never been observed in such a large proportion.

II. *On the Efficacy of a Combination of Cubebs and Copaiba in some Affections of the Neck of the Bladder and the Prostatic Part of the Urethra.* By Dr. Caudmont. (Bulletin Général de Thérapeutique, July 30, 1861.)

Dr. Candmont has employed cubebs and copaiba in different affections of the neck of the bladder and the prostatic portion of the urethra, and the results have convinced him that these medicines exert a much more certain action over these parts than over the anterior portion of the urinary canal. In some cases, he administered the copaiba alone, in other cases the cubebs alone; but in general he combines these two drugs, because it appeared to him that the digestive organs were more tolerant of their administration in this form. The morbid conditions in which these medicines are indicated, may be referred to forms of neuralgia or inflammation. The neuralgic cases begin suddenly, without being preceded by any functional disturbance, and the symptoms are immediately very severe; there is frequent desire to make water, severe pain during that process, and especially at its conclusion, and darting pains along the inferior surface of the penis during the intervals of micturition. This neuralgic form occurs in persons who are subject to neuralgia in other parts of the body. The other class of cases presents an inflammatory character, and a few drops of blood are voided at the end of each act of micturition. The greater number of such cases may be referred to a blennorrheal or rheumatic origin, and the urine deposits gravel, or contains a considerable quantity of mucus or muco-pus. In all these cases, whether neuralgic or inflammatory, the use of copaiba and cubebs is very successful, and entirely removes the pain in from twenty-four to forty-eight hours. Dr. Caudmont administers these medicines in the form of sugar-plums (*dragées*), each containing forty centigrammes of the mixed cubebs and copaiba (a centigramme is ·1543 of a troy grain), and fifteen, eighteen, or twenty-four of these are given every day, according to the intensity of the disease.

III. *On the beneficial Effects of Apiol in Amenorrhœa and Dysmenorrhœa.* (Bulletin Général de Thérapeutique, September 30th, 1861.)

Some recent observations have been made in Italy by MM. Gallico and Poggeschi, on the therapeutical action of apiol in amenorrhœa and dysmenorrhœa. Ten cases are recorded by M. Gallico, all proving the efficacy of apiol in these complaints, and the following is one of the most striking instances of the beneficial results following its employment. A lady, aged thirty-three, had suffered for fifteen years from severe nervous symptoms of an hysterical character, for which many remedial measures were recommended, and especially bleeding. These symptoms were aggravated at the menstrual periods, and the discharge presented the appearance of washings of flesh mixed with serosity, lasting from four to twelve hours, and accompanied by uterine tenesmus. M. Poggeschi determined to treat this dysmenorrhœa at the onset, and therefore at the first appearance of the return of the menstrual period he gave the patient a capsule of apiol in the morning and another in the evening. On the evening of the next day a slight flow of blood appeared, but was suppressed in the night. On the morning of the second day the menstrual discharge returned pretty abundantly, and with all its normal physical characters; it was no longer a serosity tinged with blood. It lasted for the first time for three days, without the appearance of the habitual nervous derangement. The next month the use of the apiol was resumed, and the menses appeared as before. From this period the lady obtained more relief than she had experienced for fifteen years, and she therefore desired to continue the use of the apiol for several menstrual periods, and thus became perfectly well.

IV. *Case of Ascites cured by an Injection of Iodine.* (Journal de Médecine de Bordeaux, September, 1861.)

A woman, aged twenty-nine, at the termination of an obstinate attack of intermittent fever followed by hypertrophy of the spleen, bronchitis, inveterate epistaxis, and intercurrent anasarca, accompanied by hydropericardium, was affected with permanent ascites, and œdema of the lower extremities. Dr. Galli employed various kinds of diuretics, and in less than two years he punctured the abdomen twenty-one times. On the occasion of puncturing it for the twenty-second time, he injected a solution of ten grammes of the alcoholic tincture of iodine and one gramme of iodide of potassium in one hundred grammes of distilled water; he then made slight pressure on the walls of the abdomen, and about a minute after the injection he evacuated the serosity still contained in the peritoneum, mixed with the decomposed solution of iodine and presenting numerous coagula of albumen. There was immediately acute pain; a simple diachylon plaister was applied to the wound, and a moderately tight bandage round the body. Peritonitis of a severe character ensued for thirty-six hours, but it was treated successfully by ice given internally, and two poultices containing laudanum. Afterwards an emulsion was given containing acetate of potash and castor oil, which produced abundant diuresis, and the cure was completed. The patient left her bed a week after the operation, and in another week she was employed in her usual avocations.

V. *On the Use of Cubebs in the Simple Urethritis of Women.* By Professor Trousseau. (Bulletin Général de Thérapeutique, July 15th, 1861.)

Simple urethritis is a disease which is tolerably frequent in young girls, but more frequent in married women, and it is characterized by a frequent desire to make water, with severe smarting during micturition, and vesical tenesmus lasting some minutes afterwards. Trousseau has known patients obliged to go to the water-closet ten or fifteen times in an hour, and after having passed a few drops of urine they went on straining for a minute or more, so imperious was the sensation of desiring to evacuate the bladder, which, however, was perfectly empty. In some cases, it would appear that the inflammation is propagated to the mucous membrane of the bladder. Trousseau has never found this disease to assume a serious form, and it is seldom accompanied with fever, but there are often pains in the loins and hypogastrium analogous to those observed in cystitis and metritis. Urethritis is rather common after lying-in, after miscarriage, or at the period of menstruation, and in some cases it appears to be connected with an herpetic diathesis. Whatever may be the exact form of this disease, cubebs are successfully employed in its removal. Trousseau has employed this treatment for more than twenty years, and the use of cubebs was suggested by its efficacy in venereal blennorrhagia. In simple urethritis it is not necessary to give large doses of cubebs, and in general the powder may be prescribed in the dose of ʒss. to ʒj., twice a day, at meals. It should be continued several days, and as long as the symptoms last; when improvement begins the cubebs are given only once a day for a week, and in the following week, if the improvement continues, the cubebs are given only once every second day.

VI. *Some Observations on the Therapeutical Applications of Glycerine.* (L'Union Médicale, August 1st, 1861.)

In a memoir lately written by M. Demarquay on the subject of glycerine, the author makes the following remarks on the therapeutical properties of that substance. When applied to the organic tissues, glycerine penetrates and soaks them so as to render them transparent, but it dissolves some of them, and it is thus that by prolonged contact it contracts, attenuates, and dissolves the blood-corpuscles. Applied in surgical cases, it diminishes the secretion of pus, is easily removed, and never adheres to the surface of the wound, which until the period of cicatrization is always clean, and therefore never requires washing. In burns, deep abscesses, fistulous passages, ulcerations, and chancres, glycerine, combined with appropriate medication, assists the internal treatment very remarkably. In diseases of the skin, except those which are associated with fever, glycerine has been employed beneficially; and in hyperæsthesia of the genital organs, a disease which is often very obstinate, the application of pure glycerine has proved very useful. It soothes the pain of zona, and has been serviceable in pruriginous affections. A good preparation for the skin is a mixture of glycerine and collodion, which is supple and adheres closely to the surface. Itch has been cured by a single friction of glycerine and sulphur, and lupus has been cured by an application of glycerine and iodide of potassium. Glycerine is especially useful in vaginitis, and is sometimes introduced into the vagina by means of plugs soaked with glycerine containing tannin or alum. It has also been employed internally in cases of phthisis, scrofula, dysentery, &c., but experience is still wanting to prove its efficacy in these diseases.

VII. *On the Therapeutical Effects of Colchicum Autumnale in Rheumatism and Gout.* (Archives Générales de Médecine, July, 1861.)

The late Dr. Goupil, of Rennes, has left a posthumous work, in which he explains the experimental results which he obtained as to the action of colchicum, in a series of observations made upon his patients and upon himself. It has been generally believed that the efficacy of colchicum depends upon its purgative action, because the alvine evacuations often coincide with the disappearance of pain; but Dr. Goupil thinks that this view is founded upon a serious and dangerous error. He shows that when colchicum is taken in very minute doses, it sometimes happens that the pains cease without any evacuations having occurred; and as to the effect of purgatives administered in combination with colchicum, they tend to diminish its action rather than to increase it. He therefore advises the employment of colchicum alone instead of the usual compound formulæ. He considers that the real action of colchicum is that at first it irritates slightly the digestive passages, and afterwards produces symptoms which may be referred to the nervous system; it is then absorbed, circulates with the blood in all parts of the body, and on its return, or secondarily, it increases the intestinal, the urinary, and even the cutaneous secretion. If the drug has been taken in small and repeated doses, it may happen that the gout ceases without any evacuation by the bowels, but after a more or less abundant evacuation of urine or perspiration. As for the bad effects which are attributed to colchicum, such as metastasis of the disease, intestinal ulcerations, and death, Dr. Goupil thinks that some of the consequences are due to the medicines with which colchicum is often associated, and that when the drug is used carefully, and not given for too long a time, no unpleasant results are to be feared.

VIII. *On the Preparation and Uses of Natural Iodized Wine.* By Dr. Boinet. (Bulletin Général de Thérapeutique, October 15, 1861, p. 313.)

The employment of iodine in the natural state is not a new idea, for it may be traced to a remote antiquity, although it was formerly given empirically, and without any knowledge of its administration. Thus Dioscorides and Galen recommended sponge in goître and scrofula, and in the same diseases the Chinese administered sponge and sea-plants reduced to powder. In the thirteenth century, Arnaud de Villeneuve, a physician of Montpellier, treated goître and scrofula successfully with burnt sponge, and up to the period when iodine was discovered, no physician employed any other remedy. But since chemical analysis proved the existence of iodine in sea plants, mineral waters, cod-liver oil, &c., it has been known why these substances, apparently so different, agree in their curative powers; and iodine in the pure state has been employed in medicine to the exclusion of iodized bodies in their natural condition. The researches and observations of MM. Boussingault, Cantu, Fourcault, Meyrac, Niepce, Grange, Chatin, &c., appear to prove that goître, cretinism, and scrofula do not exist or are much less common in all the countries of the world where iodine is found in sufficient quantity in the soil, the air, the waters, and the food, and that the energy of the vital functions is in direct proportion to the quantity of iodine in the economy; and Dr. Boinet therefore argues that iodine should be regarded as an article of food rather than as a medicine, since it enters into the composition of so many substances necessary to life. He thinks that the best mode of administering iodine is in a state of minute subdivision with various articles of food and drink; and, on the whole, he gives the preference to iodized wine, because this is the form which appears to him the best adapted for administering iodine during meals, and because wine (in France) is a daily article of consumption, and agrees best with everybody. In preparing this iodized wine, Dr. Boinet adopts a process which appears to him to fulfil the object he has in view, namely, to form a natural iodized preparation which is more agreeable to the tongue, the throat, and the stomach, than the ordinary preparations of tincture of iodine and iodide of potassium. He prepares this wine at the time of the vintage, by placing in the vat a thick layer of bunches of grapes, and over them a layer of sea plants reduced to powder, or even a layer of ashes from the same plants, then another layer of grapes, then another layer of sea plants, and so on in succession until the vat is full. The vatting then begins, and lasts from fifteen to twenty days, according to the temperature. When the wine is fit to draw out, and the alcohol and carbonic acid formed by fermentation have dissolved the iodine contained in the sea plants, then the wine is transferred into casks, being extracted from the vat by means of a stop-cock. The wine thus prepared is called by Dr. Boinet natural iodized wine (*vin iodé naturel*); it is preserved as well as common wine, and is not, like the syrups, liable to fermentation and decomposition. The dose in which it is given by Dr. Boinet is from two to three table-spoonfuls a day for adults, and two to three tea-spoonfuls a day for children. He recommends, in cases where the use of iodine is indicated, that this wine should be used for a long time and in small doses, as he thinks such a course preferable to the administration of large doses in a short period.

IX. *On the Employment of Acetate of Lead in Incipient Hypertrophy of the Heart.* By Dr. VALENTIN. (L'Union Médicale, September 12, 1861.)

Dr. Valentin does not claim to be the first originator of the treatment of cardiac hypertrophy by the acetate of lead, for in 1855, Dr. Brachet, of Lyons, proposed this method in a memoir read to the Academy of Medicine. Although the latter physician avowed his ignorance of the mode in which the acetate of lead operates in such cases, he thought its action might be due to its astringent properties, which assisted the contraction of the capillaries, and thus produced the absorption of the hypertrophied molecules. According to his views, it would have an elective and special action on the heart, analogous to that of mercury on the salivary glands, of cantharides on the bladder, and of diuretics on the kidneys. M. Brachet's opinions, however, were disputed on rather strong grounds, and Dr. Valentin now endeavours, by a new series of observations, to throw some further light upon the question. He first adduces some clinical facts to show that acetate of lead really produces a definite astringent effect upon the vascular system, and he then relates some cases in which the heart was apparently enlarged, and in which the symptoms were relieved by the use of the salt. He does not assert that old aneurisms or confirmed organic diseases of the heart can be remedied by this kind of treatment; but if it be admitted that these diseases commence by a slight increase in the tissue of the heart and great vessels, causing tumultuous movements of the chest, want of breath, oppression, cough, nightmare, &c., and if it be proved that these symptoms are relieved by the treatment now proposed, then Dr. Valentin thinks that the profession must recognise the efficacy of sugar of lead in hypertrophy of the heart.

X. *On the Efficacy of Pepsine in the Inanition of Newly-born Children.* (Moniteur des Sciences Médicales, July, 1861.)

The efficacy of pepsine has been often proved in cases where the stomach is incapable of the act of chymification, and Dr. Joulin has employed this substance successfully in the following case of a newly-born child. The infant was born at the full time, but was incompletely developed, presenting only the appearance of a seven-months' child. Dr. Joulin considered that this condition was owing partly to fibrous degeneration of the placenta, and partly to the fact that the milk was vomited after each meal. He therefore ordered one gramme (about fifteen grains) of pepsine to be divided into ten powders, and one powder to be taken in a few drops of sugared water, while the mother allowed the milk to flow into the infant's mouth, for it had not strength enough to take the breast. This treatment was commenced on the 8th of May, and no improvement was manifested until the 11th, but on that day the diarrhœa began sensibly to diminish, the voice was stronger, and the suction was more vigorous. On the 20th the digestion was good, and the vomiting and diarrhœa had finally disappeared; but the pepsine was continued till the 30th of June, and the child eventually became perfectly well. Dr. Joulin, from the results of this case and of some others which he has met with in practice, advises pepsine to be employed whenever the practitioner is called to cases of congenital feebleness with arrest of development of the digestive system, and even in complicated cases in which the lesion affects at the same time the digestive and respiratory systems. By removing one of the complications which threaten the life of the child, nature is frequently enabled to complete the cure, and the improvement in the digestive system is the first object to be accomplished.

XI. *On the Employment of Iodine and the Iodides in Tuberculous Meningitis.* (Bulletin Général de Thérapeutique, August 30, 1861.)

Dr. Bourrousse de Laffore has lately published a treatise in which he proposes to demonstrate by cases the curability of tuberculous meningitis by means of iodine. He has administered iodide of potassium in large doses in acute hydrocephalus, and this treatment has been attended with success. Eight cases are recorded in which the symptoms of acute hydrocephalus were well marked, and which were all cured. The effects of the iodide were the more quickly developed in proportion to the more early stage of the disease. Thus, in three slight cases, the improvement was manifested almost immediately after the administration of the medicine, while in five other cases, in which the disease was more advanced and much more severe, the beneficial effects were not observed before the expiration of forty-eight hours. This difference in the results appears to demonstrate that the tuberculous granulations of the meninges resist the action of the iodide in proportion as these granulations are more highly developed. Dr. Bourrousse de Laffore therefore concludes that in order to control this disease there is a positive advantage in administering the iodide as soon as possible.

The editors of the 'Bulletin Général de Thérapeutique remark that whatever may be the theoretical explanation of the facts adduced by Dr. Bourrousse de Laffore, the facts themselves are of great importance; and if his observations should be confirmed by those of other practitioners, a very great service would be rendered to medical science in bringing within the reach of medicine a disease which has hitherto been considered almost always incurable.

XII. *On the Absorption of Iodine by the Skin; and on the Treatment of Pleurisy and Endocarditis by Frictions of Iodine.* By Dr. Délioux, of Toulon. (Bulletin Général de Thérapeutique, September 30th, 1861.)

From repeated observations made upon patients, Dr. Délioux has been convinced that iodine, when rubbed into the skin, is absorbed into the system; and although it is introduced in less quantity into the tissues than when it is swallowed by the mouth, yet it is found by the usual reagents in appreciable quantity in the excreted fluids. In fact, the compounds of iodine, when rubbed into the skin, are eliminated from the system in the same manner as substances which enter by other surfaces; and the saliva and the urine, in particular, are found to contain the absorbed iodine. The reagents employed were merely hydrochloric acid and starch; and although the blue colour, characteristic of iodine, was less intense than when the iodine was swallowed, it was nevertheless perfectly appreciable in most cases; and by a more delicate series of tests than those usually employed, it was shown that the iodine could be detected in every instance.

After M. Délioux had thus proved that the iodine was really introduced into the system by the iatraleptic method, he was induced to employ iodine frictions in the treatment of some inflammations of serous membranes situated near the surface of the body. The preparation which he prefers is an ointment composed of two parts of iodine, eight parts of iodide of potassium, and thirty parts of lard. This preparation is very active, and never fails to irritate the skin, and therefore it should be employed with some caution in persons whose skins are delicate, but it introduces more iodine into the system than any other local application of iodine. The skin should previously be thoroughly cleaned in order to leave its pores open, and the ointment should be rubbed in vigorously for five minutes at least. The frictions should be made largely over all the surface corresponding to the diseased part, and rather exceeding its limits, and should be repeated night and morning. Dr. Délioux states that he now possesses the records of twenty cases, in which this method of treatment has subdued intra-pleural exudations, some supervening on acute pleurisy, and others, though fewer in number, of a chronic nature. The treatment lasted from a fortnight to two months, but in general fifteen to twenty days were sufficient to effect the resolution of the false membranes produced by acute pleurisy. In order that the therapeutical observations might be clearly made, the patients with pleurisy were subjected to the iodine frictions to the exclusion of every other remedy. Some had been previously treated by blisters, and it was because blisters alone had not effected a cure that the iodine applications were employed; and it appeared, moreover, that the absorption of iodine was more energetic over the scars of the blisters. Dr. Délioux has applied the same treatment to some cases of pericarditis without decisive results, but he thinks that pericardial pseudo-membranes may be treated advantageously by the iodine frictions. But in endocarditis following articular rheumatism, Dr. Délioux considers this treatment to be decidedly successful, and he states that in the case of two patients, among others, treated diligently with iodine frictions over the præcordial region, he succeeded in obtaining the complete cessation of the blowing murmur over the heart, and probably the resolution of the endocardial effusion which gave rise to it. It does not appear that the simultaneous internal employment of iodine is of any peculiar efficacy, and the resolution of the false membranes was not at all more rapid in those cases where iodine was given internally, than where iodine frictions alone were employed.

XIII. *On the Use of Chlorate of Potash in the Treatment of Consumption and Scrofula.* By Dr. Harkin, of Belfast. (Dublin Quarterly Journal of Medical Science, November, 1861.)

Dr. Harkin was induced to employ chlorate of potash in the treatment of consumption and scrofula, in consequence of the large quantity of oxygen contained in that salt, and his reasoning is that as these diseases are in great measure due to respiration in air containing a deficiency of oxygen, and to the consequent deposition of carbon in the form of tubercle, so the cure is to be sought for in a highly oxygenated body. Repeated observation has satisfied Dr. Harkin that by the use of chlorate of potash the blood is changed and its qualities are improved, and from his experience of its effects during a period of nearly eighteen months, he considers himself justified in stating, that by using this simple remedy a specific will be found for consumption in its first and second stages, and that even in the last it will be found very efficacious in controlling the hectic symptoms and the colliquative diarrhœa, without increasing the perspirations as in the administration of ordinary astringent remedies. The curative effects of the salt in scrofula are said by Dr. Harkin to be perfectly astonishing; fifteen or twenty days generally suffice to heal the most extensive ulcerations of the cervical and submaxillary glands; strumous ophthalmia yields immediately to its internal use, and glandular enlargements and indurations of a scrofulous character in any part of the body, appear to subside with wonderful rapidity. Dr. Harkin gives it internally in doses of from five to twenty grains, dissolved in water, four times a day. He has also used it as match-paper, burning it in the bedroom before the patient retires to rest, and an ointment prepared by triturating two drachms of the powdered salt with an ounce of lard, is also very serviceable in the removal of tumours, or for inunction over enlarged joints. When administered in cases of consumption, its effects are manifested in a feeling of tonicity and vigour imparted to the system, the colour of the skin, and particularly of the nails, gradually improves, the cough diminishes, and diarrhœa, if present, gradually disappears. As the treatment proceeds, the physical signs become less marked, the blood appears altered in character, its fibrine and red corpuscles increase, muscular energy gradually returns, and in some instances even a disposition to plethora is developed. Dr. Harkin gives the particulars of three cases in which the use of chlorate of potash was attended by well-marked benefit; one was a case of phthisis, and the other two were cases of strumous swellings in the neck and legs. In the case of phthisis, the disease had reached the commencement of the stage of softening, as was indicated by moist crepitation on auscultation and dulness on percussion; but by the insertion of a seton, kept open for several months, and the continuous internal use of chlorate of potash, all the symptoms, both general and local, were favourably modified, and the patient was to all intents and purposes quite cured. The other two cases, in which there was scrofulous swelling (in one of them ulceration) of the cervical glands, with other evidences of tubercular disease, were entirely cured by the use of the chlorate. Dr. Harkin relates another case, in which the chlorate of potash appears to have acted most beneficially in controlling the diarrhœa of phthisis.

XIV. *On the Therapeutical Action of Sesquicarbonate of Ammonia in Scarlet Fever.* By Dr. John McNab, of Oban. (Edinburgh Medical Journal, October, 1861.)

Dr. McNab considers it to be an authentic and incontestable fact, that the sesquicarbonate of ammonia exerts a decided and well-marked beneficial effect on all the varieties of scarlet fever, and he thinks that its action must be recognised as special or specific, just as quinine has a specific action in ague. In the spring of 1859, an epidemic of scarlet fever broke out in Oban, and spread extensively throughout the adjoining districts, continuing with unabated violence for about eighteen months. The variety of the disease that chiefly manifested itself was the anginosa, but there were several cases of both the simplex and the maligna. Shortly after its appearance, Dr. NcNab was casually led to use the sesquicarbonate of ammonia as a gargle for the throat, and he soon became deeply impressed with its beneficial effects, so that he afterwards employed it internally. When used as a gargle, it was prescribed in the proportion of two drachms of the salt to six ounces of water, and when ordered internally it was in five to ten grain doses, according to the age and strength of the patient, and given three times a day. Dr. McNab considers that the specific therapeutical effects of the sesquicarbonate of ammonia in scarlet fever depend upon its direct influence on the special blood-poison present in the system; and that the sesquicarbonate possesses the power of changing or modifying the action of the blood-poison, or in other words, of neutralizing the *materies morbi*. But apart from any specific influence it may exert, it seems to be of essential service in the treatment of the disease, in consequence of its diaphoretic properties, and from the circumstance that it is a prompt diffusible stimulant, especially of the nervous system, while it possesses the advantage of not accelerating the circulation. Dr. McNab states, that the anasarca, so often a sequela of scarlet fever, is not so frequently observed in those cases where the sesquicarbonate is administered in the early stages of the disease, as when it has not been given at all, or given only when the disease is fully developed. He considers that this favourable result is probably due to the diaphoretic operation of the remedy, which, when freely administered, relieves the kidneys of the undue action they would otherwise be obliged to perform in eliminating the blood-poison through the urine.

XV. *On the Internal Employment of Chloroform in Biliary Calculi, Hepatic Colic, and Nervous Complaints.* By Dr. E. Bouchut. (Bulletin Général de Thérapeutique, July 30, 1861.)

In employing chloroform internally, M. Bouchut recommends that it should be given in combination with alcohol, which is its best solvent. The alcoholic solution of chloroform is soluble in all proportions in water, and it may be administered either by the mouth or in injection. The proportions ought to be, one part of chloroform to eight parts of alcohol. This alcoholic solution forms a permanent syrup with sugar, and may be preserved for several months without alteration, and it also mixes with wine, giving to the latter a very agreeable flavour. M. Bouchut employs these preparations of alcoholized chloroform—namely, a syrup, a wine, and a watery solution. Before administering these preparations to patients, he made some experiments on dogs, the results of which proved that by the above-mentioned admixture, the anæsthetic action of chloroform was diminished, and that the drug might be thus administered to the human subject without inconvenience in small doses. The wine and the syrup were given to some young children suffering from chorea, and although the disease was not removed, no disagreeable effects were produced, with the exception of a little headache and insensibility. The syrup was also taken by M. Bouchut's house-surgeon, who suffered only a little headache, without insensibility. The drug was completely absorbed, for his urine contained a notable proportion of chloroform. A little girl, affected with epileptic vertigo, had an injection administered to her every day, consisting of two parts of chloroform, two of alcohol, and 250 of distilled water; she felt a little heat of stomach, with heaviness in the head, and a kind of transient intoxication, but no anæsthesia, and the vertigo was nearly removed by the treatment. From the observations made upon the lower animals and on man, it follows that chloroform, employed internally, in solution, in water, or in wine, does not possess the anæsthetic properties which might be attributed to it. In the digestive canal, it produces no effect in a dose which, when taken in vapour into the air-tubes, would produce anæsthesia. M. Bouchut considers that chloroform exercises a well-marked solvent action on biliary calculi, and much more powerful than that of ether. If half a biliary calculus be placed in a test-tube in a saturated solution of chloroform, and the corresponding piece in another tube, in a solution of ether, it will be found the next day that the calculus placed in the chloroform is dissolved and communicates to it an evident yellow colour, while the calculus placed in ether is scarcely affected, and the solution remains colourless.

M. Bouchut is of opinion that chloroform, administered internally, dissolved in alcohol, might be advantageously employed in certain nervous affections and in cases of hepatic colic occasioned by biliary calculi. He has employed it both by the mouth and by the rectum, in chorea, epileptic vertigo, and neuralgia. In four cases of chorea, so treated for a few days, a real improvement took place in one of the patients, and in three cases of epileptic vertigo, there was one in which the symptoms immediately disappeared. In many cases of neuralgia there was a complete cure.

Ether mixed with turpentine has been often employed in cases of biliary calculi, and in fact these substances exercise a positive action upon cholesterine, which they dissolve pretty well, and hence their efficacy in medical practice; but M. Bouchut has shown that chloroform dissolves biliary calculi much more readily than ether, and therefore that it should be preferred to ether in cases of hepatic colic caused by gall-stones. He records one case of hepatic colic, dependent upon the cause just indicated, and accompanied by jaundice, where the use of syrup of chloroform was attended with complete success. The important conclusions at which M. Bouchut has arrived are thus summed up: 1. Chloroform and ether may be dissolved in alcohol, in the proportion of one to eight, and the solution is soluble in water, wine, and syrup in all proportions, so as to form an aqueous or vinous drink, or a very agreeable elixir. 2. The preparations of ether and chloroform made according to M. Bouchut's formula are stable, for they may be preserved without change for eight months. 3. The wine, water, and elixir of chloroform and ether never produce complete anæsthesia. 4. These preparations of chloroform allay nervous excitement, immediately relieve pains, and cause vertigo. 5. The wine, the water, and the syrup or elixir of chloroform are useful in certain convulsive nervous and mental affections, particularly in chorea and in epileptic vertigo. 6. The soluble preparations of chloroform and ether act more energetically in the form of injection, than when administered by the mouth. 7. The alcoholic solution of chloroform acts more quickly on biliary calculi, and dissolves cholesterine better in the cold than solution of ether employed in the same dose. 8. The solvent action of chloroform on cholesterine justifies the employment of this substance in hepatic colic. 9. Lastly, in a case of biliary calculi, producing fits of hepatic colic, and attended with some icteric coloration of the skin, the syrup of chloroform effected a cure.

XVI. *On the Action of Potash, Soda, Lithia, Lead, Opium, and Colchicum on the Urine.* By Dr. William Moss, of Philadelphia. (American Journal of the Medical Sciences, April, 1861.)

Dr. Moss made a series of experiments on himself in order to determine the influence of the above-mentioned substances on the quantity of the urine, and the amount of its solid organic and inorganic constituents, with especial reference to the amount of urea and uric acid. The results are carefully tabulated, and the general conclusions are the following. Under the use of potash, the excretion of urine was increased by one-half, with a notable augmentation in the amount of all its solid constituents. Soda increased the amount of water, while the quantity of organic solids was decreased, and that of uric acid was diminished one-half. The experiments with lithia confirmed the high praise awarded by Dr. Garrod to the use of this agent in gout, and although taken in infinitely smaller quantity than potash, the effects produced by it were almost precisely similar, except that it failed to render the urine alkaline. The experiments with lead also confirmed Dr. Garrod's observation, that during the ingestion of this metal into the system, the excretion of uric acid is diminished. Under the use of opium, the amount of urea and uric acid was diminished. The experiments with colchicum confirmed the view of Drs. Krahmer and Garrod, that this drug is not a diuretic, for although there was an increase in the quantity of urine and of the organic and inorganic matters excreted, it was mainly in the amount of inorganic constituents, while the amount of urea was unchanged, and that of uric acid was even diminished.

XVII. *Clinical Remarks on some of the Extracts most commonly employed in Medicine.* By M. Hirtz, of Strasbourg. (Bulletin Général de Thérapeutique, February 18th, 1861.)

M. Hirtz instituted a number of clinical experiments in order to determine the relative value of certain extracts obtained from different parts of the same plants. The extracts employed were those of aconite, belladonna, conium, digitalis, hyoscyamus, and stramonium; and the motive for making the experiments was the uncertainty which prevails as to the efficacy of a given extract, the difference in operation being due to the varying conditions in which the respective plants, or parts of plants, are employed. Thus the extract of aconite made from the leaves is without any therapeutical value; the stem of the digitalis is generally far inferior in efficacy to the leaves of the same plant; the extract from the leaves of hemlock is feeble in its operation, as conia (the alkaloïd) exists only in small quantity in the green parts of the plant; and the extract of henbane made from the leaves is easily altered in its qualities and rendered inert.

Aconite.—The cultivated aconite does not contain any active principle, and in the wild plant this principle is almost entirely concentrated in the root, the powder of which is so acrid as to be almost insupportable to the eyes. M. Hirtz administered to patients in the hospital the extract of the leaves in doses of from half a gramme to a gramme (seven grains and a half to fifteen grains), generally without producing any physiological result; but the effect of the extract of the root was entirely different. A pill containing five centigrammes (a centigramme is ·1543 of a grain) was given to a patient with asthma and emphysema, and half an hour afterwards he was seized with vertigo, partial blindness, dilatation of the pupil, extreme paleness, lipothymia, and fluttering pulse. In consequence of these dangerous symptoms, the extract was given to other patients only in the dose of one centigramme. Therapeutically it was found that cough, and particularly hooping-cough, was allayed by this extract, which appears to be superior in its action to belladonna.

Hyoscyamus.—While the active principle of aconite undoubtedly exists in the root, that of hyoscyamus certainly resides in the seeds, and, in fact, chemical analysis proves that *hyoscyamia* is found in greatest quantity in the latter parts. The extract of the leaves was given to patients in the dose of fifty to seventy-five centigrammes a-day, and the physiological effects were those usually described, as dilatation of the pupil and dryness of the throat; but these results were not only slightly marked, but they were rather inconstant, and even a gramme was sometimes given without their being observed. With the extract of the seeds, however, the phenomena were much better marked and more constant, and although only a centigramme was given at a dose, there were dryness of the throat, dilatation of the pupil, more or less complete blindness, paleness, and cold sweats. Therapeutically, the extract of the seeds was efficacious in phthisical coughs, and more efficacious than aconite in asthmatic attacks.

Belladonna.—While the active principle of aconite resides in the root, and that of henbane in the seeds, the active principle of belladonna occupies both those parts, and it is known besides that the berries of this plant are violently poisonous. It results, however, from analysis, as well as clinical observation, that the extract or powder of the root is more active than that obtained from the other parts. In a therapeutical point of view, it was found that the prolonged use of the extract of the root of belladonna diminished the violence of some asthmatic attacks, and warded off the paroxysms in a considerable degree; the same remedy appeared efficacious in some cases of obstinate constipation in women, two centigrammes given in the evening being followed by an evacuation the next day; and in hysterical trismus the same remedy induced relaxation of the jaws in two cases after being employed for a few days.

Datura Stramonium.—The active principle of this plant resides chiefly in the seeds. The researches of M. Hirtz prove that there exists a great pharmaco-dynamic and therapeutical similarity between datura stramonium and belladonna. The therapeutical effect of stramonium in asthma was most remarkable when used in the form of cigars; but when employed internally in the same disease it was no less efficacious, although less rapid in its action.

Digitalis.—The active principles of this plant reside chiefly in the leaves, and these are the parts employed for medicinal purposes. The observations of M. Hirtz confirm the views of other writers on the peculiar powers of this drug.

XVIII. *On the Therapeutical Employment of Olibanum.* By Dr. Delioux, of Toulon. (Bulletin Général de Thérapeutique, Feb. 28th, 1861.)

Dr. Delioux thinks that olibanum is a medicine which has fallen into unmerited disuse, and that its employment would render great service to medicine. It belongs to the class of balsamic drugs, the most interesting therapeutical properties of which are that they modify certain pathological conditions of the mucous membranes, and that they moderate or dry up the vitiated secretions which those surfaces often pour out. The well-known effects produced by the balsams of Tolu and Peru, benzoin and tar, induced Dr. Delioux to inquire whether similar results might not follow the use of olibanum. He ascertained that the latter substance was equally efficacious with the other balsamic medicines, but that it was more applicable to hospital practice than the Tolu and Peru balsams, by its greater cheapness, and superior to tar by its being more agreeable to the stomach. He employs olibanum in the form of pills containing one gramme (about fifteen grains) as a medium dose, but it may be raised to four or even to eight grammes. He has also prescribed it as an emulsion, but this form has a pungent and aromatic taste which is displeasing to some palates. In the form of vapour, it has produced favourable results as a fumigating agent; sometimes incorporated as a powder with other substances, such as belladonna or stramonium, in cigarettes; sometimes thrown on an ignited body, in order that the odorous fumes may be inhaled by the patients. These fumigations were found serviceable in cases of bronchitis and chronic laryngitis, modifying or suppressing morbid secretions, and alleviating cough and pain.

HALF-YEARLY REPORT ON PATHOLOGY AND PRINCIPLES AND PRACTICE OF MEDICINE.

By JOHN W. OGLE, M.A., M.D. OXON, F.R.C.P.
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I. *On Herpes, specially with Reference to its Connexion with Affections of the Nervous System.* (Die Gürtelkrankheit. Von Prof. Dr. VON BARENSPRUNG. Annalen der Charité-Krankenhauses zur Berlin, Band ix. Hefte 2, p. 40.)

THE author gives in detail no less than 56 cases, which he has chiefly observed himself, dividing them into the following classes:—(*a*) Zoster facialis, including (herpes) labialis; (*b*) z. occipito collaris; (*c*) z. cervico-subclavicularis; (*d*) z. cervico-brachialis; (*e*) z. dorso-pectoralis; (*f*) z. dorso-abdominalis; (*g*) z. lumbo-inguinalis; (*h*) z. lumbo-femoralis; (*i*) z. sacro-ischiadiaco and sacro-genitalis. He enlarges (with much research) upon the history of this affection, showing that it was clearly known to Celsus and Pliny; and, after discriminating between this affection and the various dyscracic and virulent eruptive diseases, establishing its dependence upon disordered function of interior organs, he goes on to consider its topography, symptoms, ætiology, and treatment.

The first 30 of these cases, observed in Dr. Kruckenberg's clinique at Halle, for the most part had already been mentioned in a dissertation by Dr. Joswich (1852) upon herpes.

In by far the majority of cases, the eruption was not only on one side of the body, but also restricted to narrow limits. The circular or girdle form was peculiar to the zoster of the trunk, not being observed in the head, neck, or extremities.

In considering the topography of the disease, bearing in mind its dependence upon derangements of internal organs, the author shows that the eruption follows the course of various nerves, and commences the description of each variety (which we shall not dwell upon) by giving what he terms the type of the variety in question. Thus, the zoster facialis, limited to one side of the face, corresponds to the cutaneous and mucous twigs of the trigeminal nerve; the labial form being restricted to the labial branch of this nerve. The occipito-collaris subdivision corresponds to the peripheric distribution of the third cervical nerve; the cervico-subclavicularis subdivision to that of the fourth cervical nerve; the cervico-brachialis to that of the cervical and dorsal nerves, which are united to form the brachial plexus; the dorso-pectoralis to that of the third, fourth, fifth, sixth, and seventh thoracic nerves; the dorso-lumbar to that of the eighth, ninth, tenth, eleventh, and twelfth thoracic nerves; the lumbo-inguinal form to that of the first lumbar nerve and the twelfth intercostal, which anastamoses with it; the lumbo-femoral form with that of the second, third, and fourth lumbar nerves, involving specially the anterior and external cutaneous, the genito-crural, obturator, and crural nerves; he sacro-ischiadic form with that of the anterior branches of the sacral nerves, which unite with the two last lumbar nerves and the sympathetic, forming the sacral plexuses from which the pudendal, great posterior, cutaneous, and ischiadic nerves (those herein implicated) proceed.

In discussing the symptoms, separate consideration is made of the accompanying fever, the inflammation of the skin, the pain, neuralgia, and other indications of nerve-irritation, the gastric symptoms, and the condition of the urine and of the blood; reference being made to the observations of Rayer on the presence of fat in the blood in these cases; and of Keller, who found* that in this disease there was great increase of the chlorides, especially chloride of sodium and phosphoric acid salts, with diminution of the sulphates and urates; also much of the ammoniacal compounds, and fat. The author, however, states that he has been unable to find any remarkable changes in the constitution of the urine in his cases.

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* Archiv. f. Physiol. u. Path. Chemie. 1850.

The ætiology of the disease is considered at some length, and it is determined that in the skin-inflammation, possessing, as it does, a typical form and course, and limited to the peripheric distribution of certain cerebral and spinal nerves or their branches, the source of inflammation is not from without, nor in the blood, but that it operates through the nerves, and in fact depends upon their abnormal irritation. It becomes necessary to inquire of what nature it may be, and at what part does this irritation occur. It cannot have a central origin, for the zoster always follows the track of one or two nerves, and is almost always confined to one side of the body. It cannot spring from cerebral sources, for then it would be frequently extended to the whole of one half of the body; nor from the spinal marrow, for then would it be as a rule symmetrical. Now in all completely developed cases the anterior and posterior roots are contemporaneously affected, consequently the excitement of the spinal nerves must occur before their exit from the intervertebral foramina, and we must now locate the point of irritation in the roots of the spinal nerves. Still the question remains, which is the root affected? And it is resolved that it must be the posterior one, because in this disease all motor influences are unaffected, and inasmuch as the affection is so frequently associated with exalted sensibility. Allusion is then made to the fact, that sensitive nerve-fibres often not only convey their specific activity, but also a nutritive one; and illustrations are drawn from the co-existence of redness of the skin in neuralgia, and injection of the eyes, with increased flow of tears, saliva, &c., in neuralgia of the trigeminal nerve; and reference made at length to the established occurrence of sympathetic fibres in cerebro-spinal nerves, owing to their communication with ganglia, by which these ganglia preside, as it were, over the trophic conditions of special organs. Where, then, it is asked, are these fibres which proceed to the surface of the body, whose morbid excitement produces the remarkable phenomenon of herpetic eruptions. The place has already been pointed out—viz., the posterior roots of the spinal nerves; and it is to the spinal ganglia which are connected with these roots that we must look for an explanation of the phenomenon under consideration. The sensitive nerve-fibres are described by Kölliker and others as passing through spinal ganglia without being intimately connected with them; whilst the ganglion-fibres arising in the ganglion-masses do not extend towards the spinal cord, but take a peripheric course along the sensitive fibres. Hence arises the neuralgia so common in zoster by a propagation of irritation from the ganglion to the corresponding posterior roots, and thus the latter may propagate their irritated condition through the spinal cord to neighbouring and symmetric nerve-regions; whilst the trophic irritation always remains on one side, because the ganglion sends no fibres to the spinal cord or receives any from it. One or two cases are quoted bearing on the question, and one is specially worthy of observation, in which intense pain at the posterior part of the whole leg, and the formation of numerous groups of vesicles with reddish-yellow contents, of various sizes, was supposed to be owing to an œdematous and hyperhæmic states of the ischiatic nerve as found after death connected with psoas abscess. Cases are also quoted showing that also a peripheric irritation of a nerve containing ganglion-fibres may cause a limited eruption of herpetic vesicles.

As respects remote causes of herpes, sudden changes of weather and youth are propitious to it. It is not, however, epidemic, and sex seems to have no influence in its production.

As regards the frequency with which it affects one or other side of the body, the author has drawn up the following table from three observers in addition to himself:—

	Mehlis.	Royer.	Fenger.	The author.	
On the right side	16 ...	37 ...	24 ...	24 =	101
On the left side	9 ...	16 ...	20 ...	32 =	77

As regards treatment, this should be expectative. The vesicles should be undisturbed and protected with simple cerate or glycerine, and pain alleviated by tepid applications. When much neuralgia exists, vesicatories are of great service in the neighbourhood of the spine, and the endemic use of morphia.

II. 1. *Ataxie Locomotrice Progressive.* Two Cases by Dr. Lecoq. (Archives Gén. d. Méd., June, 1861, p. 689.)

2. *Clinical and Historical Studies of the same Disease.* By H. Bourdon. (Ibid., Nov. 1861, p. 513.)

3. *On the Nature and Seat of certain Isolated Paralyses of Sensibility.* A Thesis. (Ibid., Nov. 1861, p. 613.)

4. *On Muscular Anæsthesia.* By C. Sigacet. A Thesis. (Ibid., Nov. 1861, p. 613.)

Of the first two (Dr. Lecoq's) cases alluded to, one was aged forty-four, the other sixty-two, and both were well characterized by the chief peculiarities of the disease—i.e., abolition of the co-ordination of movements, the persistence of the muscular force, and the constant progress of the disease. There was, however, absence of the severe pains in the limbs often spoken of by Duchesne.

Mr. Bourdon's case requires longer comment, inasmuch as after death the brain and spinal cord were minutely examined, and certain interesting changes met with. The case, as put together by M. Delaunay, was that of a man, aged thirty-eight, who presented all the ordinary symptoms of the disease in a well marked form—viz., want of co-ordination of the muscles of the legs, with retention of muscular power, paralysis of the third pair, accidental myopia, weakness of sight, genital incapacity, and long duration of the disease. But there was a want of the pains in the limbs which are so often described as coexisting, just as in M. Lecoq's two cases above alluded to; and also absence of anæsthesia and cutaneous analgesy. Moreover, there was a singular retention of muscular sensibility. Diarrhœa, involuntary evacuations, vomiting, and hiccough preceded death. On post-mortem examination, the cerebral membranes, cerebrum, and cerebellum were found perfectly healthy. The uppermost of the corpora quadrigemina on the left side was more vascular than natural. On slicing the lower part of the brain, the grey substance corresponding to the continuation of the posterior columns of the spinal cord was very vascular throughout; the capillaries appearing under the form of reddish striæ, and containing globules, but no exudation along their walls. The grey substance of the protuberance was less coloured than ordinary, and very vascular. Also the continuation of the pyramids and the anterior surface of the cerebral peduncles were less firm than they should be, but not softened, and the grey substance of the locus niger, that of the red nucleus (*olive superieure*), that of the "accessory of the locus," were less coloured than they should be, and vascular, but not softened.

On examining the spinal cord, the following were the appearances met with. The *dura mater* was very injected, but showed no traces of any exudation; it was thicker than usual at the upper part. The *pia mater* was very vascular, mainly about the lower third of the cord and about the posterior columns, to which it was very adherent, being, like them, of a yellowish hue. The *posterior columns* presented most remarkable changes. They were of a transparent yellowish-amber or yellowish-red colour, and softer than they should be, but not diffluent, and were entire. This alteration was greatest in the lumbar region, but also existed in the dorsal region, and occupying the space between the two posterior horns, and gradually disappearing in the brachial region. Nevertheless, at the upper bulbous region traces were to be met with. It was found that this change of colour was owing to transformation in the nervous tubes, of most of which only the empty sheath existed. Those which were less degenerated were still cylindrical, but of a yellowish-amber look and rugged.

The *lateral columns* were natural, excepting being of a slightly yellowish hue superficially.

The *anterior columns* in the lumbar region were less thick and firm than usual, but of normal colour.

The *grey substance* in the lumbar region and at the lower portion of the cord had lost its consistence, especially in the central part, and its fibres more or less broken; and where they were not so, the form of the anterior and posterior horns were recognisable. In studying it by horizontal layers, the conservation of the network of cellules from the posterior horns to the anterior ones could be established in places, and then one came upon broken fibres, and quantities of fatty granulations and detritus. But even in degenerated parts, the nervous cellules had not all disappeared, but the most part of them were shrunk, indented at their edges, and covered by more pigment granulations than natural. In this grey substance the capillaries were very turgid, and where it had lost its consistence, deposits of hæmatine were found.

The *ganglions of the posterior roots* in the lumbar region were much increased in size, and unusually red and vascular. They were of ordinary consistence, and their covering was thickened. Their capillaries were very dilated, and hæmatin deposits existed in them. The *ganglion-corpuscles* were entirely covered with yellowish-red granulations; some being shrunk, others very distended. Some maintained their natural connexion with surrounding nerve-filaments.

The *posterior roots.*—The nerves of the "corda equina" were flattened, and resembled strips of parchment soaked in water, being of a greyish transparent hue. All the nerve-filaments passing between the ganglia and the posterior columns had the same colour and degenerated character as those in the columns. This lesion only occupied the lumbar region. The roots of the glosso-pharyngeal, pneumogastric, facial, hypoglossal, auditory, and trigeminal nerves were natural.

The *anterior roots* of the lumbar region only were affected, and they to a slighter extent than the posterior ones.

The two external motores oculorum, and the two trunks of the common motor nerves, were very much altered—the latter having assumed the condition of greyish œdematous cords, much reduced in size and in places broken; the former being in the same state, but to a slighter extent. The roots of the pathetic nerve were of the same colour and consistency.

The author then cites cases from authors, as from Landry, Romberg, elucidating the pathology of the affection.

The two theses alluded to above treat of the same malady; and M. Sellier details the case of a man, aged forty-nine, suffering from the disease, in whom after death softening of the annular protuberance and of the cerebral and cerebellar peduncles existed, the spinal cord being slightly softened at the dorsal region; the posterior roots, though healthy to the eye, had their nerve-tubes diminished in number and size, deformed, varicose, and fragile. Similar changes were found in the posterior columns, and between the diseased tubes a large number of spheroïdal corpuscles. Free fat globules existed; the corpuscles were very fatty.

III. *Case of the Manège Movement and Rotation of the Body around its Longitudinal Axis.* By M. FRIEDBERG, of Berlin. (Acad. d. Sciences, Sept. 10th, quoted in the 'Archives Générales,' Oct. 1861, p. 503.)

The patient had been successfully trepanned, owing to fracture of the right parietal bone. Ten months after, symptoms of disease of the brain appeared; at first diabetes, then the movement of rotation, then hemiplegia on the right side, then paralysis of the pneumogastric nerve. This paralysis caused the death of the patient, in spite of tracheotomy, fourteen months after the cranial lesion. After death fracture was found in the inferior occipital fossa, and a superficial softening of the cerebellum, and of its middle peduncle on the left side.

IV. *Paraplegia caused by the Use of Kessaree Dal (Lathyrus sativus) in India.* By JAMES IRVING, M.D. (The Indian Annals of Medical Science, No. 14, p. 501.)

Several cases of this very peculiar form of paralysis, as occurring in Allahabad, were brought to notice by Dr. Irving in 1857, and cited in a former number of this Review (Oct. 1860, p. 536). Dr. Irving now communicates several particulars regarding its prevalence in Mirzapore, as returned by Mr. Dennison, collector. The parts of the country in which the disease prevails possesses a black and red clay soil and nearly black soil. In one part, Ocproodh, the per-centage of males affected was 0·17 per cent; of females, only 0·01 per cent. In Burhar the proportion was 1·17 and 0·09 per cent. of the respective sexes. In Bidjeyghur the proportions were 1·51 and 0·11 per cent. of the respective sexes. Thus the affection in this district is less frequent than in Allahabad; but in both cases the women are much more exempt than men. A table of returns shows that the affection chiefly is found among the poorer classes who feed greatly on the Kessaree, which is the cheapest grain used. A table also shows that the disease would not appear to tend materially to shorten life, as out of 815, 146 individuals had laboured from the disease for ten years, 118 for twenty years, 27 for twenty-five years, 26 for thirty years, 6 for fifty years, and 1 for sixty-eight years; and it appears that from the eleventh to the twentieth year is the period when the disease mostly exists, and then from the twentieth to the thirtieth year; but four males are said to have been affected under one year of age. Returns also are given from Moozuffurgurh, showing that in this district the disease is not so common.

This Kessaree Dal almost always produces dyspepsia, but no other ill consequence in some parts, whence it may be that its evil consequences are owing to disease of the vetch itself, and this was the opinion of Dr. J. Thomson, of the Botanic Gardens, Calcutta. Mr. Tribe, surgeon of the 41st Regiment, Madras Native Infantry, suggests the possibility that the paralytic symptoms of the disease called Beri-beri might be partly owing to the use of Kessaree dal, and this he thought before it was said that this grain produced paralysis. Dr. Irving quotes letters from several observers in various parts of India, in which doubts are thrown out regarding the supposed power of the grain to produce paralysis. It is in return suggested that this may be owing to its being only eaten in small quantities in these districts, the paralysis being only produced when Kessaree is used to the almost total exclusion of all other food, or when made into bread. It seems, from Mr. Tribe's account, that the rations served out to troops are at times adulterated with the Kessaree in Bengal and Calcutta.

Dr. Irving's communication closes by allusion to the supposition that the disease of the horse, from which he is said to be "gone in the loins," may be accounted for by the adulteration.

V. *Intermittent Neuralgic Gout cured by Quinine.* By C. R. FRANCIS, Surgeon to H.M.'s 4th European Regiment. (Indian Annals, No. 14, p. 513.)

The author details three cases, in which the disease as described by the above title was "extinguished" by quinine.

CASE I.—Col. ——, aged thirty-two, whose father died of gout, was attacked in the rainy season with severe neuralgic pain (without external indication except slight flow of tears) every night for four nights successively in the right eye, at the point of exit of the supra-orbital branch of the ophthalmic nerves from the supra-orbital notch, and in the space to which the anterior temporal branches of the auriculo-temporal nerve are distributed. Slight dyspepsia coëxisted. Everything was tried in vain until the fifth night, when a scruple of quinine and one quarter of a grain of morphia were given one hour before the expected attack. No attack came on, and on the following evening ten grains only were given, and for several evenings only five grains, and the patient became quite cured. Threatenings afterwards were always successfully met by quinine along with attention to hygienic rules.

CASE II.—A major, aged forty-five, of a gouty diathesis, was complaining of an intermittent neuralgia of the ball of the great toe, which he had had before. By regulation of bowels and full doses of quinine, a cure quickly followed.

CASE III.—A private, aged thirty-three, with chronic rheumatism in both knee-joints, shifting down to the ankle; pain worse at night; had acute rheumatism some time previously. By use of quinine in full doses, he left the hospital in nine days.

VI. *On Cretinism at the Abendberg.* By Dr. GUGGENBUHL. (Comptes Rendus, tome li. No. 24.)

The author affirms that cretinism consists in various pathological alterations in the cerebro-spinal system, which produce the irregular and slow development of the body, and the obtusion of the senses and intellect which is characteristic.

The pathognomonic symptom is the cerebral stupor, which, however, does not preclude the development of isolated faculties. The disease is evidently not necessarily hereditary, as at the Abendberg it is only the thirtieth case in which the parents showed symptoms of cretinism. The author admits, however, that the germ or predisposition is developed in the mother, as he thinks proved by the fact of there being many families in which, under similar circumstances, some members become cretins, whilst others do not. He allows the distinction between cretinism and idiocy, the latter being much less curable, whilst at the same time those affected by it are ordinarily strong and robust.

As regards treatment, it consists in general means for the purpose of strengthening the physical development, such as tepid aromatic baths, friction remedies, such as cod-liver oil, iodide of iron, electricity, good diet, exercise and walks on the mountains.

The post-mortem appearances met with are as follows:

(1.) For the most part œdema of the brain, with anomalies in the lateral ventricles, which are dilated. At a later period, softening of the neighbouring convolutions. Microscopical examination has not led to the discovery of any minute pathological appearances.

(2.) Imperfect or retarded development of certain parts of the brain, specially the anterior and posterior lobes; sometimes general atrophy; rarely, hypertrophy of the organ.

(3.) Hardening of the brain or of some parts.

(4.) Hypertrophy of the cranial bones which compress the cerebral substance.

(5.) The premature closure of the cranial sutures by inflammation frequently produces a deformation of the head in cretins and idiots; but as the same is often found in quite intelligent people, the disease cannot be attributed to such changes.

It appears that as institutions similar to the Abendberg have been established in Austria, Bavaria, and Saxony, so the Emperor of the French has directed the construction of a like establishment for Savoy, Maurienne, and Tarantaise, districts greatly affected by the disease.

VII. *On Epilepsy as a result of Malarious Infection.* By A. J. Payne, M.D., Bengal Medical Service. (Indian Annals, No. 14, p. 597.)

After speaking of the manifold ways in which the action of miasmata on the body simulates various diseases, and the consequent probability that convulsive disorders should have a place among the results, the author draws attention to a particular form of convulsive affection which, differing from eclampsia from the mode of its origin and the character of the attendant symptoms, may be designated as "malarious epilepsy." This form is often attended by no febrile disturbance; "follows, but sometimes after a long period, on malarious infection; may not have shown itself in paroxysms of ague at any period; so that the convulsive seizures may become the only active symptoms." As regards the phenomena, however, of the paroxyms, "they resemble epilepsy and epilepsy alone;" but, unlike the ordinary form of the disease, is much more hopefully to be prognosed.

The author quotes from various authors, such as Wood,[*] Romberg,[†] Handfield Jones,[‡] and Brinton,[§] who have alluded to convulsive action as occurring in malarious districts, or as being likened to malarious affections by the treatment which proved useful, but who have not in any special manner drawn attention, as does the author, to epilepsy as secondary to, and symptomatic of, an affection of the blood, which affection, whilst having other causes, is produced with great facility in certain situations by atmospheric or telluric influences, called malarious.

Several cases are then related in point, and it is specially to be observed that extremes of complexion, pallor, or fulness of colour, existed, and great muscular weakness, so that the body drops, but no syncope, no sensation or warning; the sudden feeling being, in fact, the "action of neurolysis on the muscular system," and the evidence of non-nutrition of the central parts of the brain. "It must be remembered," observes the author, "that there are other sources of neurolysis besides miasms, and mental toil is one of them."

Cases of infantile convulsion, frequently seen in Calcutta, and which stand in very close relation to malarious epilepsy, are alluded to, and one detailed. In all of them quinine afforded a cure.

The pathology of this class of disorder is to be looked for in the morbid state of the blood, of which the pallor, the tendency to œdema, and all the usual symptoms of hydræmia, are evidences, and this form of blood-disease is looked upon as being a result of defective oxidation. In this state of blood the presence of pigment in the cerebral capillaries is apt to occur, but this does not seem able to account for the convulsion, as these granules are easily removeable, and only adhere to the sides of the vessels. Allusion is made to the views of Dr. Hunt, who, in the 'Medical Times and Gazette,' 1856, suggested that a tendency to spasm and convulsion is created by abnormal excess of alkali in the blood; and the nitric-acid bath is spoken of (as brought forward by the latter gentleman) as the best therapeutic agent to be resorted to. The anti-periodic action of this acid, as pointed out by Dr. Hammond,* is highly spoken of.

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[*] Practice of Medicine, vol. i. p. 257.

[†] Vol. ii. p. 192, 205.

[‡] On Neurolytic and Agueish Disorders: Medical Times and Gazette, 1859.

[§] Lancet, March 3rd, 1860.

VIII. *On the Admission of Air into Serous Cavities.* By A. Meadows, M.D.

The author, with much ingenuity and appearance of reason, attempts to subvert the extreme fear which to a great extent prevails, of the harm which is likely to result from opening the peritoneal or pleural cavities. It is allowed that there is danger in such operations, but the author thinks that the true cause of such danger is not sufficiently considered, and the various heads of inquiry which he seeks the solution of are—1st. A consideration of the question, whether, in perforating wounds of the pleura or peritonæum, air really finds an entry into these cavities at all; these cases only being referred to when the viscera are free to perform their usual movements, and are not bound down by any adhesions. 2ndly. The effect of air on serous membranes in a state of health. 3rdly. Its influence when they are the seat of inflammatory action. 4thly. The effects produced by it on the products of inflammation. 5thly. To what other causes these results may be ascribed.

These points are variously considered, and their discussion, which we cannot now give in detail, leads to the following rules of inference—1. To get the patient in the best possible condition for the operation. 2. To disregard entirely the presence of air in which we are operating, just as we do in all other operations. Let the air only be healthy, we need not fear it will do much harm. 3. To avoid all complexity about the mode of proceeding, and make the operation as simple a one as possible. Let the wound be no bigger than is absolutely necessary for the purpose, having regard only to the letting out of the fluid, and doing this in the quickest and easiest way. 4. To use our best efforts to procure the healthy healing of the wound, by rest, cleanliness, and non-interference. 5. To look closely to the patient's general condition, avoiding all excitement, keeping up the strength, and being on guard to combat the first bad symptom.

IX. *Obliteration of the Abdominal Aorta by Embolia in a Paralytic. Softening of the Spinal Cord.* (L'Union Médicale, Août 24, 1861.)

This case was that of a man, aged sixty-one, under M. Broca's care, for dry gangrene of one of the toes of the left foot, who had suffered from loss of power of moving the four limbs for above thirty years, which had come on suddenly. The thighs were abducted, the legs flexed. Pulsation of the arteries of the lower extremities, from the iliac fossæ downwards, could nowhere be felt, and no collateral circulation could be found; nevertheless, the temperature did not appear to be diminished. On the other hand, the temperature of the knee and of the groin was exalted on the left side (the side of the gangrene). Bed sores, which he had, spread, and sphacelus of the whole foot and of the lower fourth of the leg came on. The affected limb then sank much in temperature.

On post-mortem examination, all the muscles of the limbs, except the deltoïd and the sartorius, were in a very fatty state, but maintained their form and size. The anterior columns of the spinal cord were diffluent from the fourth cervical to the lower part of the dorsal region, and of a milk-white colour. There was no trace of extravasation of blood, either old or new, and the anterior columns of the cord were slighly softened. The right side of the heart contained a decolorized, friable, greyish clot, of the size of a filbert, adherent to the cardiac wall. The abdominal aorta was obliterated at and above its bifurcation for some length, by a clot formed of two distinct parts. The two lower kinds consisting of a grey, friable, and granular mass, but slightly adherent to the walls, and prolonged into the two iliac arteries. Above this decolorized clot a second one existed, more adherent, and of a reddish colour and firmer consistence. This latter filled up the calibre of the aorta as far as the origin of the last pair of lumbar arteries, and above this point terminated in a long conical point.

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* Maryland and Virginia Medical Journal, Feb. 1861.

It appeared as if the obliteration of the aorta was caused in the first instance by removal of a portion of a clot detached from the cardiac one.

The two common and the two external iliac arteries were plugged up as far as the origin of the epigastrics, and on the right side the hypogastric artery was in the same state, as also the ileo-lumbar, the lateral sacral. The arteries of the right lower limb were free, but on the left side the femoral below the ring of the great adductor was quite obliterated, as also the popliteal and the upper part of the three arteries of the leg, by a clot tolerably recent, but containing portions of evidently an older standing.

The relation of the case is followed by interesting observations, which we have no room to quote.

X. *On Aortitis, terminated by Suppuration, and its Influence in the Production of Purulent Infection.* By E. Leudet. (Archives Générales, March, 1861, p. 575.)

The author enters at some length into the history of this affection, quoting cases placed on record, as well as some observed by himself, and after considering fully the anatomical characters as well as the symptoms of the disease, arrives at the following conclusions:—

I. The inflammation of the coats of the aorta give place in some rare cases to the formation of an abscess.

II. This purulent collection is situated in the external cellular and middle tunics.

III. It does not determine the alteration of the internal membrane, of coagulation of blood, or of pseudo-membraneous deposit on its surface.

IV. The abscess of the aorta sometimes opens into the interior of the vessel.

V. The suppurative inflammation of the arterial walls is generally consecutive to an intense phlegmasia of the endocardium or of the artery, and coincides with disorganization of the heart's substance or of the walls of the vessel.

VI. The abscess of the aorta communicating with the interior of the vessel gives place to lesions and symptoms of purulent infection.

XI. *On Pericarditis.* By W. J. Gairdner, M.D. (Reprint from Edinburgh Medical Journal, April, 1859, and February, 1860.)

This pamphlet is not sent forth by its author as in any way a systematic exposition of the subject, but contains highly valuable remarks on—1. The diagnosis of pericarditis. 2. Frequency of the disease and its results as indicated by examination after death. And 3. The prognosis and treatment of the disease.

It would be impossible to give in a short space a digest of the observations offered, and we shall therefore content ourselves with alluding to one or two of the chief points presented to our notice. Thus, in a section treating (p. 7) on the presence of friction-sound not being necessarily a proof of the existence of pericarditis, after alluding to the sounds which, contrary to the statements of many, are attendant upon the presence of loose organized fibrous tissue, and form "milk-patches," Dr. Gairdner asserts his belief, "that it is very far from true that adhesion of the pericardium necessarily leads to the absolute suppression of murmur; loose adhesions being oftener than not to be found associated with a degree of murmur." Chronic roughness of the pericardium of any kind, and under some circumstances adhesions, even when close and universal, may cause a distinct friction-sound.

The remarks upon the important question of treatment are summed up as follows:—The principles of safe treatment are—1. To make large allowance for the insignificant and spontaneously healing class of cases revealed more by physical signs than by symptoms, and to regard them as demanding little active treatment. 2. To consider rheumatic pericarditis in general as a disease susceptible, to a great extent, of cure under mild palliative local remedies and fitting constitutional treatment. 3. To hold the constitutional treatment as subordinate to that of the disease with which the pericarditis is associated.

XII. 1. *On the Influence of Lactic Acid upon the Endocardium and in the production of Rheumatism.* By JUL. MÖLLER and CORNELIUS RAUCH. (Virchow's Archiv, Band xx. Hefte i. and ii., p. 211.)

2. *On the Production of Endocarditis by injection of Lactic Acid into the Peritoneal Cavity of Animals.* By Dr. G. REYHER, Assistant-Physician to the Clinic at Dorpat. (Virchow's Archiv, Band xxi. (Zweite Folge, Band i.), Heft i., p. 85)

1. The observations of Möller and Rauch are reported by Virchow—in the case of Möller partly from the 'Konigsb. Med.,' Jahrb. ii. p. 2, s. 277; and in that of Rauch from an Inaugural Dissertation (Dorpat, 1860).

Referring to the history of the question, the Reporter notices the experiments of Dr. Richardson, in which he seeks to prove directly what Prout had originally supposed, which Williams and Todd had also conjectured—that the accumulation of lactic acid in the body was the cause of rheumatism. He also alludes to the observations of Schenlein, of Joseph Meyer, and of an anonymous writer, "A. W." on the same subject.* He remarks that none of the German observers, in their experiments, had found that rheumatoïd affections of the limbs had been produced, and Virchow suggests that possibly in certain cases in which it was supposed that such had been artificially produced, the appearances were the result of injury to the joints occasioned in the necessary operation of binding the animal's limbs.

It was, however, quite different with regard to the influence of lactic acid upon the endocardium. On this point the observations of Richardson had been abundantly confirmed by late experimenters. In eight cases in which Rauch had injected lactic acid into the jugular and abdominal veins, changes in the endocardium, and gelatinous thickening of the valves, were produced; and even eight hours after injection into the jugular veins these effects were found. Rauch, who had minutely examined the thickened and swollen granular parts of the valves, takes exception to the statement of Richardson, that it was the result of an exudation. He found, as Virchow had often previously found in the human subject, that it consisted of a soft mucus-like tissue, with an increase and growth of the connective-tissue corpuscles, which at a later period undergo fatty changes, and that an epithelium existed. He concluded that the influence of the lactic acid was exercised not upon the free surface, and this especially, as the right side of the heart was not much affected.

Dr. Möller looks upon the swelling and redness found at the edges of the valves as a post-mortem phenomenon, and says they do not occur in animals examined directly after death. Both Rauch and Möller found in some cases a want of rhythm in the heart's movements and systolic bruits. Möller considered the murmur to be anæmic, whilst Rauch attributed them to an affection of the valves.

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* Götting. gelehrten Anzeige, 1859, st. 168, s. 1668.

2. In the second of the papers alluded to in the heading, the author first sets forth the views of Richardson and Rauch,* whose observations lead them to agree in the main; and alluding to the statements of A. W. and J. Möller† and others on this question, entertaining some doubts as to the correctness of the alleged causation of endocarditis by injecting lactic acid into the body (as professedly exhibited in the case of the dogs, &c., experimented upon); and encouraged by Virchow, the author was led to undertake a series of examinations of the hearts of dogs which were killed without having had lactic acid injected into them. These dogs were of every age up to nineteen years old, were killed in the speediest possible way by prussic acid or the blowing of air into the jugular veins, and their hearts were examined as quickly as could be. On opening the very first heart of an apparently healthy dog, the author found that the very same anatomical changes in the heart's valves existed as Rauch and Richardson found after their injection of lactic acid. Thirty-two dogs were sacrificed in all, and similar changes, in a greater or less degree, and evidently of various dates, were universally found, except in fœtal hearts. Moreover, in one dog, in which the writer had during life injected a solution of lactic acid (according to Richardson's directions) into the peritoneal cavity, and which died twenty hours after, on examination after death the results of a *chronic* endocarditis were found, but nothing like the remains of any acute endocarditis. The various conditions of the valves and their appendages, as displayed after death in these dogs, are minutely given, and their development, as it were, traced from the simplest up to the most decided form of alteration. Moreover, the microscopical characters of the altered valves and new formations are elaborately given in detail, sections as well of the fresh structures as of prepared parts having been carefully examined. For these general and minute descriptions we have not room in this place. In conclusion, from the observations of the author the following are interesting:

1. That dogs, and probably also many other mammalia, are not so free from pathological changes in the heart as is supposed, and consequently that great caution is necessary in drawing inferences from such experiments upon these animals. (Dr. Albaum also found in the hearts of young horses, cats, and rabbits, similar changes to those found by the author in the dog.)

2. That in all the thirty-two apparently healthy dogs whose hearts were examined, the changes observed were "identical" with those described by Richardson and Rauch as the result of lactic acid injection, and called by the latter "parenchymatous endocarditis."

3. As Richardson and Rauch have not sufficiently avoided sources of error (owing to neglect of the examination of healthy dogs), their investigations are of no value in support of the supposed origin of endocarditis from the injection of lactic acid into the peritoneal cavity or jugular veins.

4. Consequently, that endocarditis arises from an accumulation of lactic acid in the blood remains *un*-proven.

XIII. *Pharyngoscopy in the Treatment of Diseases of the Ear.* By Dr. VOLTALINI, Breslau. (Virchow's Archiv, Band. xxi. (Zweite Folge, Band i.) Heft 1, p. 45.)

It is not proposed to do more than to draw attention to this communication. In it the author describes the instrument which he uses (of which drawings are given), which consists of a tongue-spatula, at the end of which is attached, at an angle, a polished steel plate like that used in the laryngoscope. The mode of using this instrument, specially in investigating the condition of the Eustachian tube, the methods of illumination, &c. &c., are described, and the author makes reference to former observations regarding treatment of ear-disease to be found in the 'Deutsche Klinik,' Nos. 21 and 40, 1860, particularly referring to the operation proposed by himself of passing the catheter through one nasal cavity into the Eustachian tube of the "*opposite*" side.

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* Inaugural Dissertation above quoted.

† Symbolæ ad theoriam Rheumatismi Criticæ et Experimentales. Regimonti, Pr. 1860.

Cases are given having considerable pathological interest, and exhibiting the utility of pharyngoscopy. The paper concludes with some observations upon (so called) œsophagoscopy and its difficulties.

The following papers, for the detailed notice of which we have no space, are worthy of being referred to:

Five Cases of Diseases of the Spinal Cord. By Prof. Traube. (Annalen d. Charité-Krankenhaus, Band ix. Heft 2, p. 129.)

On Chromidrosis, or Dark Coloration of the Eyelids. By Dr. Bellier. (Arch. Gén., Août, 1861, p. 187.)

On the Disease called Beri-beri. By MM. Foussagrines and Le Roy de Mericourt of Brest. (Ibid., Sept., p. 257.)

On the Form of Paralysis in Young Children termed "Essential." By Dr. A. Brunniche. (Ibid., Oct., p. 405.)

On Tænia in Man. By Dr. W. F. Pissling. (Wochenblatt d. Zeitschr. d. k. k. Gesellschaft. d. Aerzte zu Wien, April 9th, 16th, 1861.)

Case of Congenital Atresia of the Duodenum. (Ibid., Aug. 7th.)

On Diseases of the Pulmonary Artery. By Dr. Erichsen. (Petersburger Medicin. Zeitschr., Band i. Heft 4, p. 89.)

Observations on Tracheotomy resorted to in the last stages of Croup. By Dr. E. Moynier. (L'Union Médicale, 15, 17, and 22, Aug. 1861.)

On the Effects produced upon the Encephalon by Obliteration of the Arterial Vessels distributed thereto, &c. (Ibid., Nov. 16th, 1861.)

On the Diagnosis and Treatment of Hydatids of the Lungs and Pleuræ in Children. By H. Roger. (Ibid., Nov. 16th, 19th, 1861.)

Case of Croup in the Adult. By R. Bruce, M.D. (Edin. Medical Journal, July 1861, p. 20.)

On the Supposed Influence of Ozone on the Pulse. By W. R. Hill, M.D. (Ibid., p. 41.)

Case of Acute Cerebral Softening, with Contraction of Flexors of Arms and Paralysis of Tongue in a Case of Bright's Disease. By J. A. Marston, M.D. (Edin. Journal, Sept. 1861, p. 256.)

Contributions to the Doctrine of Uræmia. By S. Oppler. (Virchow's Archiv, Zweite Fogle, Band i. Heft 3.)

Two Cases of Acute Miliary Carcinoma. (Ibid., Band i. Hefte 5 and 6, p. 465.)

On Cretinism. By Professor His. (Ibid., Band ii. Hefte 1 and 2, p. 104.)

On the Antagonistic Effects of Opium and Sulphate of Quinia. By R. Nevison, M.D. (American Journal of Med. Sciences, July, 1861, p. 51.)

Remarkable Case of Muscular Atrophy, accompanied with Disease of the Spinal Cord. Clinical Observations and Comments by Drs. Gairdner, Adamson, Bell, Gull, and Day; Microscopic Examinations by Dr. Lockhart Clarke. (Beale's Archives of Medicine, No. ix. p. 1.)

Cases of Death in Epilepsy from Suffocation caused by Regurgitation of Food into Larynx. By J. Lalor, M.D. (Pamphlet.)

The Synthesis of Cataract. By B. W. Richardson, M.D.

Historical Sketch of the Madras Leper Haspital, and Notices of Leprosy as seen therein. By W. J. Van-Someren, M.D. (Madras Quarterly Journal, Oct. 1861, p. 271.

Case of Poisoning from Eating Common Honey. By G. Bidie, M.D. (Ib. p. 399.)

QUARTERLY REPORT ON SURGERY.

By John Chatto, Esq., M.R.C.S.E.

I. *On Excision at the Hip-joint.* By MM. Lefort and Gosselin. (Bulletin de l'Académie, tome xxvii. Nos. 2 and 3; and Gazette Hebdomadaire, Nos. 42, 46, and 49.)

M. Lefort, one of the rising French surgeons, who has paid more attention to what is going on in other countries than is usual with his countrymen, recently laid before the Académie de Médecine the results obtained by the English and German surgeons from the performance of the operation of excision at the hip-joint. The cases, the particulars of which he was enabled to collect, amount to 85 in number. Of these, the results are doubtful in 13 instances, in consequence of their too early publication; of the remaining 72 cases, 42 are returned as successful, while there were 29 deaths and 1 relapse, due to suppurative osteitis. The cases are considered as successful, provided that life was saved and the wound cicatrized; but the amount of motion possessed by the false joint, together with that of the solidity of the limb and power of using it in walking, are only specified in some cases. In 27 out of the 42 successful cases, at all events, these points are satisfactorily made out as highly favourable. In 67 cases the ages were specified, and it is found that between the ages of five and nineteen that there occurred 17 deaths in 49 cases; and between the ages of twenty and fifty, 7 deaths in 18 cases. In 30 of the cases the acetabulum, as well as the head of the femur, was excised; and in 3 of these the result is uncertain, recovery taking place in 15, and death in 11, of the cases.

M. Gosselin, the reporter on this essay, points out how desirable it would be to have more exact information as to the precise amount of unaided usefulness obtained in this last category, in order to be able to compare the functional condition of such limbs with that of those of patients who have recovered from hip-joint disease without operation.

The Reporter, in explanation of this operation not having as yet become adopted by French surgeons, suggests that either hip-joint disease is a less dangerous affection in France than in other countries, or that its earlier stages are more successfully treated in the former country than elsewhere. His observations apply, however, only to the young subjects met with in private practice, the treatment of which cases is often very successful; while that of the far more numerous cases of hospital patients is less encouraging. Then, again, French surgeons have been deterred by the contra-indications to the operation furnished by the concomitance of pulmonary tubercle, or other fatal complication in the severer forms of hip-joint disease, by the repugnance manifested by the friends of the patients, and by the ill-success which attends great operations in the Paris hospitals, chiefly due to the prevalence of purulent infection. Lastly, the difference in the practice of the French as compared with that of foreign surgeons, may be also due to the too great proneness of the latter to resort to the knife before essaying other means of treatment, while the former have remained in ignorance of the results really due to operative procedures executed elsewhere than in France.

A section of M. Lefort's paper is devoted to a consideration of the applicability of this operation after gun-shot wounds. Reasoning from analogy, both he and his Reporter admit that it may be preferable to disarticulation of the femur; but the comparative statistics of the two operations are in too imperfect a condition to determine which operation has proved least fatal. M. Larrey, speaking to this point, is unwilling to entirely discountenance excision in comminuted fracture from gun-shot wounds, but in truth he can scarcely indicate the case suitable for its employment; for while, on the one hand, in the case of moderate injury by a small projectile, it is far safer practice to remain contented with the removal of the shattered fragments of bone, so again, in the case of great devastating injury, resection would prove an insufficient procedure. With respect to the application of this operation in disease of the hip-joint, he agrees with the Reporter, basing his conclusion upon the numerous cases he has met with among young soldiers, that the disease is frequently curable, or its effects so much to be mitigated, as to render operations unadvisable. He believes that some of the English cases might have been otherwise treated with advantage, an opinion shared also by some English surgeons of great repute; and he thinks that the information supplied as to the power of using the limb in the cases of patients said to have recovered, is lamentably deficient. He also fears that resection of a diseased joint may become the immediate cause of the development of latent tubercular disease. Still he thinks that the operation should not be absolutely excluded, its performance being confined to the cases of adults debilitated by long-continued suppuration, in which other means proved of no avail, when diathesic or other contra-indications do not exist.

M. Malgaigne hit upon the truth of the matter when he stated that it is not so much the more favourable character of the results of the French mode of treating these cases, as it is the repugnance for the adoption of new operations—that is, although he did not say as much, when recommended *ab extra.* Thus, notwithstanding their success in other parts of Europe and America, resections of the knee are still prohibited in Paris, fatal though amputation of the thigh always proves there: and resection of the elbow has only become recognised after a desperate struggle. Not that routine has anything to do with this, adds M. Malgaigne, but an excess of prudence, which he assures us is a characteristic of French surgery—a fact which it certainly required so eminent an authority to vouch for. However, this paper of M. Lefort he regards as a step in advance towards the recognition of the possibility of resection of the hip being sometimes useful; but he insists upon the necessity of a greater exactitude in distinguishing the exact character and degree of the lesion it is adapted for. M. Velpeau declared that, so far from the subject of resection having been ignored in France, that is just the country in which its merits have been best and most effectually discussed—a statement that might well astonish his auditors. That it has not been there adopted arises from the fact of the French surgeons being better physicians, and having more confidence in other means of treatment, succeeding in curing cases which elsewhere would have been subjected to the knife. He does not believe that the disease so frequently proves fatal, as it is stated to do in England and Germany. Even in cases which resist the ordinary means, the operation would be a questionable procedure, seeing the common results of great operations in Paris. If, however, it is a fact that they succeed better in the English hospitals, this does not arise from the different regimen adapted and the better hygienic condition of these hospitals, but is due to the fact of the "English flesh" being gifted with a kind of physiological privilege which renders it more refractory to the accidents following operations!

Two good results have indirectly sprung from this discussion, the one being an exposition of the extremely defective condition of Hospital Statistics, rendering the comparison of results of different procedures absolutely impossible. The other is the comparison of the condition of the French and English hospitals. This interesting topic is still under the consideration of the Academy, so that we must defer noticing the statements brought forward until a subsequent number.

II. *On a Novel and Efficient Method of Reduction in Dislocation of the Shoulder-Joint.* By Professor N. R. Smith. (American Journal of Medical Science, July. pp. 17–25.)

After stating that the real difficulty in reducing dislocation of the shoulder is the production of immobility of the scapula, and that this is insufficiently provided for by several of the means in use, Dr. Smith describes the procedure which he has himself had recourse to for many years past.

"In effecting counter-extension, it is undoubtedly in most instances expedient to apply our resisting bands as directly as possible to the bone from which the other is dislocated. But the difficulty in this case is to effect it without defeating the object in the manner indicated above. On noticing the mechanical relations of the scapulæ with their apparatus of muscles to each other, it occurred to me to make counter-extension from the opposite wrist. Anteriorly the two acromion processes are bound together by an unyielding chain of bone and ligament. The two clavicles, the sternum, and the intra-clavicular ligament, chiefly constitute this band of union. The fibrous, resistant ligaments in this chain are not capable of being stretched; and if traction be made from opposite wrists, the two acromion processes, thus tied together, are not capable of being drawn asunder to the extent of half an inch. Posteriorly, the continuity of resisting parts is almost as perfect. . . . . The object of counter-extension is to prevent the yielding of the scapula to the tractive force. Nothing does this so effectually as the *fixing of the opposite scapula* by counter-extension at the wrist. Not only is the scapula thus sustained, but the spine is erected and prevented from yielding to the tractive force, and becoming carried to the injured side, as invariably happens when counter-extension is chiefly made from the axilla, as usual. . . . . In some of the cases in which I first employed this method, I directed simply traction from the two wrists, and I am not now confident that this is not the best method. I placed the patient in a chair, and directed two strong persons to make steady horizontal traction from the two wrists. As soon as the spasmodic resistance of the muscles was overcome, the head of the bone was disengaged, and the muscles which help us in such cases suddenly lifted the head into its place. It will be observed that this means causes no appreciable pain, but rather relieves the suffering of the patient caused by the pressure of the head of the humerus. In cases in which, from unusual muscular development, or the age of the dislocation, much resistance is expected, I have modified the application of counter-extension thus: I place the patient on a chair, sitting a little on one side of it, so as to allow room on the side of the injury for the operator's foot. I then pass a piece of stout muslin, folded, around the chest and under the axilla of the injured side. The tails of this I carry horizontally to the opposite side—one in front, the other behind—and extending the arm horizontally, bandage them firmly to the wrist on the sound side, leaving the ends projecting, to be well secured to the wall, or other unyielding substance. I then pass an ordinary roller over the top of the injured shoulder, and back and forth twice under the muslin band, to prevent its slipping down. Then I continue the same roller under the bottom of the chair and over the shoulder three or four times. This helps to give steadiness to the scapula, and especially prevents the involuntary rising of the patient, or the tilting of the scapula upwards, when it is necessary to make the manipulation of which I am about to speak. I first apply a wet roller to the wrist of the injured side, and then attach a muslin extending band by the clove hitch. Next, I direct extension to be made by two persons, at first outwards and a little downwards, gradually raising the arms to the horizontal direction, and finally a little above it. The extension must be made gently and steadily, gradually increasing the force so as to not provoke the muscles to spasmodic resistance. As no pain is produced by the force thus employed, it may be continued for a considerable time. The muscles, which at first resist, become fatigued, and finally relaxed; and in a large majority of instances of recent luxation, the head will slip into place without resort to any manipulation. I would even continue this traction, where much resistance is encountered, for a quarter of an hour before modifying the force; but in case the object is not then effected, let the surgeon place his foot on the margin of the chair and his knee in the axilla. Then let the assistants raise their line of traction above the horizontal as much as possible, and continue it for a moment. The surgeon should then direct that the arm be, by a sudden movement, carried downwards, while, by extending his foot, he elevates the knee in the axilla. He aids the assistants in this by grasping the arm near the elbow and using it as a lever. If the first effort be not successful, he should repeat it. Sometimes I place on the knee a ball made by rolling up a bandage, but it is not important. The surgeon can thrust his knee into the axilla, so as to avoid much pressure on its marginal muscles. I generally direct those who make the traction to sway the limb horizontally backward and forward, and, grasping it with my hands, at times I rotate it a little on its axis, thereby contributing to the disengagement of the head of the bone. . . . . The supine position of the patient on a table is a very convenient arrangement in all dislocations of the shoulder, and is well calculated to promote the efficiency of the method which I recommend. I am not sure that it is not the very best, especially when chloroform is employed. . . . . I will here state that I have by this mode repeatedly reduced dislocation of the shoulder which was two months old, and once when three months had elapsed. In cases in which I expect great resistance I commonly employ chloroform. The effect of this agent must, however, be rendered very complete, for its incomplete use causes spastic rigidity of the muscles and defeats the object."

III. *Observations on Strangulated Hernia.* By M. Verneuil. (Bulletin de Thérapeutique, tome lx. pp. 249, 399, and 450.)

In this paper M. Verneuil furnishes the results of his own experience in the treatment of strangulated hernia. This has been as yet but limited, but viewed by the light of the large field of observation of that of other surgeons which he has long enjoyed, it is of interest. With respect to *femoral hernia*, he lays down the following principles: 1. Irreducibility with symptoms of obstruction, is almost always due to true strangulation. 2. The taxis and other modes of reduction almost always prove insufficient. 3. When reduction is thus exceptionally produced, a cure is far from being certain; and the patients still incur almost as much danger as they would have done had they been operated upon without the prolonged employment of the taxis. 4. Early operations, without the preliminary administration of purgatives, and preceded only by a very moderate application of the taxis, offer very good chances of recovery, when the strangulation is not old. 5. When the exposed intestine is found to have undergone great change, even independently of gangrene and perforation, if peritonitis is already very evident, an artificial anus should be at once formed, as giving a much greater chance of the preservation of life. 6. If the vitality of the intestine is doubtful, and *à fortiori* if a perforation, however minute, whether caused by the surgeon or not, exist, the gut must not be returned. Although sutures have sometimes succeeded, their employment is perilous. 7. It cannot be too often repeated that the methodical establishment of an artificial anus is a valuable resource not sufficiently employed. M. Verneuil has operated upon six cases of femoral hernia; in three with rapid success. In another, recovery took place with an artificial anus; and probably the two cases which proved fatal might have been saved by the same means. In inguinal hernia he has been unfortunate, for all the four cases upon which he has operated have died. But while he has never succeeded in returning a femoral hernia by means of the taxis, he has frequently replaced inguinal hernia by this means. He disapproves of the employment of forced taxis. He relates some interesting cases of complicated inguinal hernia, but to these we can only refer.

IV. *On Percutaneous Ligature of Arteries.* By Professor MIDDELDORPFF. (Abhandlungen der Schlesischen Gesellschaft, 1861, Heft iii.)

This procedure consists in passing a curved needle through the skin at about half or a third of an inch from the artery intended to be tied, carrying it beneath this, and bringing it through the skin again at the same distance on the opposite side. The silk employed for the ligature should be firm, but not too thin, lest it tear or injuriously compress the soft parts surrounded with the artery; silver or iron wire ligatures may also be used. The ligature must be only tightened sufficiently to arrest the bleeding or pulsation, as the case may be. When the peripheric end of a wounded artery continues to bleed, this must also be surrounded by a ligature. The arteries best suited for this procedure are those of the scalp and face, the *arcus plantaris et volaris*, the radial, ulnar, and the tibial in the middle of the leg. It is well adapted, too, for arteries which have undergone dilatation, as in anastomotic aneurism of the scalp. Its advantages are the rapidity and facility of its execution, the slightness of the wound, danger, and pain, the simple after treatment required, the possibility of employing it in cases difficult of treatment (as in aneurism by anastomosis, &c.), and the fact that the ligature may be passed around the artery very near the point of injury. As to any ill consequences which might have been apprehended from including veins and nerves in the ligature, these (nor, indeed, have any others) have not occurred, a fact which may be due to the bulkiness of the ligature employed. Since 1856, when he first commenced this practice, the author has put it into force only in ten instances.

V. *On the Return of Pulsations in Aneurysms after the Ligature.* By M. BROCA. (Gazette des Hôpitaux, 1861, Nos. 14 and 17.)

During a discussion on aneurysm at the Society of Surgery, M. Broca observed that the return of pulsation may depend upon three different causes—the persistence of the pulsations, return of the pulsations, and relapse of the aneurysm. In the first of these cases it is due to the presence of a somewhat large collateral vessel very near the aneurysmal sac. The pulsation soon appears, but the cure of the disease is not prevented. In the second case, when the pulsations return, the sac is suddenly filled with coagula which have not had time to become organized, and these "passive" coagula are gradually dissolved, the arterial pulsations then reappearing. They appear soon after the operation, and may persist for one, two, three months or more. At last, these first coagula are replaced by "active" coagula, and a cure is produced in almost all cases. When relapse is to happen, the pulsations do not reappear so soon as in the preceding case, not occurring until one, two, or six months, or even from one to three years, after the ligature. At one of these remote periods the blood has made a passage into the sac, and having hollowed out a more or less spacious cavity the tumour takes on again the characters of an aneurysm. Under all the above circumstances, more or less circumscribed pulsations are perceived in an aneurysm for which an operation has been performed, but the prognosis is very different. In the two first cases no surgical treatment is called for, while that of relapse is a very serious one.

M. Broca gave an account of the various cases he had collected exemplifying these occurrences. These do not comprise mere persistence of the pulsations after the application of the ligature, as owing to the circulation being rendered slower the deposition of fibrine goes on gradually and regularly until a cure is accomplished. They relate only to the cases in which the aneurysmal tumour, after having been filled with coagula, at the end of a certain time again becomes permeable; and these, as stated above, may consist of instances of *return of pulsation* and *relapse properly so called.* Cases of return of the pulsation are distinguished from those of relapse by the fact that the tumour makes no progress; its pulsations, always more feeble than before the ligature, acquiring their maximum of intensity in a few hours, and then diminishing in force. Of 26 cases collected from various sources by M. Broca, 13 were aneurysms treated by ligature of the femoral, 4 of the external iliac, 6 of the primary carotid, 1 of the subclavian, and 2 of the radial. In 5 of the cases the pulsations reappeared before the end of the first day, eight times from the third to the seventh day, four times from the eighth to the fifteenth day, once on the nineteenth day, and once at the end of the seventh week, in this case only continuing during one day. None of these patients became the subjects of gangrene. One died from phlebitis, the ligature having perforated the femoral vein, and another in consequence of secondary hæmorrhage from the wound made for the ligature. All the other 24 cases survived, and all were radically cured with the exception of two, which went on to true relapse. Of *relapse,* properly so called, he has collected 22 examples, this manifesting itself in 7 towards the end of the first month, and in 13 after the end of the sixth month, the epoch not being exactly indicated in the two others. It is remarkable that in none of the cases completely known has the relapse occurred between the first and the sixth month. All the relapses occurring after the end of the sixth month, related to aneurysms of the lower extremity. Six relapses occurred between six months and a year, 2 at the end of a year, 1 at the end of two years, 1 at the end of four years, 1 at the end of 7 years, and one at the end of fifteen years. This class of cases is far more serious than the former; for although in three instances the aneurysms were cured spontaneously or by means of slight treatment, and in two others they remained stationary for a long time, in all the rest their progress was much more alarming.

VI. *On the Extraction of Articular Foreign Bodies from the Knee-joint.* By M. H. Larrey. (Gazette des Hôpitaux, No. 67.)

As the result of his bibliographical researches, M. Larrey has collected references to 167 published cases in which operations have been performed on the knee-joint for the extraction of articular foreign bodies. In 129 of these cases the extraction was performed by the old or direct method, and in 38 by the indirect or subcutaneous method. The results are given for 121 of the former cases, being successful in 98, doubtful in 5, and fatal in 28; while of the 38 indirect operations, 19 were successful, 15 failed, and 5 were fatal. From these facts and from others communicated to him orally by his colleagues, the author deduces the following conclusions:—1. The operation of extracting these foreign bodies from the knee-joint is a serious one when practised by direct incision, and a difficult one by the subcutaneous method. Many accidents have occurred even amongst the somewhat numerous cases in which recovery has taken place. 2. Extraction is proportionally more dangerous than is the affection itself; that is, that more redoubtable accidents may be induced by the operation than by the presence of the foreign body. 3. While many cases of recovery may be verified, many instances of failure or of fatal result have doubtless never been published. 4. The operation definitively would seem to be indicated only under the following circumstances: the certain and complete mobility of the foreign body within the joint; the persistence of the accidents caused by its presence—viz., pain, arthritis or hydarthrosis, and lameness; the failure of compression and other means for fixing the position of the body; and the free consent of the patient, although made aware of the dangers and difficulties of the operation.

VII. *On Dressing Wounds by Occlusion.* By M. CHASSAIGNAC. (Presse Médicale Belge, No. 30.)

M. Chassaignac here again insists upon the great advantage of this procedure, followed by him since 1839. It consists in covering a recent wound, whether complicated with fracture or not, with a cuirass of adhesive plaster. The strips must be crossed in order to give solidity, and imbricated so as to prevent the mischief which often occurs when they are separated by spaces. They must never be applied circularly lest they cause strangulation, and they should always extend to a considerable distance beyond the limits of the lesion. The plaster must be chosen of a good quality, not becoming detached when the two surfaces of the strapping are pressed together. Over this cuirass is placed a fenestrated cloth well spread with cerate, and then charpie, compresses, &c., after the usual French fashion. The discharge issues from between the strips and at the edge of the cuirass, the dressing external to this being renewed as often as necessary. The converting a wound thus from an open one into one analogous to a subcutaneous wound, is the best security against the occurrence of inflammation; but in cases in which the violence has been considerable, and the subsequent inflammation is great, leeches should be put on at a distance from the wound, on the track of the lymphatic vessels, and refrigerants, made with ice mixtures, applied through the cuirass, while the position of the wounded part should be raised. The dressing need not be meddled with for eight or ten days; but if the patient suffers and there is suppuration, all but the cuirass may be taken off. This also must be removed, if on exploration some complication is feared; otherwise its surface must be washed over with water containing some drops of camphorated spirit or lemon-juice. If the cuirass seems to be giving way, it should be strengthened by some supplementary strips. When we wish to remove the cuirass, it must be carefully divided by a scissors or a grooved director. The surface of the wound is to be washed, and touched with a solution of nitrate of silver (five parts to thirty), and the dressing is then to be re-applied. This solution lessens the pathological vascularization of the wounded surface, diminishes the amount of purulent and plastic exudations, and facilitates the adhesion of the epidermis to the dermis. The want of such adesion is the cause of a great number of the complications of wounds, and especially of erysipelas and reticular angioleucitis. It is in this way may be explained the beneficial action of this solution in many exanthemata.

For the following papers we have not space for more than reference:

Angiectasis.—Krause on Traumatic Angiectasis of the Arm. (Langenbeck's Archiv, vol. ii. part i.)

Bladder.—Mercier on Inflammatory Hæmorrhage of the Bladder. (Gaz. des Hôpitaux, No. 142.)

Cataract.—Mirault on Capsular Cataract. (Bulletin de l'Acad. de Méd., vol. xxvii. p. 281.) Saemisch on the Operations for Cataract. (Wurzburger Med. Zeitschrift, vol. ii. p. 272.) Macnamara on Extraction in Cataract. (Indian Annals, No. 14.)

Dislocation.—Bartmer on Dislocation of the Femur. (Langenbeck's Archiv, vol. ii. No. 1.) Goyraud on Dislocation of the Intra-articular Fibro-Cartilage of the Wrist. (Gaz. des Hôpitaux, Nos. 126 and 129. Streubel on Injuries to the Fore-arm in Young Children. (Prag. Vierteljahrs., 1861, No. 2.)

Ear.—Voltolini's Pathological Examinations of Diseased Ears. (Virchow's Archiv, vol. xxii. No. 1.) Bonnafont on an Instrument for Injecting Gaseous Substances into the Internal Ear. (L'Union Médicale, No. 142.)

Fistula in Ano.—Verneuil on a Fatal Case of Fistula operated upon by the Ecraseur. (Gaz. des Hôp., No. 131.)

Fore-arm.—Streubel on Injuries of the Fore-arm in Young Children. Prag. Vierteljahrs., 1861, No. 2.)

Fungus.—Carter on a New Form of Fungus of the Foot. (Trans. of Bombay Med. Soc., N.S., No. 6.)

Hernia.—Frickhoffer on Statistics of Strangulated Hernia in Nassau. (Med. Jahrbuch für das Herzog, Nassau, No. 17.) Von Wahl on Statistics of Strangulated Hernia and of the Operation. (Prag. Vierteljahrs., 1861, No. 3.) Debout on the Treatment of Congenital Umbilical Hernia. (Bulletin de Thérapeutique, vol. lxi. Nos. 9 and 10.) Huguier on the Treatment of Umbilical Hernia. (Gaz. des Hôpitaux, No. 137.) Fano on a Rare Form of Inguinal Hernia. (L'Union Médicale, No. 145.)

Knee-joint, Fock on Loose Bodies in the. (Langenbeck's Archiv, vol. ii. No. 1.)

Orbicularis Palpebrarum, Nivery on Spasmodic Contraction of the. (Bulletin de Thérapeutique, vol. lxi. No. 8.)

Polypus.—Paul on Polypi of the Rectum in Children. (Abhandl. der Schlesischen Gesellschaft., 1861, No. 3.)

Ranula.—Pauli on Pathology and Treatment of Ranula. (Langenbeck's Archiv, vol. ii. No. 1.)

Skull.—Meyer's Cases of Recovery after Injury to the Skull. (Langenbeck's Archiv, vol. ii. No. 1.) Friedberg on Indirect Fracture of the Skull. (Prag. Vierteljahrs., 1861, No. 3.) Moore on Fracture of the Skull, with Injury to Brain. (Indian Annals, No. 14.) Friedberg's Clinical and Forensic Relations of Injuries to the Head. (Virchow's Archiv, vol. xxii. No. 1.) Wills on Gun-shot Wound of Skull, with Loss of Brain. (Amer. Med. Times, No. 15.)

Syphilis.—Scarenzio on the Treatment of the Primary Syphilitic Ulcer by Caustic. (Omodei's Annali, vol. clxxvii. p. 115.)

Testis.—Demme on Tubercle of the Testis. (Virchow's Archiv, vol. xxii. No. 1.) Santopadre on Necessity of Exploration in Tumours of the Testis. (Annali Amodei, vol. clxxvii. p. 513.) Zeis on the Abnormal Descent of the Testis. (Langenbeck's Archiv, vol. ii. No. 1.) Rollet on Epididymitis in Undescended Testis. (Gaz. des Hôp., No. 141.)

Tetanus.—Thamhaya's Report on Recent Investigations on Tetanus. (Schmidt's Jahrb., vol. cxii. p. 210.

Tracheotomy.—Maisonneuve on a New Mode of performing Tracheotomy. (L'Union Méd., No. 147.)

Uranoplastics, Langenbeck on (in his Archiv, vol. ii. No. 1.)

Urethra.—Roser on Contraction of the Orifice of the Urethra. (Archiv der Heilkunde, vol. ii. p. 309.)

QUARTERLY REPORT ON MIDWIFERY.

By Robert Barnes, M.D., F.R.C.P.
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I. The Unimpregnated Condition.

1. *Vesico-vaginal Fistula; Spontaneous Relief.* By B. E. Cotting, M.D. (Boston Med. and Surg. Journal, July, 1861.)
2. *Fatal Peritonitis after the Operation for Closing a Vesico-vaginal Fistula.* By Dr. Alex. Simpson. (Edinb. Med. Journ., Nov. 1861.)
3. *On Galvano-caustic in Gynæcology.* By Dr. O. v. Grünevaldt. (Peters. Med. Zeitschr. ii. Heft. 1861.)
4. *The Angulations and Curvatures of the Non-Pregnant Uterus.* By Dr. F. H. Arneth. (Petersburger Medic. Zeitschr., Heft v. 1861.)

1. Dr. Cotting relates a case in which, in consequence of a labour protracted during five days, a vesico-vaginal fistula, large enough to admit the tips of two fingers, was formed. The patient refused to submit to any operation; she suffered from incontinence of urine for three years; after this, power to retain urine gradually returned, and eventually she was quite cured. Deep corrugations and contractions were formed at the place of the fissure.

2. Dr. Alexander Simpson showed to the Edinburgh Obstetrical Society a preparation of the pelvis and pelvic organs of a woman who had died fourteen days after being subjected to the operation for the cure of a vesico-vaginal fistula. Four iron stitches were used. On the tenth day symptoms of peritonitis set in. The recto-vaginal and vesico-uterine reflections of the peritoneum were comparatively free, but the ovaries were thickly coated with fibrin. There was no inflammatory action near the fistula.

3. Dr. Grünevaldt contributes a systematic memoir on the applications of the galvanic cautery to the surgery of the female organs. He relates two cases in which he successfully employed this means for the removal of uterine fibrous polypi. A platinum wire is passed in a loop around the stalk of the tumour; then, being heated to a white heat by the galvanic battery, the part included is rapidly cut through. In the cases described, no bleeding or pain was occasioned. He then discusses the application of this method to the extirpation of the entire uterus (an operation which ought, with our present knowledge of the method of re-position by the caoutchouc dilator recommended by Dr. Tyler Smith, and practised successfully both by himself and Dr. Charles West, to be entered upon after great hesitation). Grünevaldt does not relate a case of extirpation of the whole uterus, but gives one in which the anterior lip of the cervix was thus removed with success. He next refers to the simple galvanic cautery for the relief of ulcerations of cancerous or other nature. He proposes to amputate the breast by this method.

4. Dr. Arneth, in his paper on angulations and flexions of the non-pregnant uterus, enters upon a general history of these affections. He relates one case in which Simpson's intra-uterine pessary was used, and was abandoned on account of an accident which deserves to be recorded. The patient had worn the instrument for some days without much inconvenience, when it slipped out of the uterus; the patient, as she thought, replaced it herself. On being examined by Arneth, it was found that the stem had been fixed in the vaginal cul-de-sac behind the cervix uteri. Dreading that a repetition of this accident might lead to perforation of the roof of the vagina, the instrument was not re-applied.

II. Labour.

1. *Remarks on the Removal of the Placenta.* By Dr. Abegg. (Monatsschr. f. Geburtsk., October, 1861.)
2. *Employment of Electricity to Expel a Placenta.* By Dr. Kuhn. (L'Union Méd., October, 1861.)
3. *On the Employment of Electricity in Midwifery.* By Dr. A. Baer. (Mon. f. Geb., October, 1861.)
4. *Uterine Hæmorrhage, Ante-partum and Post-partum.* By Dr. G. Hamilton. (Edinb. Med. Journ., October, 1861.)
5. *On Placenta Prævia; its Nature and Treatment.* By Dr. Sirelius, of Helsingförs. (Arch- Gén. de Méd., September, October, 1861.)
6. *Twin-birth, with Placenta Prævia.* By Dr. Schuhardt. (Mon. f. Geb., October, 1861.)
7. *Rupture of the Uterus during Labour.* (*Recovery.*) By Dr. J. H. Warren. (Boston Med. and Surg. Journ., March, 1861.)
8. *A Case of Symphyseotomy.* By Dr. Faucoult. (Arch. Gén., November, 1861; and Mon. f. Geb., October, 1861.)
9. *Study on the Statistics of the Cæsarean Operation.* By M. Pihan-Dufeillay. (Arch. Gén. de Méd., August, September, 1861.)
10. *On the Use of the Forceps in Tedious Labour.* By Dr. G. Hamilton. (Edinb. Med. Journ., October, 1861.)
11. *Case of Puerperal Eclampsia in connexion with a Fungus of the Dura Mater.* By Dr. Kehrer. (Monatsschr. f. Geb., September, 1861.)
12. *Case of Puerperal Convulsions and Mania unconnected with Albuminuria.* By Dr. Main. (Edinb. Med. Journ., November, 1861.)
13. *On the Treatment of Puerperal Fever.* By Dr. Kehrer. (Mon. f. Geb., September, 1861.)
14. *Observations contributory to the History of the Relations between Puerperal Fever and Epidemic Erysipelas.* By M. Pihan-Dufeillay. (L'Union Méd., August, 1861.)

1. Dr. Abegg concludes an historical and critical memoir on the removal of the placenta by advocating the "English method," which consists in maintaining pressure upon the fundus of the uterus from the moment of the child's delivery, and following the contracting uterus downwards into the cavity of the pelvis. With a view to the prevention of puerperal fever, he always gives secale to multiparæ the day after labour.

2. Dr. Kuhn relates a case in which the placenta was retained in utero half-an-hour, the cord having broken off. The os uteri had closed, and several doses of ergot only brought on a kind of continuous trembling of the uterus. At the end of eighteen hours one of the poles of H. Legendre's battery was applied to the fundus uteri; the other, by means of a female catheter, inside the os. Scarcely was the circuit completed, when a powerful contraction was produced in the uterus, and the placenta was immediately expelled. In a discussion upon this case, M. Béraud objected that the contraction was caused by the titillation of the os uteri, alleging that electricity has little effect upon the muscles of organic life. Numerous previous titillations, however, had failed to evoke the contractile energy of the uterus; and the fallacy that electricity has no influence upon organic muscles has been completely exposed by the observations of Drs. Radford, Barnes, and Mackenzie.

3. Dr. A. Baer has published an elaborate memoir on the use of electricity in midwifery. Considering first the application of this agent to the induction of premature labour, he cites the experiences of Frank, Jacobs, Höniger, Harting, Schreiber, Hennig, Radford, Barnes, Mackenzie, Houghton, Dorrington, Dem-

sey, Cleveland, in affirmation of its power to initiate uterine action. In a second part he discusses the uses of the agent in defective uterine action and in hæmorrhage. In a third part he examines its uses in the asphyxia of new-born children. He says, one of the most frequent causes of this asphyxia is an accidental impediment to the capacity of breathing. The common means for exciting respiration are uncertain. Boër, in 1791, bore strong testimony to the efficacy of electricity. Froriep, in 1801, did the same. Others have confirmed by fresh experiments the conclusions of Boër and Froriep. Scholz, in 1851, warmly advocated the method. The author says the surest way to excite respiration is to stimulate the phrenic nerve, for this causes a contraction of the diaphragm, and hence a deep inspiration. This had been pointed out by Soemmering, but Ziemssen (1857) first by exact experiments showed the spot where the phrenic nerve was to be touched. The nerve, he says, lies on the outer edge of the sterno-cleido-mastoid muscle.

The author observes that from Duchenne's experiments, as well as from his own, it appears that Remak's fear lest tetanization of the two phrenic nerves be excited is groundless. Ziemssen recommends the application of Faraday's current by means of large sponges placed so as to affect not only the phrenic nerves, but also all the nerves proceeding from the cervical and brachial plexuses to the respiratory muscles, so as to bring about the fullest possible expansion of the thorax, and therewith the inhalation of a corresponding quantity of air.

4. Dr. G. Hamilton describes a vaginal plug which he has used with success in cases of uterine hæmorrhage from cancer, or connected with gestation. It consists of an india-rubber ball sufficiently strong to re-expand after introduction into the vagina; connected with this ball is a brass tube, 3" long, to supply the air necessary for expansion. It is maintained *in situ*, against the os uteri, by tapes attached outside.

In post-partum hæmorrhage, Dr. Hamilton speaks strongly in favour of the following method of compressing the uterus: The fingers of the right hand are passed *under* the uterus, which the relaxed state of the parts generally allows of being easily done; then, with the other hand upon the uterus externally, the organ is firmly compressed between the two hands. The hand should only be inserted inside the uterus for the purpose of clearing out clots, &c.

5. The memoir of Dr. Sirelius consists of extracts from a larger memoir published in Norwegian in 1861. He describes with some minuteness the histology and structure of the placenta, following generally the views of Robin. An elaborate portion, devoted to the pathology and treatment of placenta prævia, is mainly a reconstruction of the facts and reasonings contained in Dr. Barnes's Lettsomian lectures on the subject, published in 1857. Dr. Sirelius relates confirmatory cases. He however argues in favour of Cohen's method of partially detaching the placenta from the neighbourhood of the cervix. Although it forms an useful essay on placenta prævia, the paper contains little that is original.

6. Dr. Schuhardt records a case of twins with placental presentation. The placenta of the first child was centrally placed over the os. This child born, the membranes of the second ovum were pierced. The second placenta was attached to the fundus. In both the umbilical cord sprang from the centre. Both children were born alive, but being premature, did not survive. Dr. Schuhardt says this complication is so rare, that he has only met with four recorded examples—namely, one by Professor Niemeyer, of Halle, in 1831; one by Ricker, 1853; one by Professor Trefurt, 1844; and one by H. Spondli, in 1854. (Another case may be found in the Reporter's work on Placenta Prævia, 1858.)

7. Dr. J. H. Warren relates an interesting case of rupture of the uterus during labour, ending in recovery. The patient, an Irishwoman, aged forty, had been delivered by craniotomy thirteen months previously. The pelvis was contracted in the conjugate diameter to 2½". The present labour had been attended with very severe pains, the liquor amnii escaping early. On the access of a very violent pain, accompanied with vomiting, she said she felt something "give way." The pains diminished, but the vomiting continued. Delivery was effected by turning and craniotomy. After delivery there were prostration and vomiting of dark coffee-ground matter. The hand introduced into the uterus, detected a rent sufficient to allow it to pass through. The rupture was in the anterior portion of the cervix and os. Morphia was given. Metritis and fever, with vomiting, continued for three or four days. At the end of the month she had recovered.

8. A case of symphyseotomy calls for record on account of the rarity of the operation. It is related by Dr. Faucoult. A primipara, aged twenty-four, rhachitic, was in labour at term. Pains had lasted several days; breech presented. The feet were brought down, but the head remained fixed; section of the symphysis pubis was determined upon. After separating the pubic bones to the extent of four centimetres, the passage of the head was permitted. The child was dead, and the bladder was perforated. Two months afterwards the patient was able to resume her usual occupations.

9. M. Pihan-Dufeillay contributes an excellent analytical essay on the 'Statistics of the Cæsarean Operation.' He insists that the method so much followed of simply comparing the deaths with the recoveries after the operation must lead to false conclusions. In many cases the deaths can in no way be ascribed to the operation, but to antecedent conditions, which may in many instances be averted. The principal causes of death directly connected with the operation are—hæmorrhage, nervous accidents, as shock, and peritonitis. Even these may be diminished by care, especially peritonitis, by withdrawing the patients from hospital influence. The author gives a table of 88 cases, all he has found recorded since 1845. Of these 50 ended in recovery. Of the remaining 38, the causes of death were, in 6 some antecedent disease, as capillary bronchitis, convulsions, putrid poisoning, the result of detention of a putrid fœtus in the uterus; 2 are stated to have died of puerperal fever, the operations being performed in hospitals. In one of these cases, occurring at Vienna, the Cæsarean section was actually decided upon with the single hope of saving the child, it being considered inevitable that under any treatment the mother would die of the hospital epidemic. Thirty cases remain, amongst which the influences of manœuvres unsuccessfully tried to deliver by the natural passages must be sought. The information concerning this influence is not precise enough to justify an estimate. The author, however, constructs a comparative table, which exhibits the influence of the duration of labour preceding operation over the result. Of 29 successful cases, the powers were preserved in 24; in 20 the duration of labour was under twenty-four hours. Of 19 fatal cases, the powers were failing or gone in 15, and in 11 cases the labour had lasted more than twenty-four hours.

M. Pihan-Dufeillay concludes that the Cæsarean section, performed under favourable conditions, gives nearly 75 per cent. of recoveries. The lesson drawn from his review is, to operate as soon as the diagnosis is clear, and the impossibility of delivery is recognised.

10. Dr. Hamilton advocates a far more frequent resort to the forceps in tedious labour than is customary. He says, that since he has used the instrument in between every seventh and eighth case, 731 children have been

delivered successively, not one of which was still-born. In explaining this remarkable statement, Dr. Hamilton states that this series excludes all cases which were not under his care from the commencement, and all children that were not viable, or were dead when he took charge of the cases. Taking this advantage for himself, however, it is scarcely fair to contrast, as he does, his 731 cases with the alleged loss of "1 child in 20 or 30" under ordinary management. Similar deductions must obviously be made on both sides. Dr. Hamilton says, when the os uteri has become dilated, and the head has entered the pelvis, so that an ear can be easily felt, that the danger to the child actually becomes imminent if it be allowed to remain undelivered much more than two hours. Hence the indication for resort to the forceps is derived. Dr. Hamilton argues, that a low rate of infant mortality involves a low rate of maternal mortality. Of his 731 cases, 6 mothers died; but in only 3 of these was the forceps used.

11. A case of puerperal convulsions associated with a fungus of the dura mater is related by Dr. Kehrer. A healthy-looking person, aged twenty-nine, was admitted into the Giessen Lying-in Hospital. She had often complained of pains in the head during her pregnancy. There was no sign of paralysis, but both eyes were affected with strabismus. She was delivered without accident. On the third day symptoms of cerebral disturbance set in. Shivering was followed by general convulsions; the head, neck, and right limbs were chiefly affected. The face was congested, the lips livid; pulse 140. No urine in bladder; no œdema. The convulsions continued, coma in the intervals; pupils contracted. Death.

Autopsy.—The vessels of the brain were much injected. Directly under the optic chiasma, between the anterior cerebral lobes, was a round, firm, pale-red tumour, the size of a walnut. It arose from a small base from the middle of the sella turcica, where it was fast adherent to the dura mater, but not to the sphenoïd bone; it spread forwards and to the right over the lesser sphenoïd, alæ, and the orbital plate of the frontal bone.

12. Dr. Main relates a case of puerperal convulsions and mania occurring in a primipara, no albumen being detected in the urine. When eight months pregnant, after having been thrown from a cart, slight sanguineous discharge and smart pains set in. Soon after a convulsive fit occurred, then two others, and complete unconsciousness. At this time there was no albumen. The fits and coma continued notwithstanding leeches, croton oil, and morphia. Chloroform had the effect of arresting the attacks. Labour came on, and a still-born female child was delivered spontaneously. Urine taken after delivery was also free from albumen. Mania lasted two days, but recovery was ultimately complete.

13. Dr. Kehrer, after discussing the various remedies used in the treatment of puerperal fever, says that quinine has been tried as a prophylactic in a lying-in hospital under his observation. Most of the patients took from a scruple to a drachm before labour; but it neither prevented the attack, nor modified the severity of the disease. He speaks well of the system of disinfecting the wards by means of Wilson's "aqua regia."

14. M. Pihan-Dufeillay contributes an excellent essay in illustration of the connexion between puerperal fever and epidemic erysipelas. He adds to the general mass of facts already recorded, three cases observed by himself. In a particular ward puerperal fever had recently prevailed. Three women pregnant were admitted. Erysipelas attacked them and several of the attendants.

III. PATHOLOGY OF THE OVUM AND FŒTUS.

1. *Report of the Midwives' Institution of Stettin*, 1834—1859. (A curious example of injury to the fœtus in utero.) By Dr. BEHM. (Monatsschr. f. Geburtsk., Aug. 1861.)
2. *A Case of Hydatid-moles with a Second Normally-developed Ovum.* By Dr. HILDEBRANDT. (Mon. f. Geb., Sept. 1861.)
3. *On the Influence of the Mother's Imagination on the Fœtus.* By M. MARTIN and Dr. VAN EULEN. (L'Union Méd., Sept. 1861.)
4. *A Case of Hydrorachis successfully treated by Iodine Injections.* By M. GOSSELIN. (L'Union Méd., Sept. 1861.)
5. *Two New Cases of Congenital (Simple) Cystic Hygroma of the Sacro-perineal Region.* By Dr. H. STRASSMANN. (Mon. f. Geb., Aug. 1861.)
6. *On Torsion of the Umbilical Cord and Consequent Stenosis of the Vessels.* By Dr. DOHRN. (Mon. f. Geb., Aug. 1861.)

1. Dr. Behm relates a curious accident which happened to a pregnant woman in the Stettin Lying-in Hospital. The patient was in hospital expecting delivery. When busy sewing, she reached over the table, and suddenly cried out that she was pierced with a needle. The epigastrium immediately examined, a needle was seen deeply penetrating the walls of the abdomen, the eye, and half an inch of it, still projecting. Before the patient could seize it, it was wholly drawn within the abdomen. This so quickly happened that her tale was not believed by the midwife. She was delivered fourteen days later, and as soon as the child was born the needle was discovered sticking in its left knee. It was a long, strong needle, and penetrated so deeply in the bone that it could only be extracted with forceps. Dr. Behm says, that when the accident occurred, the point of the needle must have pierced the uterus and become fixed in the child's leg, and that reflex action being thus excited, the retraction of the limb in utero pulled the needle through the abdominal walls into the uterus.

2. Dr. Hildebraudt describes a case in which there was a twin-pregnancy, one ovum being normally developed, the fœtus being dropsical, whilst the neighbouring ovum had at an early period degenerated into an hydatid mole, with loss of the fœtus. The conception had been preceded by a severe typhus, which left the patient very weak. The fœtus was very anæmic, and weighed two pounds.

3. In a discussion at the Société Médico-Pratique of Paris, suggested by a memoir communicated by Dr. Van Eulen, M. Martin reviewed the question of the influence of the mother's imagination upon the fœtus. His conclusion is in negation of this influence. Reference is made to this discussion, since it embraces a very able argumentation of the subject.

4. M. Gosselin relates a case in which Dr. Sézérie successfully injected a solution of iodine in a case of spina bifida. The child was nine and a half months old, and affected with paraplegia and incomplete paralysis of the upper extremities. The tumour was the size of an orange, and seated at the lower lumbar region. It had opened twice spontaneously. A puncture made with a hydrocele trocar let out a considerable quantity of lemon-coloured fluid. Whilst an assistant compressed the base of the sac, a mixture, consisting of equal parts of water and tincture of iodine, was thrown in. The injection was allowed to remain three minutes. The tumour did not return; the paralysis of the arms soon ceased, the paraplegia gradually lessened, and two years afterwards the child could walk and run. She is still affected with incontinence of urine.

5. Dr. Strassmann contributes an addition to our knowledge of congenital sacro-perineal tumours, the study of which is interesting not alone from a pathological point of view, but also because these tumours may prove a cause of difficult labour. Wernher (1843) was, says Strassmann, the first who described the cystic tumours in new-born infants. He proposed the name "hygroma cysticum." From their seat he classified them as hygroma cysticum congenitum colli, h. c. c. cervicis, h. c. c. axillare, h. c. c. sacrale vel perineale. The last kind was particularly described by Lotzbeck (1858), who called them hollow fibrous tumours. He divided the sacral cystic formations according to their histological characters into three groups: 1st, pure cystic tumours; 2, mixed cystic tumours, in which a row of isolated or communicating cysts was united by an intermediate substance of considerable thickness to a larger tumour, this intermediate substance being either purely cellular (as, for example, carcinomatous) or connective (fibrous); and 3rdly, agglomerated cystic tumours, in which the cysts and intermediate substance are connected with other tissues, cones, cartilage, &c. Dr. Strassmann describes two cases, which he refers to the category of proper or simple cystic formations. The first case was observed by Dr. Strassmann's brother in 1856. The head presented, the labour was arrested when the breech came to the brim, notwithstanding energetic uterine action. Manual tractions were necessary to deliver. Child, female, living. The cause of the obstruction was a sacro-perineal tumour of considerable magnitude. Of the size of the head of a child two years old, it sprang from the lower sacral vertebræ, having a broad base. The skin covering the tumour was much thinned and traversed by broad veins. The tumour was very transparent and fluctuating, compression caused no pain nor cerebral symptoms, as is the case in hydrorachis. The vertebral column was throughout perfect. The tumour preventing the child from being either laid down or carried without risk of injury, it was determined to reduce it. Forty-seven days after birth more than a quart of fluid was let out by a trocar. The sac collapsed entirely, with the exception of a small solid portion. No nervous symptoms followed. The empty bag of the tumour was maintained by a compress against the left dorsal region. The bag began to fill again, and another puncture was made, followed by iodine injection. Acute pain ensued; sleeplessness and febrile movement. In October, 1858, the tumour was traced in a parchment-like shrivelled bag, without fluid or other contents.

The second case was that of a child which died soon after birth. The labour was easy. Blood was issuing from a tumour the size of a man's fist, attached by a broad base to the sacrum. It was a cystic hygroma. Death was caused by its bursting and bleeding. The tumour was hollow and filled with coagula, containing also a smaller cyst; it extended from above nearly to the upper part of the sacrum. The inner membrane presented placenta-like masses of villous excrescences.

6. Dr. R. Dohrn enters upon a minute investigation of the subject of torsion of the umbilical cord as a cause of closure of the vessels and death of the fœtus *in utero*. After an historical retrospect, he relates the following case:— A primipara, after six months' pregnancy, lost the sensation of the fœtal movements, as she thought, in consequence of a violent fright. From this time the body and the breasts ceased to expand. Six months later still—a year from date of conception—she was delivered. The child was macerated and much shrivelled. The development corresponded to that of a seven months' child. The umbilical cord was of ordinary thickness, eighteen inches long, twenty-eight times coiled from right to left, and showing at its fœtal end for an extent of three inches a strong constriction. This constricted place was only filled by one spiral turn. The navel was dragged forwards. On

injection the vessels were found permeable. The umbilical vein had, however, a diameter of only half a line; whilst a little above the stenosis its diameter was four lines and a half. In the remaining part of the cord it was four lines. The arteries were also proportionally constricted at the same spot. The walls of the vessels exhibited no thickening or deposit of hæmatoidin. The Whartonian matter was altogether deficient. The placenta was in process of fatty degeneration. There were no traces of apoplectic effusions in it. The fœtus exhibited no mark of pathological change, except these appearances in the cord. There is hence no doubt that the torsion of the cord, and especially the stenosis at its fœtal end, was the cause of death.

The memoirs in the following list could not be analysed from want of space:—

On the Suction-Apparatus of New-Born Children. By E. von Siebold. (Monatsschr. f. Geburtsk., Sept. 1861.)

A Glance at the latest Contributions to the Question concerning the Sexual Relation of New-Born Children. By Dr. Ploss. (Monatsschr. f. Geburtsk., Sept. 1861.)

Some Observations upon Extra-Uterine Pregnancy. By Dr. P. U. Walter. (Monatsschr. f. Geburtsk., 1861.)

Amputation of the Cervix Uteri. By J. Marion Sims, M.D. (Trans. of Med. Soc. of New York, 1861.)

Tenth Report of the Royal Lying-in Institution of Göttingen for years 1857-60. By Dr. E. v. Siebold. (Monatsschr. f. Geburtsk., Oct. 1861.)

RECENT CONTINENTAL RESEARCHES ON THE CHEMISTRY OF THE EXCRETIONS.

The work of Bischoff and Voit, 'On the Laws of Nutrition of the Carnivora,' and that of Voit, 'On the Influence of Common Salt, Coffee, and of Muscular Exertion on the Metamorphosis of Matter,' have given a new and most important start to the physiology of nutrition.

Hitherto, however, the experiments of the above-named physiologists, complete as they were in controlling the ingestion and excretion of nitrogen, were deficient in one very important point—viz., in not giving any account of that other not less important factor of tissue-change—carbonic acid. This deficiency has now been supplied by Professor Pettenkofer, who, with the pecuniary help of the King of Bavaria, has constructed an apparatus which answers completely.

The small space allotted to the present communication does not permit of a full description of this interesting contrivance;* suffice it to say that the chief advantages of Pettenkofer's apparatus are twofold. In the first place, the results obtained by it are totally controllable, as can be proved at any moment by placing in the apparatus a lighted composite candle, the amount of carbon contained in which has been previously determined by elementary analysis. For the difference between the amount of carbonic acid as determined in the apparatus after combustion of a certain weight of candle, and the amount of carbonic acid calculated to correspond with that weight of candle, as found by elementary analysis, ought not to be more than half, or at the utmost one per cent., if nothing is amiss with the apparatus. The second great advantage is, that man or animals, whilst experimented upon in the apparatus, are under entirely natural conditions; they are enclosed in a room, eight feet square, in which they breathe quite freely, and have even space to move about.

With this apparatus Professor Voit has been enabled to complete the experiments on the nutrition of the carnivora in regard to the ingestion and elimination of carbon. But perhaps of greater interest than these experiments on dogs, are analogous experiments which have lately been made by an assistant of Professor Voit on man. His researches show, that under certain conditions in man, the same as in dogs, the whole quantity of nitrogen taken in the food reappears again in the urine; and that in health the perspiration does not contain any ammonia or other nitrogenous compounds, as Funke and others had asserted.

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* For such a description, see Medical Times and Gazette, Sep. 14, 1861.

In regard to the elimination of carbonic acid in man, the chief results hitherto obtained are the following, all figures having reference to the elimination within twenty-four hours:—A healthy young man, of 73 kilogrammes weight, exhaled, whilst remaining in complete rest and eating moderate quantities of mixed food, from 207 to 215.7 grammes of carbonic acid. Being restricted to food containing no nitrogen whatever—as starch, sugar, fat—the same individual excreted a little less carbonic acid—viz., 200·5 grammes.

In a state of complete inanition (the experiment commencing twenty-four hours after the last meal), a pretty large reduction in the excretion of carbonic acid took place, the first experiment yielding 180·8 grammes, and the second 180·9 grammes.

After a meal of great quantities of meat (4 lb.) containing no fat (the fat having been carefully dissected out), the quantity of carbonic acid exhaled was found higher than when an ordinary quantity of mixed food was taken, and amounting to 231·1 grammes. The highest figure was obtained after a meal on as much mixed food as could be taken without causing nausea, the excretion of carbonic acid then being as high as 252·4 grammes. These figures show that in man, during a state of rest, the variations in the excretion of carbonic acid keep within narrow limits; the greatest difference between a state of inanition and a state of over-feeding on a mixed diet amounting to no more than 39·6g. In this respect a great difference shows itself between the laws of the elimination of nitrogen and those of the excretion of carbonic acid, nitrogen varying in much wider ranges; for the same individual excreted during a state of inanition 17·1 grammes, and after a superabundant meal on meat 86·3 grammes of urea.

We propose, on a later occasion, to give further details of these interesting and important experiments.

In Memoriam.

SIR JOHN FORBES, M.D., D.C.L., F.R.S.

During the last two years Death has made sad gaps in the leading ranks of our profession. Bright, with his deep sagacity; Addison, with his acute perception; Todd, with his singular tact and insight; Baly, with his calm and calculating judgment, have been taken away; and now we have to mourn the loss of a physician who possessed qualities equal to any of these, and who exerted an influence on his contemporaries and on his art second only to that of Richard Bright.

These pages are an appropriate place for a short memoir of Sir John Forbes, for his chief influence on the art of medicine was owing to the journal which he established and edited, and of which this Review is the successor. It is right, then, to devote here a few pages to the record of a life so active, useful, and influential.

Sir John Forbes was born in December, 1787, at Cuttlebrae, in the parish of Rathven, Banffshire. In 1799 he went to the Academy of Fordyce,* and obtaining a Bursary or Exhibition to the grammar-school at Aberdeen, he proceeded there in 1802. In the following year he entered at Marischal College, in Aberdeen, and remained till 1806. He then went to Edinburgh, and took the diploma of surgery, and in 1807 entered the medical service of the navy.

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* It was at this school that he formed that friendship with Sir James Clark, which lasted through his entire life, and was to him a source of the most constant pleasure.

He used to mention that he came up to London by a Leith smack, and was fourteen days on the passage, and that the journey down to Plymouth to join the *Royal George*, to which ship he was appointed, took three days and nights more. He remained in the navy till 1816, and served chiefly in the North Sea and in the West Indies.

Those who knew him, even as an old man, will have no difficulty in forming an idea of his appearance and habits at this time of his life. He was about the middle height, and was strongly and squarely built; he had blue eyes, a bright florid complexion, and was full of spirits, frank and joyous. His manner was bluff and hearty, but pleasing, from the evidence it gave of sincerity and goodness. His habits were extremely active. Through the whole of his service he was a hard student, and besides going through an extended course of classical reading, he learnt French, German, and Italian. He attributed his proficiency in languages to being for several months in a small sloop with no one to speak to except a young officer in command. A quarrel at length put a stop even to this recreation, and Forbes was driven to his books for society and change.

He obtained very early promotion, in consequence, as he believed, of a Report on the Meteorology of the West Indies. No report of the kind can now be found, and it is possible that promotion was granted on other, and perhaps better, grounds. At this time, and during his whole life, Dr. Forbes possessed the qualities which mark so many of his countrymen. He was a thorough man of business—methodical, accurate, and perfectly to be relied on. An officer of that stamp is of much greater use in the public service than a man of mere brilliancy and originality. Dr. Forbes must have been a first-rate officer, and his qualities in this respect would soon have become apparent.

When in the West Indies he became flag surgeon to Sir P. H. Durham, and acted as secretary as well as doctor; he wrote many of Sir P. Durham's despatches, and was with him when he took Guadaloupe. Shortly afterwards he was present at the capture of a French line-of-battle ship (*Pompée*), whose crew was terribly cut up by the fire of a small brig, commanded by Lieutenant (afterwards Sir Charles) Napier. After the action, Forbes was sent on board to assist the French surgeons, as he was the only man in the squadron who could speak French.

In 1813 and 1814 he served in the home squadron, and in the former year occurred an incident to which he used to refer as one of the most curious of his life. His vessel was off the Elbe, and as he was the only man on board who could speak French, he was sent to Bremen with despatches to a Russian general who was at the head of 10,000 Cossacks. A letter containing an account of this adventure is still extant. Forbes was both surprised and pleased with the Cossacks. Instead of a tribe of savages, he found a set of amiable and pleasant men, remarkably courteous among themselves, and to all strangers except the French. When brought in contact with the French, they seemed suddenly possessed with a devil. On one occasion he witnessed the darker side of the Cossack's character. His escort, consisting of ten Cossacks, surprised two French *gendarmes*. Instead of taking them prisoners, the Cossacks cut them to pieces with the greatest barbarity, and Forbes's attempt to save them only caused the lance of a Cossack to be turned against himself. Before the men were dead, their bodies were rifled and stripped. When this was over, the Cossacks came to Forbes, shook him by the hand, and returned to their previous state of friendliness and good temper. The marches of the Cossacks astonished him, as every one else. Their greatest desire was at that time to join Wellington, who had just forced the Pyrenees and entered France.

They looked upon Wellington as the first general in Europe, and declared that 5000 of them would force their way without difficulty across France.

It was soon after this adventure that Dr. Forbes almost lost his life in a desperate attempt to save the life of a sailor who had been washed overboard. He was a strong and bold swimmer; but the current of the Elbe was so rapid, that he was carried down two miles from the ship before he was picked up. In the West Indies he had saved the life of a man in the same way.

One more reminiscence of his navy life may be permitted. It was in the North Sea that Forbes volunteered for a cutting-out expedition; the lieutenant in command showed the white feather, and would not bring the boat within reach of shot. Forbes, who was all ardour and courage, was so indignant that he tore off his cutlass, and throwing it down at the bottom of the boat, with some strong expressions, declared that he would not be caught on a fool's errand again.

In 1816 came the reduction, and Dr. Forbes, with many others, was placed on half-pay. He then went to Edinburgh, and spent a whole year there (1816–17). He graduated in 1817, and wrote an Inaugural Dissertation, with the title "De mentis exercitatione et felicitate exinde derivata." This subject was always a favourite theme with him; it was clearly handled in the Dissertation, and, as far as we can venture to speak of it, the Latin seems to us both elegant and clear.

At Edinburgh, Forbes attended very diligently the lectures on geology by Jameson, and it was on the advice of this celebrated teacher, who had been requested to recommend a physician acquainted with geology, that he determined to settle at Penzance, where he succeeded Dr. Paris.

He remained at Penzance till 1822, and during the greater part of this time was busily engaged in meteorological and geological investigations. He was secretary to the Geological Society there, and wrote several short essays of singular merit. The 'Observations on the Climate of Penzance' contains perhaps still the best account of this point; and two papers on the 'Geology of the Land's-end,' show great knowledge of the subject, and unwearied industry; but his paper on the 'Temperature of Mines,' written in 1820, is certainly the most philosophical of all his essays, and we are not certain that he ever wrote anything better during his life. It is still much referred to and quoted by writers on this subject.

In 1820 he married a daughter of John Burgh, Esq., H.E.I.C., and in 1822 removed to Chichester, as successor to Sir William Burnett. His life at Chichester has been so well sketched by a friend who knew him intimately, that we shall use his words in describing this part of his life:

> "Early in the year 1822, on the removal of the late Sir William Burnett from Chichester to London, when appointed Director of the Medical Department of the Navy, Dr. Forbes became a resident in that city, where he obtained, and for twenty subsequent years had, the principal practice, extending widely over the neighbouring district of Sussex. At that time Chichester, like other county towns, was undisturbed by railways, and, under the shade of the cathedral, had an air of repose which contributed to make it the favourite residence of a number of quiet families of a certain station and fortune, of those of retired officers of the army and navy, and others who—with the clergy of a cathedral town—formed a highly agreeable society, courteous and polished, a little old-fashioned, but so generally refined and cheerful, that a pleasanter abode was, perhaps, not to be met with in any part of England. When Dr. Forbes selected it for his home, he was in the most vigorous period of his life—active, lively, full of energy, remarkable for the elasticity of his step, and his almost youthful bloom and freshness of complexion; every movement was expressive of an animation unusual in the older days among the physicians flourishing within the ancient walls of undisturbed provincial cities. His hearty manner was perhaps somewhat unpolished, and new to the older inhabitants; now and then it was even moody and abrupt, and these peculiarities were still observable in his later years; but his evident sincerity, his good sense and his general intelligence, soon made a favourable impression on his new neighbours, and with their increased knowledge of him there grew the utmost confidence, not only in his skill as a physician, but in his value as a man. His uprightness,

> and the thorough absence of affectation in all that he did and said, were soon appreciated, and his real kindness was more and more perceived the more intimately he was known. Those who had experienced his attentions in suffering and sickness, or the few who had the privilege of meeting him when a few friends were collected, and conversation was not chilled by formality, were fully aware what manner of man the new physician was. Of evening meetings of this kind, those at the house of the late venerable Dr. Sanden must still be remembered by some surviving friends of that accomplished person, who belonged to a class of physicians now nearly extinct—learned, polished, modest, gentle, unacquainted with any of the ordinary arts of obtaining notoriety, or even professional success, to which they scarcely presumed to make a conspicuous claim. Such qualities scarcely fitted them for business of any kind, and, except in such examples as that of Dr. Sanden, where an ample fortune makes a medical man independent of the favour of the public, are not perhaps altogether to be recommended; but they were never more attractively mingled than in the character of Dr. Sanden. He was then between seventy and eighty years of age, and for at least forty of these years he had lived in great retirement, observing a peculiar regimen, not habitually taking animal food, never taking wine, rising very early, and never dining out, and only known to the community around him by a benevolence which was never appealed to in vain. But although thus carefully avoiding publicity, and leading the life of a secluded scholar, he took a peculiar pleasure in seeing men of more ambitious temperament, and listening to their varied discourse. Among such it need scarcely be said that Dr. Forbes was one of his particular favourites, to whose hopeful views he used to listen with almost as much astonishment as admiration, not unfrequently benefiting him and others, entertained in his elegant retirement in the South Pallant, by the old experience that had made him sage, and always charming his guests (never exceeding three or four) at those simple entertainments by his kindness, his varied conversation, and his graceful vivacity. It was chiefly at these agreeable and exclusive reunions that the acquaintance commenced between Sir John Forbes and Dr. Conolly, which led to their being in after years associated in some medical works of extent and importance. At Chichester they were in some sort rivals for practice, but Dr. Conolly was several years younger than Dr. Forbes, and the rivalship became the foundation of a life-long friendship. When the 'Cyclopædia of Practical Medicine' was undertaken, in 1831, Dr. Conolly's name appeared as one of the editors, with the names of Dr. Forbes and Dr. Tweedie. Some years afterward, when Dr. Forbes established the 'British and Foreign Medical Review,' he arranged to be solely associated in the labour of that work with Dr. Conolly; and this arrangement continued until the latter became the Resident Physician of the Hanwell Asylum, of which the duties were incompatible with sustained literary work. The labour arising out of these undertakings was a source of continual delight to a man of Dr. Forbes's habits of mind; and during their whole progress he was indefatigable, not only in mere editorial work, but in seeking knowledge from all the medical writers of past ages, and enlarging his experience by a continual correspondence and intercourse with the most distinguished of his professional cotemporaries in Great Britain and Ireland, and throughout Europe, and in America. His character for honour and integrity, for punctuality and good faith, enabled him to command the services of most of the distinguished medical writers of his time, and it is generally admitted that their efforts, conjoined with his own, have left an impression on medical opinions which will be recognised for many future years.
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> "From the first few months of Dr. Forbes's becoming a practising physician at Chichester down to the day of his beginning to reside in London, it may be truly said that his public and private life were so consistently ordered as to be written in daily benefits to humanity and science, and to all his fellow-creatures. His great perseverance and resolution led to the expansion of the small Dispensary of that city into an Infirmary, which continues to be one of the most valuable of the charities of Sussex. Every institution for a good purpose, for the extension of judicious aid to the needy, and for the diffusion of education and the advancement of useful knowledge, was patronized by him to the full extent of his power and opportunities. Although disliking all idle sacrifices to what is called society, his house was the hospitable resort of men of science and literature. Above all, his compassionate regard for the sick and the variously afflicted, and the extent of his generous patronage of young persons of talent and virtue, whatever their position in life, although it can never be fully written except in many grateful hearts, was such as to deserve perpetual remembrance among the attributes most becoming and most honourable to those whose lives have been devoted to the study and practice of physic."

In 1840, Dr. Forbes removed to London, and in 1841 was appointed Physician to the Prince Consort and to the Queen's Household. The reasons for this removal will be seen hereafter.

As it was at Chichester that the chief part of Dr. Forbes's professional work was done, or at any rate planned, this seems the place to attempt some general estimate of his character and mind, so as to guide us in our judgment of his future literary and scientific career.

The moral qualities which are alluded to in the memorandum we have just quoted, were perhaps those which were most strongly marked in Dr. Forbes's character. He was one of the most truthful of men; this was partly from a habit of mental accuracy, partly from an original sound moral disposition. This extreme honesty guided him in everything—in every action, in every thought, in every sentence he ever wrote. It was that which gave him his immense influence, and in great measure won for him the regard of so many of his brethren.

His benevolence was almost as marked a feature in his character. It was shown in all ways, in constant kindness and assistance to individuals, in the most strenuous support of charitable institutions. Both in Chichester and London his private and public charities were almost incredible.* He was in this respect almost lavish with his money, and prodigal of his time. But he was not merely generous with money; he was ready with advice, sympathy, and help in all ways. He was generous also in his estimation of the early work of young members of our Profession; whenever he saw honesty, and, as he thought, merit, he sought it out and befriended it. He has been thought to be a severe critic; he was in reality a most lenient judge; when he was severe, he was so at the instance of his truthful judgment, and at the cost of his kindness of heart.

With all this kindness of heart he had a certain impetuosity of temper, which led him to take rapid and strong views. Especially when his moral judgment condemned a man, or an action, he was forcible, perhaps sometimes unduly so, in the expression of his opinion. He had very little of the proverbial caution of his countrymen. His mind was of that strong and determined cast which is not given to reticence, or can tolerate ambiguity. What he thought he expressed, and oftentimes more strongly than in a calmer moment he might have approved. This impetuosity was at variance with his habit of correct thinking, and with his great tenderness of heart. As years passed over, it was more and more controlled by these; and in the latter years of his life it is difficult to imagine a more perfect and even moral character than that which he possessed. At all periods of his life he was perfectly free from malice, and he was very forgiving when he had been, or considered himself to have been, unjustly treated.

His intellectual gifts were of a very high order, but perhaps were not equal to his moral qualities. His truthfulness was here also a marked characteristic. During his college life, and while in the navy, he was fond of writing verses, and some pretty vigorous little poems were published in some of the West Indian papers. He had command of words and of good sensible thoughts, but he was wanting in imagination and originality. The divine insight of the poet was not his, and of this he was himself conscious. All his life, however, he was a great reader of poetry and fiction, and was an ardent Shakspearian. When he lived in London he scarcely ever missed a representation of one of Shakspeare's plays, and he read Shakspeare aloud with considerable dramatic power. His pleasure in seeing good acting brought him into contact with many actors, and he attended a large number in London, and always gratuitously.† He had all the ardour of a Scotchman for the songs of Scotland, and would go anywhere to hear one of his national melodies.

In early life he was more interested in literature than in science, and the circumstances of his life did not fully call out the power of original investigation which he undoubtedly possessed. His powers of perception were very good; he was not a rapid man, but he was not at all one-sided. He seized all the aspects of a case, and his great truthfulness and accuracy led him to be never satisfied till he had done so. His judgment was quite judicial; he considered and balanced, and apportioned with the utmost impartiality; he tried in all cases to cultivate a rigorous indifference, and to welcome the final decision, however at variance this might be with his previous convictions or wishes. Partly perhaps from original constitution, partly from his acquired habit of thought, the tone of his mind was sceptical; before forming an opinion he required the most complete and indisputable evidence. Yet, in contrast with these qualities, and in contradiction to what might have been expected from a character so strong, and in many respects so self-reliant, he was a good deal under the influence of those whom he respected. This occasionally was injurious, and the few mistakes into which he fell in his judgments on men and books were owing to this peculiarity of his mind.

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* Like most benevolent men, he was extremely fond of, and kind to, animals, and always had pets of some kind, especially dogs.

† The managers of the Covent Garden Theatrical Fund presented him, in 1849, with a handsome silver cup, in testimony of these services.

This mind, so large and so strong, was also as active. Work of some sort was a necessity for it, and certainly Sir John Forbes acted up to the views he enforced in his 'Inaugural Thesis,' and in the little lecture he published thirty-three years later, 'On Happiness in Relation to Work and Knowledge.'

In addition to various lectures and little essays, he published, in 1821, a translation of Laennec's great work on 'Auscultation.' His accurate mind foresaw the great weight of this new discovery, and in this he was in advance of most of the physicians of the day. Even Dr. Baillie, to whom the translation was dedicated, expressed doubts whether auscultation would ever be used in general practice, although this great pathologist fully appreciated Laennec's discoveries in morbid anatomy. Some years later Forbes published a translation of Avenbrugger on 'Percussion.' Had he been ambitious of notoriety or fond of money, this would have been the time for his removal to London. He would have had the large practice in chest complaints which fell to others who brought Laennec's views before the public. The translation of Laennec went through three editions; it has long been out of print, but it is much to be regretted that this masterwork, which marks an epoch, should not be more fully known to the younger practitioners. Succeeding writers on auscultation have added little to Laennec's statements; succeeding writers on pathology have lost something of his clearness and precision. If Forbes had continued the editions with notes and emendations, the work would still be in the hands of every one.

It was some time after the publication of this work that, in conjunction with Dr. Tweedie and Dr. Conolly, Dr. Forbes projected the 'Cyclopædia of Medicine.' The original idea of this great work was Dr. Tweedie's. It is unnecessary here to inquire what share of merit should be assigned to each of the three editors. Were Sir John Forbes now living, he would insist, with all the generosity of his nature, on the claims of his colleagues, and would ignore his own. But the 'Cyclopædia' was so great a work, that the credit due for its successful issue is enough for any three men. The character of Forbes' mind was precisely fitted for this sort of work. His wide reading, his careful judgment, his honesty, and his methodical and business habits, made him, to use the expression of one who knew all the circumstances of the case, "the life of the work." His own articles are most excellent, and the one on Auscultation we should still recommend to the student commencing the study of the physical examination of the chest. It would form a good introduction to the elaborate works of Walshe or Scott Alison. In the 'Cyclopædia' was also inserted the "Bibliography," which he subsequently expanded into a separate work. The 'Cyclopædia' was commenced in 1832, and completed in 1835.

When this great work was nearly completed, Dr. Forbes planned, and with his friend Dr. Conolly commenced, the publication of the 'British and Foreign Medical Review.' He has himself, in the last number of this journal, published in 1847, given a most interesting account of the motives which led to the publication of this Review; of the rules which guided him in its editorial arrangements, and of the sale and expenses of the journal. He conducted it for twelve years (1836–47)—four years in conjunction with Dr. Conolly, and eight years alone after Dr. Conolly's new duties at Hanwell compelled him to withdraw from participation in the labour either of editing or reviewing.*

The effect of this Review on medical literature, opinion, and practice has been enormous. The fair, candid tone of criticism, the careful collection of facts, the freedom from all party bias, have now been received as the basis of all medical reviewing, and for this the profession is indebted to Sir John Forbes. The mere literary merits of the Review were great; its scientific character was still higher. It may fearlessly be affirmed that it did more than anything else to gain for British medicine a high position in the estimation of Continental physicians. 'Forbes' Review' soon became known and read all over Europe; and, indeed, in Germany its character was even higher than in this country. In America, also, its reputation was high, and its sale considerable.

At the time of its publication, the 'Medico-Chirurgical Review' of Dr. James Johnson was the only English quarterly medical journal. The 'London Repository,' a monthly journal of great merit, which most flourished some forty or fifty years ago, had died out; the 'Medical Journal' (also a monthly publication), and the 'London Review,' which succeeded it, had been killed by Johnson's vigour and originality, and now 'Johnson's Review' was itself to suffer a serious reverse at the hand of this new competitor for popular favour. Dr. James Johnson was undoubtedly a man of considerable powers; when he commenced his Review, he conducted it with much skill; his criticisms, and those of his son at a later date, were seldom profound, but were highly vigorous and telling. Authors did not like him, for he extracted their best passages, and no one turned to the works themselves. Publishers did not approve, for men bought 'Johnson's Review,' and nothing else. But the public appreciated and bought. The circulation of the 'Medico-Chirurgical Review' was large; it is understood that at one time it averaged 2500 copies.

But with success James Johnson became careless; his reviews were sometimes apparently entirely made up by scissors and paste; he paid no contributors for many years, and hence accepted reviews from any one, careless of the motives which actuated the writers. He paid little attention to foreign literature, and as long as he was not dull, cared for little else.

All this was gall and wormwood to a man of Forbes's accuracy, fairness, and wide reading. He therefore established a Review in which the contributors were paid; and though the remuneration was not great, yet it and the character of the Review together soon caused it to be considered a favour to write a review, instead of its being a favour to give one. Forbes's principle was to get the best and most impartial man to review a book, and *never* to admit a review written by either friend or foe. He did away with the practice of gutting an author, though he liked good analytical reviews. He attempted to give an account of every part of medicine, and of all progress, wherever made; and he consequently made the foreign department of his Review extremely full and valuable. At the close of his editorial career he summed up his principles in a quotation from Bacon on "great place," which he recommended to his successor, but which it will do every man good to lay to heart.

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* The first announcement of the British and Foreign Medical Review was made in May, 1835, and issued from Paternoster-row, the names of the editors being Dr. Forbes and Dr. Conolly. After that announcement, Dr. Forbes considered himself fortunate in effecting an agreement for its publication by Mr. Churchill. We learn from Dr. Conolly that nearly all the editorial labour was performed by Dr. Forbes alone, but that the correspondence between the editors was constant; and he adds, that he remembers Dr. Forbes's lively communication to him of his first introduction to Mr. Churchill, which took place at Chichester, and made an impression that was never effaced. In reality, Dr. Forbes met in the liberal publisher precisely the energetic and exact man of business which a new and extensive undertaking required, and one in whom he felt he could place entire confidence. The first number of the Review appeared in January, 1836.

The effect of the 'British and Foreign Medical Review' was soon marked, and was most beneficial. Dr. James Johnson made great exertions to improve his Journal, and he associated with himself some able contributors. Opposition was of the greatest service to the 'Medico-Chirurgical Review,' and had it been as good before as after the establishment of Forbes's Journal, it is possible that the 'British and Foreign Medical Review' would never have been issued.

Dr. Forbes appeared to have fancied that his Journal would get into circulation without injuring Dr. Johnson's; but the result was that the two journals divided the reading, or rather the subscribing public, between them, and that neither was profitable in a pecuniary point of view. Dr. Forbes's kind heart appears to have felt some compunction when he heard how the circulation of his opponent had gone down. In a letter to his publisher he says—

> "What you tell me of the circulation of our brother surprises me much. I cannot yet believe it; surely we cannot have so reduced the sale. I own I feel rather hurt at the idea of having done so. Still, as I have only used legitimate means, doing only what medical science had a right to demand of me, and what *he* ought to have done, I can have nothing to reproach myself with."

The writers in the 'British and Foreign Review' were very numerous, and many men of note in the present day contributed to it. All that Forbes demanded of a reviewer was, "Has he the best knowledge? Is he impartial and fair?" If a review was unfavourable, he never shrank from the odium its publication might bring on him, and in this way he incurred a few enmities, and lost a few, a very few, friends.

At this time of day it can do no harm, and may be interesting, to name a few of the early contributors. Forbes' provincial position and the great interest he always took in the Provincial Association established by Sir Charles Hastings,* led him in the first instance to seek for reviews among his country friends. The first article in the Review, a long and careful analysis of Professor Bene's (of Pesth) 'Elementa Medicinæ,' was written by Professor Alison, and for more than twenty years this wise and good physician continued to write reviews for the Journal and its successor. Dr. Conolly, as might be supposed, was a frequent and most excellent contributor, and his articles on Insanity, Public Health, and Personal Hygiene, are most valuable Reviews. Dr. Andrew Combe, Sir Robert Carswell, Dr. Streeten, of Worcester, Mr. Newport, Mr. Dalrymple, Mr. Domier, Mr. G. Toynbee, and Dr. Rigby, wrote largely. We trust we shall not offend some gentlemen who still happily remain among us, if we mention the names of Dr. Brown of Sunderland, the two Bullars of Southampton, Dr. Farr (who wrote some admirable statistical articles), Mr. Cock, Mr. North, Dr. Symonds, Dr. Joy, Dr. Mackenzie, Mr. Ure, Dr. Carpenter, as among those who wrote for the first three or four volumes. Shortly afterwards the names of Apjohn, Jones, Laycock, Paget, Taylor, Walshe, Spencer Wells, and West are found, and then the reviewers become too numerous to be mentioned.

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* The Provincial Medical and Surgical Association was formed at a meeting of about fifty medical men, held in the Board-room of the Worcester Infirmary on the 19th of July, 1832, on the invitation of Sir Charles Hastings, the founder. The late venerable Dr. Edward Johnstone, of Birmingham, presided; and those present included some of the most eminent physicians and surgeons of the Midland and Western counties of England. Sir John Forbes, it need scarcely be said, took a warm interest in the proceedings of this important association, and contributed a valuable paper to the second volume of its Transactions (1834), on the 'Medical Topography of the District of the Land's-end;' comprising its natural and physical history, its history—civil, economical; and statistical. This paper is full of interesting particulars of a peculiar portion of our island, collected with his usual industry and related with his usual clearness. It is illustrated by a map of the district, taken from Dr. Boase's Geological Map. The medical history of the district was the subject of a second paper, which appeared in the fourth volume of the Transactions (1836). This paper remarkably exemplifies the author's habits of diligent observation, and the section on the Diseases of Miners is full of curious and instructive matter.

In the first few numbers there were only six or eight reviews, but subsequently the number was increased to ten, twelve, sixteen, and even more. The shorter notices numbered from twelve to twenty, and the extracts from foreign journals, which were given with the greatest care, occupied from sixty to ninety pages.

Of the character of the reviews we have said enough. They were certainly not light or amusing reading, but with scarcely an exception the subjects were well chosen, and the reviews were well and carefully written. At a later period many of them became scientific essays of very great value, and there can be few better exercises for a young physician than to go through a course of reading in Forbes's Journal. Forbes managed to stamp upon it everywhere the character of his own mind—justice, accuracy, depth. He did not write many long reviews, but many of the shorter notices are by him, and he often added weighty paragraphs to the reviews of others. Some of these paragraphs show the man better than anything else. We can well remember being very much struck with a paragraph attached to a review of a work of a man of genius, who unfortunately possessed some of the infirmities of genius. The review itself was not by Forbes, but, as we subsequently learnt, he added the paragraph we refer to. We cannot avoid quoting it as an instance of the temper and disposition of the man. After some remarks, severe but certainly not too severe, on the querulous and unmanly complaints of neglect and want of appreciation and reward from the author in question, the review continues—

> "Such an exhibition of the workings of mere intellectual ambition may usefully show the utter worthlessness of mere intellectual endowment. There is a wisdom, we believe, as well as a happiness, which has its root in the affections and moral qualities; and where it flourishes, the mind expatiates freely and cheerfully beneath its modest shade. In our time this source of wisdom is too little sought. The fruit of knowledge is too often gathered as a mere means of procuring worldly distinction and advantage, and if these do not follow, the blessed fruit itself turns but to bitterness. Hence arise heart-burnings, and jealous controversies, and all that so often makes the minds of scientific men unamiable, and their lives a waste of sour dissatisfaction; and all that so often causes them, when collected into societies, to constitute but a community of angry and stinging insects, whose foibles and whose fierceness console and amuse those who possess no science at all. Even this author, aspiring as he does to the fame of Bacon, forgets one at least of the limitations of knowledge set forth by that great and imperfect man in the outset of his work on its proficience and advancement: 'that we make application of our knowledge to give ourselves repose and contentment, and not distaste or repining.'
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> "The man of science, to be happy, must pursue science with exalted aim; forget that there are Royal Societies and books of periodical criticism; and knowing how small a corner of the curtain that hides all truth from man's gaze can be lifted up by any one hand, should be charitable towards those labouring, like himself, for all time. In this spirit, with unfeigned sorrow we say it, this author appears to us to be eminently wanting; and great as his merits unquestionably are, and praiseworthy and admirable his labours, this defect will detract from his just reputation until, in the course of years, the trifles which trouble the surface of science are swept down the gulf-stream of human events, and the solid deposits alone remain to aid in the formation of the great continent of truth."

We doubt whether a wiser or a better lesson was ever given to irritable and impatient genius.

Dr. Forbes was always very careful in concealing the names of his reviewers. Whether he was right or not in this may be a question; but he thought so, and it was a point of honour with him, neither to divulge nor to give the slightest indication of the name of a reviewer. He inserted in his own copy of the Review the names of the writers, and that copy he left at his death "to his good friend and faithful publisher, Mr. Churchill, as a small mark of esteem and regard." It is by Mr. Churchill's courtesy that we have been enabled to learn the names of the early writers in the Review.

In 1840 Dr. Forbes determined to remove to London. His motive was simply to improve the 'Review.' He felt that in London he would have a larger staff, and would be able to gather more completely the scientific harvest of the day. At that time he was completely absorbed in the 'Review,' and was willing to make any sacrifice to improve it. There can be little doubt that it was a sacrifice; his practice at Chichester was very good; he often made as much as 1500*l.* a-year; he was universally esteemed and liked. He was fifty-three years of age, and was conscious that the time for having a good London practice was past. His appointment to the Household would have been a bait to some men; we question whether it was so for him. We think he was influenced solely by his wish to make the 'Review' still worthier.

There is no doubt that he succeeded in his object, and that for several years the 'British and Foreign Medical Review' was as good in all respects as such a journal can be made. Its circulation was fair; it was not unprofitable, and its verdict on books and men was invariably looked to as the most able and honest that could be given.

During this period Sir John Forbes published nothing except some short lectures, and an account of an inquiry into the truth of some mesmeric phenomena. He succeeded in perfectly exposing the tricks of various reputed somnambulists, but it seems a complete waste of power when we read the account of the dealings of his vigorous mind with these shallow pretenders. It is really like the *Warrior* running down a Thames wherry.

The period from 1840 to 1846 must be considered to have been a prosperous and happy one. He was respected by all, was looked on with affection by many; he knew himself to be useful, and must have felt that in his way he was powerful and influential.

In 1846, however, a circumstance occurred which had considerable influence on the fortunes of the 'Review,' and on Sir John Forbes himself. We allude to his celebrated article on "Homœopathy, Allopathy, and Young Physic." The effect of this article must be allowed to have been in the first instance disastrous. That Forbes, to use his own words, had only one object in writing it—viz., "the improvement of the medical profession," all who knew him and his inmost thoughts, would testify. But the article must, we believe, be now admitted to have been a mistake. Even now, on re-perusing it, we are astonished that a man of such judgment could have written some part of it. Although with the logic which was natural to him, and with the sarcasm which he could wield at will, he tore to pieces the nonsense of infinitesimal doses, and the absurd idea that almost all diseases are caused by the itch; and although he did not bring forward a single argument in support of Hahnemann's hypothesis that "Like cures like," he yet spoke of Hahnemann, not only with respect, but as one who should be placed on a level with "Paracelsus, or Stahl, or Silvius, or Boerhaave, or Brown." It is difficult to conceive what sort of analogy he found between these several men, and especially between the weak, dreaming, illogical, and, as we believe, insincere mind of Hahnemann, and the subtle spirit which animated Stahl, or the close and sustained powers of observation which made Boerhaave famous throughout Europe.

But this was not his only offence in this article; he used the term "Allopathy" as applicable to the ordinary practitioners, although it neither expresses their creed, nor explains their practice, and in his vehement onslaught on the enormous evils resulting from indiscriminate drug-giving, he did not perhaps sufficiently point out that his strictures were directed, not against the proper use of drugs in their proper places, but against that indiscriminate dosing which looked upon the human body merely as an appointed and convenient place for stowing away mixtures at three and sixpence, and pills at half a crown. In insisting strongly on the curative powers of Nature, and on the necessity of aiding and not impeding them, he did not sufficiently consider, or at any rate state, that this doctrine has always been taught, and by the best physicians has always been practised.

The consequence was that the article was not understood. What was objectionable was on the surface; what was excellent and good—and much of it was so—was less apparent. If any other man had written it, it would have been of less moment; but that the wise and accurate Forbes should have done so, seemed almost incredible. What, then, were the reasons that led him to write it? They may perhaps have been these.

His natural scepticism and his logical mind led him to be much dissatisfied with the loose evidence which is all that can be brought to prove the efficacy of some even of our boasted medicines. He was also extremely impressed with the folly of that practice which, instead of searching with great care into the causes of disease, and placing the sick body under the hygienic conditions most favourable to health, is satisfied with pouring almost hap-hazard into the masterpiece of nature, the most powerful and dangerous drugs. He wished to strike a blow at both these errors; a blow strong and mighty, and that would be felt.

Then with regard to homœopathy, he desired to judge it fairly; he had that sense of justice which can almost pass into injustice. He was so afraid of being unfair to an adversary, that he was led to be unfair to himself. He could not hold the balance quite even, and he inclined too much to the weaker party. Then, again, his very fearlessness and moral courage would influence him. He knew the outcry that would be raised against him, and he would have thought it cowardice to soften one word for fear of giving offence.

The effect of this article was, as we have said, at first unfortunate. It lowered Forbes's influence and the influence of the 'Review.' But by degrees it has produced the effect he contemplated. His courtesy towards homœopathy is forgotten; his exposure of its errors is not so. His sternness in dealing with the shortcomings of his own profession has been long since forgiven; his powerful arguments against these shortcomings have borne their fruit. That Forbes has been mainly instrumental in bringing about a more rational mode of treatment among the great bulk of practitioners, and especially among the junior members of the profession, we have no doubt. The good resulting from his article has far outbalanced the evil.

If it be said he could have obtained the good without the evil by a gentler and milder plan, we grant it. But who will cast a stone at this man, so honest, so earnest, so penetrated with the single wish to do good?

At the close of 1847, Dr. Forbes decided on giving up the 'Review.' His reasons were that he was then sixty years of age, and he found the work too much for him. He may have been influenced, also, by finding his work less pleasant. He was a man of a most kindly and even sensitive nature, and the feelings he had raised distressed him, though they could never intimidate. His 'Review' was bought by Mr. Churchill, and was incorporated with Johnson's 'Medico-Chirurgical Review,' and the two old opponents have since prospered together, and it is hoped have not much degenerated from their vigorous parents.

On resigning the 'Review,' a large number of Dr. Forbes's old contributors, friends, and readers combined to present him with a testimonial. A superb candelabrum was presented to him by Mr. Stanley, in the name of 264 physicians and surgeons of this country and America. Mr. Stanley's eloquent address recapitulated all Dr. Forbes's many services to medicine, but did not say one word that was too much nor too strong.

From 1848 to 1859, Dr. Forbes continued to reside in London, and employed himself chiefly in works of benevolence and in literary labours. In 1857 he published a little work, called 'Nature and Art in the Cure of Disease,' which was intended to explain his views more fully than had been done in the article before referred to. He wrote it with the knowledge that it would be his last professional work, and he "bequeathed it as a legacy to his younger brethren." It may be said to be an exposition of the views on "Rational Expectation," promulgated by Stahl, and to be intended as an examination of the relative powers of Nature and Art. It is a most able and thoughtful work, yet deals a little too much in general statements, and is even in parts open to the charge of indefiniteness and ambiguity. Those who believe that Forbes underrated the powers of medicine rightly and skilfully used may be referred to his chapter on The Instruments of the Medical Art, in which is an admirable summary of the various remedial means which the physician can set in action.

He also published several accounts of summer excursions:—'A Physician's Holiday,' 'Memorandums made in Ireland,' and an 'Excursion in the Tyrol,' are all works which were published in this period, and met with much success. They are not brilliant, but are sound good works, with much reliable and carefully arranged information.

In 1853, the honour of knighthood was bestowed on him. The University of Oxford had in 1852 conferred on him the honorary degree of D.C.L. He was a member of learned societies belonging to almost all civilized nations.

At the end of 1854, Dr. Forbes was requested by the Government to organize a large hospital at Smyrna for the sick of the Crimean war. He accepted the post with alacrity, and made all the principal arrangements. But he finally resigned on the score of health. It is believed that he became aware even then that his days were numbered. Curiously enough, he appears to have had a presentiment, or, we should rather say a belief, founded on correct observation, that the cause of his death would be some affection of the brain. Three years subsequently he began to suffer from giddiness, and frequently fell; and in the course of a few months this increased to such an extent, that he left London in 1859, and went to live with his only son at Whitchurch, near Reading. Here he gradually sank, and died easily and calmly on the 13th November, 1861.

It has been supposed that the cause of the softening of the brain—for that probably was the fatal disease—was Sir John Forbes's incessant mental work. But this may be doubted. For many years he had suffered from pulmonary emphysema and some enlargement of the heart; it is more probable that there was general atheroma of the arteries, and among others, of the cerebral vessels, and that the nutrition of the brain finally suffered. It appears from a letter still extant, that when he went to Penzance his lungs were affected in some way, and that probably influenced him in choosing that locality.

And now we must bring this brief memoir to a close. We have done our best to draw Forbes as we knew him. But how pale and tame a portrait have we drawn of this noble and large-hearted man! Would that to some pen which could draw the outlines more boldly, and could fill in the lights and shadows more delicately, this task had been assigned. But after all, the true character of Forbes is in his works. By these posterity will judge him, and no friend of his need fear the verdict. He will stand out among the physicians of his day as the bold and yet cautious thinker, and as the uncompromising reformer. His benevolence and his charities will find no record, nor perhaps is it meet they should. But his uprightness, his candour, and his moral courage need no testimony, for they are impressed upon his works.

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THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

APRIL, 1862.

PART FIRST.

Analytical and Critical Reviews.

REVIEW I.

La Folie Lucide etudiée et considerée au point de vue de la Famille et de la Société. Par le Docteur TRÉLAT, Médecin à l'Hospico de la Salpêtrière, &c.—*Paris*, 1861. 8vo, pp. xvi. 357.

Lucid Insanity, studied and considered in Relation to Family and Society. By Dr. TRÉLAT, Physician to the Salpêtrière, &c.

IF we could safely proceed to so extreme a dogma as that which reduces generally the idea of vice to the idea of insanity, and which would typify the gaol, therefore, as merely a less reputable phase of the lunatic asylum, our task in considering such a work as that of M. Trélat, in at least the more essential portion of its details, would be greatly simplified. Nor, as is sufficiently known, would we, in adopting this course, be wholly without precedents to sanction us; for there have not been wanting men who, influenced by a few plausible views, sustained on partial truths, which we shall not venture to term arguments, have, even in a worldly sense, looked upon virtue as in itself so amiable, and so certain to be finally crowned by its rewards, and upon vice as so hateful, and so sure to be pursued by its punishments, that it could only be just to regard the conduct which forsakes good to follow evil as the offspring of that utter irrationality that belongs to a defective intelligence.

But, alas! in the discrimination of human conduct, folly, or the neglect or the abuse of reason, is not the equivalent of insanity, or the perversion or the want of it: and we fear that we must not seek an escape for man from his responsibility for his vices or his crimes by charging these, unless stintingly and exceptionally, not upon his own natural or acquired depravity, but upon that which an inscrutable Providence has laid upon him as a divine visitation. While hinting, however, our difficulty in disconnecting such views from certain portions of the doctrines urged in the volume before us, as well as in other writings of a kindred character, we shall not advance so far as to hold as wholly prejudged that question, some of the special applications of which will offer themselves for closer examination in the course of our remarks. Concurring cordially, it is but just to state, in the scope and the design of our author's observations, and prepared to give a respectful consideration to all of them, we shall leave those points, in which we consider it necessary to oppose or to limit his conclusions, to evince themselves in their due order, and shall willingly refer the decision to the discretion of our readers. It will probably be chiefly to the topic of the so-called *Dipsomania* that we shall direct our attention; led towards it by the interest which it has recently excited in this country, the arena of discussion being still open and the matter *in foro.*

The work of M. Trélat is one of unquestionable value and importance. Perhaps, everything considered, we ought not to take special exception to the idea of paradox and contradiction involved in its title, conveying, as that does, the notion of the co-existence of rationality with irrationality, of the darkness of insanity with the light of the higher intelligence, jointly and simultaneously ruling in an individual mind; because anomalies of this kind have long been familiar to us in the designations adopted by other eminent writers, with whom, indeed, they seem to spring necessarily, however unfortunately, from the direction which has been assumed by certain of their speculations. To not a little of that peculiarity of tone, in colour and sentiment, which belongs to our vivacious neighbours, M. Trélat adds much of that genuine ability which is even more justly their characteristic. He has, he tells us, a leading object before him; and this consists in the promulgation of an earnest warning against intermarriage among families, not merely where indubitable mental derangement has evinced itself, but where eccentricity, or depravity, or dissoluteness of conduct has given token of whatsoever description of departure from the customary rules of a prudent and reasonable behaviour.

Some of M. Trélat's animadversions on this point have their application more especially to continental usages in nuptial arrangements; where little is ordinarily left to those who are themselves to enter into the union, but where all, or nearly all, is the subject of bargain and contract among the parents, or natural or legal guardians, who arrogate authority to join those to whom have been permitted but scanty opportunities of acquaintance with each other; nothing being regarded beyond what is judged to be a due consideration for pecuniary interests, in the advancement of which, according to our author, few scruples intervene to prevent a reciprocal use of reticence and fraud, directed towards the concealment of the defects of those whom it is designed to intermarry. No one will question that such matters, on every fundamental ground, are equally deserving of earnest attention in this country, notwithstanding that, through the state of our manners, there may be no precise identity in the peculiar and less essential conditions against the influence of which it is necessary to contend. Not otherwise here than in France, the individual is rash that enters into connexion with a family whose mental aberrations, or whose vices, give no warrant to hope for a happy union, or for a well-trained, even if a well-constituted progeny. Yet such unions, with all the openness of our antecedent opportunities for mutual knowledge and appreciation, are still frequently and too heedlessly entered into among us to render a word of caution superfluous, and we may take, though with some variety in their basis and in their application, many of those admonitions to ourselves which M. Trélat has addressed to his countrymen.

We might have desired in M. Trélat's work a somewhat more rigidly scientific classification, yet possibly this was not necessary for the semi-popular nature of his design. That design he defines more narrowly to be a passing beyond the forms of insanity which are easily cognizable, with the view of pointing out, and causing to be acknowledged as diseased, more than one mental condition hitherto regarded as sane: a task which he acknowledges to be difficult, but which he believes to be not beyond the resources of our present and our future science. The state of the individuals thus affected, he considers to be for the most part incurable; and hence he urges the greater necessity that those endowed with reason should be taught to recognise them, and admonished to hold aloof from close alliance with them. Commencing with those who are merely intellectually weak, he proceeds to range among the other subjects for his consideration those affected with satyriasis and nymphomania; monomaniacs less strictly definable; erotomaniacs; the jealous; dipsomaniacs; spendthrifts and adventurers; the proud; the mischievous; kleptomaniacs; suicides; the irreclaimably indolent; and lastly, a class whom he designates as lucid maniacs. The most formidable of all these he considers to be the groups of the proud and the mischievous, and the lucid maniacs. A copious array of cases illustrates each of the categories. Many of the examples adduced might be held to be instances of unquestionable insanity, proved, as every case ought to be proved, not by the single presence of the attribute or propensity that suggests the designation, but by an association of other and mutually confirmatory symptoms, for in mental nosology, as in corporeal, one symptom is no symptom.

Others of his examples we approach with far greater hesitation. It is with reference to these, when we reflect how necessarily all crimes are referable to offences against the person, feelings, or property, and how large a portion of our gaols is consequently filled with those who have been convicted, and often repeatedly convicted, of such offences, or, in other words, with those whom an ardent morbid psychologist might denominate the mischievous and the kleptomaniac, that we seem compelled to pause at the outset, to question whether it be really intended that the prison and the lunatic asylum should finally become, with the march of intelligence, in so far convertible terms: and whether we ought reasonably to understand by them, and with regard to all inveterate offenders, merely different classes of institutions appropriated to different descriptions of mental sufferers, whom the further growth of knowledge will ultimately cease to hold as separate; while what will be accounted merely a becoming delicacy will refuse longer to employ titles which are thus wrongly distinctive, because challenging for one form of mental disease sympathy and for another opprobrium. For our own parts, we shall continue, we suspect for a long time, to demand that more shall be shown, for example, of any individual than a rooted propensity to steal, in order to prove that he is a maniac. That he may be already rich, and under no necessity to steal, we shall not allow as the test that he is under no temptation, until it shall have been clearly laid down at what point acquisitiveness ceases, so that the merchant shall hold himself content with his gains or the avaricious man with his hoards. Should the appropriation be of objects ridiculous in themselves and of no value, and the individual come home loaded with rusty iron, fragments of glass, or tags of rope, we have of course, if he be rich, *primâ facie* evidence of insanity; but where pieces of plate, or lace, or silks, or other more or less covetable and costly commodities, are the articles pilfered, we must demur to a plea that leads to so gentle and charitable a solution, and must ask for further and less questionable demonstrations of a shaken intellect. For the poor as well as for the rich, moreover—nay, even more manifestly, because more unfailingly, for the poor than for the rich—honesty has its most certain reward; so that, if there be a real and intrinsic insanity, the essence of which lies in a propensity to theft, there can be no reason why the one class, surely not less liable to infirmities, should not be admitted to have its occasional victims as well as the other. Till, therefore, the plea of kleptomania shall have been once sustained successfully for one of our poorer culprits, we must remain sceptical as to the justice of its efficacy for the richer, whenever urged in its pure simplicity. Add other proofs, however, of an unsound mental condition, and the dishonest propensity may well stand as their corroboration, and receive that weight accordingly in which it would otherwise be wholly wanting. When our courts shall content themselves with less than this, and when they shall carry the involved principle onwards, yet not beyond its conceivable conclusions, into other spheres of vice and depravity, it will then indeed be time for us to think, be the consummation a happy one or not, of our having no longer the need of either prisons or gaolers, but of only hospitals for the mentally sick and psychological physicians.

But leaving the more general question, it is to the class of so-called dipsomaniacs, as we have already indicated, and to the opportunity afforded us, through the consideration of it in M. Trélat's volume, of renewing a discussion regarding the important bearings of the topic of dipsomania upon our social polity, that we now desire more exclusively to direct attention. It is everywhere manifest that there is a growing inclination, in whatever way originating, among medical men, to include the inveterate drinker among those afflicted with mental disorder; while commissioners in lunacy have signified their pretensions, accordingly, to have them transferred from under the control of the civil magistrate to their own peculiar jurisdiction; and the superintendents of lunatic asylums consentaneously evince their readiness, we had almost said their impatience, to offer a retreat to the drunkard within the precincts of their institutions. The latter, doubtless, are not uninfluenced by what must be a natural anxiety to have a practice of this kind extended and legalized, which, in fact, they have already acceded to partially, yet hitherto only tentatively, and on little more than sufferance. None of this, however, has taken place without such an amount of wavering in plan and suggestion as to show that there remains, among all those who have approached the question, still abundant hesitation and dubiety as to the fundamental propriety of the course contemplated. While one set of innovators proceeds to pronounce the inveterate drunkard unequivocally insane, and consigns him simply, as such, to the medical psychologist, to the ordinary asylum, and to the lunacy commissioner, with a view to a treatment not differing generally from that enforced upon other insane persons, another regards him as indeed insane, yet occasionally, or always, with such a qualified insanity, that he seeks for the peculiar maniac a seclusion in a retreat appropriated to himself, and which may, or may not, have its medical superintendent, but which is to have, not the less, a lunacy board for its superior jurisdiction. Nor is a third class wanting, and that, too, with no ordinary weight of authority on its side, which adheres to the notion of insanity, yet seems so startled with its own decision as to recoil from what might be regarded as the legitimate consequences; and which claims for the alleged lunatic, neither necessarily the care of the physician nor the supervision of the commissioner in his enforced seclusion, but desires to leave him, as if he were still to be regarded as merely an ordinary transgressor, under the sole authority of the ordinary magistrate.

For the most part, however diversely modified and directed the ideas of the different advisers, the creation only of self-supporting institutions appears as yet to be contemplated by any; and the poorer drunkard, in the midst of what may be accounted by many as an exuberance of legislation otherwise, is to be left in an outer sphere, to embarrass and perplex by what, under any circumstances, would be the hardship, and, under the notion of insanity, must be the anomaly of his position. It is not to the purpose to say that a small proportion of the improvident and the dissipated, when reduced to the abyss of impoverishment, find now their way into the poor-house; for there is here no question of insanity, and little thought of cure. The sole poverty, like any other shape of poverty, alone opens the gates, the last, before those of the grave, to shut in upon whatever form of ruined humanity. Manifestly, then, where there exists so great a diversity of opinion and of project, or where an idea seems, if it be the true one, to have halted within the limit of its proper and apparently indispensable development, much must still remain to be considered and adjusted before the public can be reasonably expected to award the approval that is requisite to call an enactment into existence, or that could impart to it weight and sanction afterwards. The theme, precisely in the proportion that it is an important one, and of extreme delicacy, is one that is not fitted for crude legislation; while we should regret, with the greater reason, to see the discussion of it silenced, before the general views regarding it, whether of assent or of denial, had been ripened into unity and safety. Possibly the consideration which we now design to bestow upon it may contribute something towards this desirable result.

To show what is the momentous nature of the subject, and how great the demand it implies, or may be extended so as to imply, on the public care and generosity, demands only the barest statistics. We do not enter on the question of the special bearings of the vast evils of intemperance, acting as these do upon the individual, and ramifying beyond him through the whole social system. It is a mere question of numbers with which we at present concern ourselves. It has been computed that there are not fewer than half a million of drunkards within the United Kingdom: but of course there are no materials which render it easy for a proportion of this kind to be determined, and it may be safe, therefore, to yield to what cannot be otherwise than the gratification of attributing exaggeration to a statement, which sets down nearly every fiftieth member of the community as addicted to so gross a vice. And yet, were each to turn himself to his own individual experience, and to reckon up the many friends and acquaintances, or the individuals of whom he was otherwise cognizant, who have ranked themselves among the victims, and were he to extend the idea of this to the general experience of the country, the aggregate would speedily assume proportions, if not so extravagant as those at which we have hinted, yet well fitted to surprise by their magnitude. What village or community of two or three hundred inhabitants is there, which cannot point to its one or two individuals notoriously addicted to habitual intemperance; while, in some of our larger cities, may we not find courts and alleys in the low quarters, whose whole adult population might almost be included in the same category? In London, the drunkards have been reckoned at 1 in 113, and the thieves at 1 in 266 of the whole population. This would give upwards of 24,000 drunkards for the metropolis alone. But this has more than its parallel in Norway, in five of the chief districts of which, according to the judicious Dahl,* founding on the diligent researches of Sundt, the utterly drunken and the doubtful vary in numbers, so as to constitute together from twenty-seven to forty-three per cent., or from above a quarter to nearly a half, of all the married men and the widowers; as well as in what is disclosed by the able and ingenious researches of Lippich,† in Styria, where reason was found to estimate the proportion of drunkards at one in about eighteen of the whole adult male and female population.

Few, then, will be inclined to charge us with an over-estimate if, reckoning the whole population of Great Britain, excluding Ireland, at twenty-three millions, we compute, in round numbers, the proportion of drunkards at 1 in 300, or at a total of about 76,000. Escaping, as we can, from the indignation of those who have adopted so large a number as 500,000 as the proper estimate, and anticipating, on the other hand, a possible demur on the part of those who, looking to the project they have contemplated, find themselves appalled by even the lesser number that we have admitted, we can scarcely be entitled to concede more, and to exclude any considerable portion of our 76,000 from the contingent operation of that legislative enactment, of whatsoever quality, which is to be destined to bring the drunkard under a more immediate surveillance and control, while one more specially adapted to his real or presumed condition, than belongs to our ordinary police arrangements. We are not unmindful, besides, that however doubtful may be the character of the statistics as to the benefits, for the ordinary insane, of seclusion in asylums, and however imperfectly established the relation between these and the proportion recovering at the same early period under a home treatment, there can be no dubiety with regard to the drunkard, with whom, certainly, the sooner his habit is encountered, the more readily will it be checked and rooted out. Where an attack of insanity has supervened suddenly, from apparently obvious causes, and in circumstances under which a home treatment has been possible, we have repeatedly seen such treatment wholly successful; while it is easily credible that the patient's condition might have been prejudiced by a hurried removal to an asylum, as it would besides be certain that his after reflections, and his after prospects in life and its employments, would not have been improved. Where the mental disorder has approached gradually and slowly, as the habit of the drunkard advances gradually and slowly, there is increased difficulty in the cure; but this is true, whether of the asylum or of the domestic circle, though for the insane the resources of the asylum are then assuredly generally superior, or may even be indispensable. Of the drunkard, we know how difficult it is to annul his habit, when once established, by any duration of confinement, and how prone its re-appearance is to follow his release. The habit should be assailed, therefore, if the measure be to have its due spirit and efficacy, as soon as it is recognised.

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* Sindssyge i Norge, p. 104. Christiania, 1859.

† Grundzüge zur Dipsobiostatik, p. 57. Laibach, 1834.

Thus, as the drunkard should be early secluded, if seclusion, with the simple and tangible object desired for him, be the proper treatment, the number to be subjected to it will be proportionately enlarged; for no confirmed drunkard, in fact, can be rigorously excluded, inasmuch as, if restraint be thus beneficial, each that is excluded is injured. Still, to avoid the veriest pretext for cavil, we shall hold ourselves, for the nonce, within the limit that reason and truth thus seems to allow us, and shall estimate the number properly amenable to the new legislation at only a half of that previously stated, or at 38,000 individuals. Considering that, out of the gross number of ordinary lunatics in Great Britain, those hitherto defined and admitted in public establishments as such amount to only about 27,000; and yet remembering that difficulties in the accommodation of this number are beginning to be so experienced, that already suggestions are proceeding from the central authorities for the withdrawal of a large proportion of them from the asylums, and for their restoration to the homes and to the private guardianship from which many of them had been previously almost severely and inexorably separated; we might well pause, with some show of diffidence on this score alone, before irreversibly engaging ourselves, or inviting the public to engage itself, in any new expansion of what seems already felt to be a sufficiently extended enterprise. But we own no inclination to occupy a ground so narrow. If there be a real emergency, based on a real justice, that emergency must be encountered; and the nation must proceed, having measured itself duly with the actual requirements that it may judge of what lies before it, to meet the demands it involves, with what capabilities and with what energies its resources can supply. If the project, on the contrary, be opposed to reason, and herein lies the main question, or even if it be likely to prove simply inefficient, as in the one case we should have a positive evil, and in the other a costly negation, let the idea of it be abandoned at once, and let us turn prudently to some more promising direction in search of a remedy.

But it may be maintained, that the public is no further involved in the measures contemplated than that it shall sanction them by an approval of the views on which they are founded, so as to admit the existence of an actual want urgent enough to justify a legislative interference: that there needs to be no call upon its generosity, for that the institutions created will be self-supporting, because they are only designed to be open to the rich: and that, if the measures fail, the failure will prove pecuniarily injurious only to those who have used their private resources to carry out a public enactment, of the result of which the utmost that can be said will be, that they should have dealt with it less confidingly. It is, however, contrary to the spirit of all our institutions, contrary to our experience in other matters, and contrary, indeed, to what ought to be our experience, to imagine that, if places of legalized seclusion for the wealthy sot be found to be proper and beneficial, they can long be denied to the poorer transgressor also. It has been well said by Adam Smith,* that dissipation is more dangerous to the poor man than to the rich, because its effects are more immediate, complete, and irreparable. There are few of us who have not seen the home of the poor drunkard, without furniture, without fire, and with its sickening squalor; or his family, half-naked, half-starved, wholly untrained, or trained only to evil; or himself, ragged, bloated, begrimed, without the stay of friends, and almost without the chance to retrieve his position, if he could raise himself to the needful exertion, or to the hope: and which of us, who have so seen, would not desire some means for his being arrested in the terrible proneness of his descent, or, where too late for this, for redemption from its utter completeness, should such means have been found to have their worth or their energy for his wealthier, better instructed, and therefore less excusable fellow-delinquent, if, under the idea of an insanity, we ought not rather to designate him his fellow-sufferer? Besides, we know that it is not the ordinary tendency of ideas, for which a limit is thus originally assumed, to permit the projects to be developed from them to confine themselves long within the bounds first contemplated or imagined. Numbers of fitting objects are gradually discovered, whose existence was at first not known or acknowledged; the statistics involved extend themselves over wider ranges and greater masses; and, year by year, the attention which has been fixed upon the topic sees its aims multiply, and its functions enlarge. Thus, by a spontaneous sequence, the arms of that jurisdiction which includes and protects the rich drunkard will not long be able to repel the poor. The commencement alone is wanted. The extension is a customary, and will be almost a sure and a necessary, consequence.

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* Wealth of Nations, Book V. chap. i. part 3, A. iii.

It would be unjust, however, to omit, that an attempt has been made, independently of the mere relations of comparative wealth, so to restrict the category, that the risk of an extension of this kind may be expected to be materially lessened, if it be not, in one sense, altogether annulled. A writer, whose talents and knowledge in relation to this question do not permit us to rank him behind any other, lays it down as a maxim, though it is one to which, as in the case also of kleptomania, it does not always seem easy for him to adhere with rigid consistency, that any token of mental aberration, supposed to constitute the insanity, does not establish this by its own independent existence, but by its appearance as one of many symptoms, collectively indicating and proving disease affecting the brain. Viewed apart from these, drunkenness is no more a pathognomonic symptom of insanity than the heat of skin, or the cough, is a pathognomonic symptom of scarlatina or of pneumonia, both of which require the association of other tokens, in order to evince the presence of the disease. Insanity becomes thus defined as an apyretic affection of the brain, in which emotions, passions, or desires are excited by disease (and not by motives), or in which conceptions are mistaken for acts of perception or memory.* It is as an illustration of this that it is stated, that a man may be carried to bed drunk every night of his adult life, and give no room for any question as to his sanity, for which something beyond, incontrollable, and indicating real insanity, is requisite. Now the views thus enunciated would seem to limit the idea of the so-called dipsomania, to those cases where the existence of the propensity to excessive drinking was only one symptom, added to a group of other symptoms, manifesting together the mental disorder, of which the drinking propensity by itself would be no sufficient criterion: or, in other words, that to prove insanity we must have other evidence of insanity than the mere habit of intoxication, though carried to daily and extreme excess. No one ought to demur to this; for, while it is what we have required ourselves, it is less than is usually advanced by the expounders of a dipsomania, and reduces itself, in fact, to little more than to say that the vice of intemperance may become associated with mental disease in one and the same individual.

To say that the insane are never prone to intoxication would indeed be preposterous; yet it is not the less a truth, that in those examples of melancholia and depression, in which we might naturally expect a resort to this species of excitement, nothing is more rare than that it should actually present itself. On the other hand, to deny that the drunkard ever becomes insane would be alike preposterous; and yet confirmed insanity, other than the more fitting and casual delirium tremens, is very generally held to be comparatively and, we may add, even unexpectedly rare as an unquestioned product of the habit of intemperance, the causes of mental derangement being, in fact, and this is well worth remembering with a view to the question before us, infinitely more frequently psychical than physical. Still, if we remember that the proportion of the insane to the general population of the United Kingdom may be approximatively reckoned at about one in six hundred, we may well admit that, out of our myriads of habitual drinkers, all of whom are at the age at which insanity is prone to occur, mental affliction, under the various shocks to which they subject themselves, ought to be observed in a considerably larger proportion. Our native statistics, however, on this point, have been of the most vague, divergent, and fluctuating description; some of our psycho-pathologists, who formerly assigned a large influence to drunkenness in causing insanity, having now veered to the more questionable proposition of charging the insanity with causing the drunkenness, and having conformed their tables to the new doctrines. But the alternative of either view would not carry us beyond the roll of the ordinary insane; and there would be no call for us to provide for any added host, were it not that this class of writers, while they aver a distinction between the habit of intemperance and the disease of dipsomania, universally admit the proclivity of the former to pass into the latter, or to become what they maintain to be a true insanity: from which we advance to the easily to be anticipated corollary, applicable to this as to any other insanity (nay, as we have already indicated, reasonably more applicable), that it is the province of the physician to detect its premonitory signs or earliest symptoms, and, these being discerned, to take measures at an early stage for its prevention or cure.

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* Skae: Edinburgh Medical Journal, 1861, p. 881.

Thus, our prospect widens, and the whole field of inveterate drunkenness, insane or not insane, comes once more within its range. It is a confirmation of the view that the dipsomaniac, as ordinarily understood, is merely the confirmed drunkard, with no correlative evidence of mental aberration besides, which we find evinced in the consideration that by far the greater number of those for whom seclusion is now resorted to may be shown to be thus simply characterized. We have entered into an analysis of a variety of cases, of those who are either at present, or who have been formerly, in confinement for drunkenness in the customarily selected description of retreat. Notwithstanding the difficulty of arriving at the truth from individuals so unhappily circumstanced, we have ascertained that of twenty of these, an unselected series which we considered extended enough for our purpose, thirteen had sunk into that condition for which their seclusion was held justifiable, under the gradual influence of these common incitements of company and of example which prepare the way for a habit; while one professed to have become somewhat more rapidly a victim owing to domestic unhappiness; and six alleged excessive occasions of grief as the cause of an almost sudden lapse into intemperance, though even for any of these it is scarcely safe to assume more than that their previous degrees of familiarity with intoxicants were less than with the others. In not one, even of the most suddenly implicated, was any associated token of insanity ascertained to have presented itself.

Another justly eminent and highly experienced physician, to whom we certainly do not concede the less weight on such a question that he is not a specialist, and from whom no one would willingly differ without subjecting his own opinions first to a severe mental challenge, states of a form of habitual intemperance which he describes,[*] that no medical man of consideration doubts that he has here to deal with a form of insanity, which, originating as a physical impulse, unrestrainable by the moral powers, becomes first a mental furiosity, and, eventually, also a fatuity. Our own experience of the views of not a few medical men of consideration, at home and abroad, to whom we have listened on this point, is, we must avow, by no means so absolute as this in any respect; and, in some respects, is diametrically opposite.[†] But the question is not one which we can leave to be settled by authority; for we know the old danger of believing that a dogma is necessarily true, merely because distinguished men have alleged it, and we look for conviction where we have no right to repose on faith. No one would deny, that where mental furiosity, or, still more, fatuity is present, there is insanity: but it would involve a *petitio principii* to assert unconditionally the presence of either in the inveterate drinker; and, as to an insanity besides this, we must not assume that to be true always, which may be true occasionally, but which, true or not true, can only be decided by individual proof. Nor can we admit that they greatly err, either in science or in polity, who join in our desire to have a clear token of insanity, associated with the intemperance, but recognisable apart from it, before they can accept the latter as standing in any strict relation to a mental derangement. The eminent writer in question does not refuse, certainly, to admit that there is an intemperance which is a vice, besides this intemperance which is a disease, and ought to be treated as an insanity; but he sees no necessity for insisting upon this distinction, for the reason that it is not less desirable to cure the vice than the disease. As to the objection of any difficulty in deciding what amount of deterioration should be held as demanding, or authorizing, restriction of liberty, that he considers also as equally unpractical and futile; and he points to the circumstance, that there is no form of ordinary insanity where the same difficulty is not ready to occur, the danger, besides, lying with the drunkard more in being too lax than in being too strict.

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[*] Christison: Medico-Legal Relations of Intemperance, 1861, p. 18.

[†] Let us take for an example the accurately and judiciously discriminating Clarus, who thus writes of dipsomania: "Dieser krankhafte Trieb ist jedoch noch keineswegs eine Krankheit der Seele." (Erkentniss und Beurtheilung zweifelhafter Seelenzustände, p. 128.) That Henke even (Abhandlungen aus dem Gebiete der gerichtlichen Medicin, zweite Aufl. p. 269), and Friedreich (Handbuch der Gerichtlichen Psychologie, p. 726), in their able discussions, by no means stretch their conclusions to so wide a limit as has been held justifiable by some in this country, is only another proof of what we have stated, with regard to the tendency of such questions to expand as they are contemplated, if once a certain barrier be overstepped. There are, indeed, no writers on the Continent who have advocated the idea of an insanity of drunkenness to the extreme with which it has been advocated by a few among ourselves; though many, in as far as they have gone, have seen better, and have laid down more distinctly, the consequences, personal and jurisprudential, which such a doctrine involves.

But we have scarcely here a genuine analogy. The conditions of sanity and insanity, in their ordinary acceptations, are, at any rate, opposite states, however closely they may run into each other; and there is rarely occasion to hesitate in arriving at a decision between them, though such occasions, when they do occur, naturally attract a serious attention. But the condition of habitual drunkenness is, in itself, an identical state: and to pronounce that this state is within the bounds of sanity in one man and of insanity in another, or is either in the same man at one time or in another, demands a very different and a far nicer distinction, if, without some added criterion, it be ever a possible one. Above all, we are far indeed from admitting this distinction to be a matter of indifference.* That radical cures are rare among the secluded, the respected author, to whom we have been referring, attributes, and of course justly, to the fact that the habit of intemperance is generally too far advanced before the individuals can be persuaded to submit to treatment. Thus, once more, all things considered, as the drunkard now found in the houses of seclusion is really the ordinary drunkard; as the vice and the disease, according to the most able advocates for the notion of an insanity, practically do not admit easily of being, and do not even require to be, discriminated; as the poor drunkard, if drunkenness be legally constituted an insanity, cannot, by the genius of our institutions, be neglected where the rich drunkard is guided into safety; and as an early resort to treatment is held to be the indispensable requisite for its success; it becomes manifest that the measure of seclusion for the intemperate is virtually made to include every drunkard in the country, and stretches towards him as either actually or potentially insane. And where this expansiveness in a jurisdiction exists, we repeat that its natural tendency leads it to be brought into action: so that, where the law has at first contemplated an occupation with merely open and flagrant facts, the authorities it constitutes find themselves speedily engaged in eliciting further facts by comprehensive and searching inquiries, and their sphere of duty widens in the degree that they grow familiar with its objects. We have said thus much to show how momentous is our topic, and how great is the demand about to be made upon society by those who maintain that the inveterate drunkard is insane, and to be, as such, formally and legally treated. Upon many important points we have not touched. We have not considered how justly the divine and moralist is startled at the contemplation of what may be the results of charging, not disease upon vice, but vice upon disease; or the anxiety of the civil lawyer, when he foreshadows the immense interests to be involved with regard to personal liberty, claims of rights, and of property, by the introduction of the idea of insanity; or the distrust of the criminal lawyer, when what he viewed as an offence, and a possible source of crime, demanding punishment, appears converted into an infirmity requiring protection; or the hesitation of many medical men, who are unwilling to relieve the police magistrate by what seems tantamount to admitting the management of a delinquency into their province, though ready as ever to treat its consequences when they appear as disease, in the same way as they would regard and treat those of any other description of vice or of excess. Even in the restricted view of the statesman and political economist, who fears a new and extensive source of national disbursement, it is a gigantic project that we have before us. Let us see, a little more narrowly, in how far it is justified by the conditions on which it is contemplated to be based.

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* Those who accept the various forms of *reasoning* or moral insanity, already feel themselves forced, by the implications the ideas necessitate, to admit varying degrees of civil capacity and criminal responsibility in the insane, Hence, although all justice in law is in proportion to its precision and certainty, they bring into the field two terrible sources of vagueness and uncertainty: first, by aggravating the difficulty of the old question as to who are the really insane; and second, and most perilous of all, by opening up a new question, as to what share, greater or lesser, of rights and liabilities is apportionable to the various modifications of an alleged unsoundness, as affecting the many relations of the individual to the family and to society. There can be no safety of legal action in the face of such intangible and shifting conditions as are here implied. To admit varying degrees of insanity, involving varying standards of civil position, and varying amounts of accountability, is to slip from the foundation of rational law into the unstable quicksand of surmise and conjecture, with the consequent risk of arbitrary procedure. Already the excitement of the chances of escape from danger, in a criminal trial, is scarcely more than equivalent in amount to that which gives animation to a hunting-field: and neither is without its charms and enticements. Increase these further, and, such is human nature, with the increased haphazards through the occasions of uncertain or misapplied justice, we have fresh incentives to crime. A man, an individual unity in mind as in body, is insane, or he is not insane. If insane, he is not responsible; if not insane, he is responsible.

We admit the difficulty of defining absolutely what is insanity; nor is it indispensably necessary for our present purpose that we should attempt it. We have no sure grounds as yet for connecting all mental aberration with some fixed and specific pathological change in the structure of the brain; nor, to pass beyond this, have we even any absolute right to assert, however confidently it has been asserted, that mental derangement cannot subsist while the brain retains its full integrity as a physical organization. That the brain is the instrument of the mind, renders it infallible, indeed, that, under the existence of certain deteriorations of construction (we by no means always know determinately what, for they vary immensely in character and in result), the power behind can neither receive pure intelligence from without, nor give ordinate form and force to its own internal movements or behests. It may even be said, and to a certain extent truly, that all bodily diseases are, more or less, mental diseases, because the mind is always more or less affected. But the converse of the main truth here by no means necessarily follows: that the mind cannot be diseased, and yet the great nervous centre remain comparatively, if not wholly, uninvolved. Unsoundness of belief, in matters belonging to belief, that is to say, in all matters beyond the reach of our own immediate and unobstructed experience, may conceivably concur with the most entire soundness of brain; and yet, from unsoundness of belief may spring the grossest unsoundness of reason and of conduct, or, in other words, insanity. In the same way, the most furious fit of insanity may pass away, leaving the reason in the possession of its finest attributes, and imposing upon us the unavoidable inference that, whatever may have been the proximate cause of the insanity, in relation to the cerebral structure, it was one at least of no fixed and determinate nature. Thus, the delirium of fever at one time, or that of exhaustion at another, more than reciprocally negative in their conditions, approximate to each other in their positive effects; while they fully concur in this, that either may be a transient form of aberration, passing away without a trace. There may be false perceptions, too, and therefore false reasonings, with the brain in perfect soundness, through false or imperfect impressions, leading to illusions, having been conducted to it by the medium of an action or condition, certainly anormal, but limited to the senses or the nerves. We do not admit, then, that to constitute the idea of insanity, we must hold as demonstrated, on precise and wholly *à posteriori* grounds, the inevitable co-existence of physical disease of the brain. That we must allow, on the other hand, for an exceedingly wide range of irrationality of belief or of conduct, yet not beyond the bounds of sanity, is very certain; otherwise, we fear the proportion of the world, whom we should be compelled to admit now, as in all former times, to be insane, would be indeed startling. We must have, in short, not only the absence or the neglect of reason, but, what is much more, the incapacity of reason, more or less complete and general, in order to constitute what can be justly termed insanity; and it is not required, perhaps, for the object before us, that we should offer any more rigid definition than is comprised in these simple terms.

Many, indeed, are the topics of human consideration, in which the easy proneness to a lax and popular use of language has raised, by a metonomy, what was originally conceived as a metaphor into undue currency and acceptance as the expression of a fact. And such, there is little reason to doubt, has been the history of the term dipsomania, with at least a great proportion of those who concede to it its present direct interpretation, though neither that which is etymologically, nor, we suspect, philosophically orthodox. We do forget that Platner, in his seldom-quoted treatise, 'De Amentia Vinolenta,'* published in 1809, holds clearly that the inveterate drunkard labours under a proper mental disorder; but with Salvatori,† who has also been remembered by few, and with Brühl-Cramer,‡ who is more usually referred to, and who appears to have wrongously stripped Salvatori, his fellow-townsman, of some of his laurels, it is a physical disease that is described and insisted upon as the condition of the confirmed drunkard; while the mind is only alleged or admitted to be implicated in so far as that here, as with every other form of bodily ailment, it must stoop to the conditions imposed upon it by the exigencies of the disorder. We cheerfully join in the praises of Salvatori by Dr. Christison, to the extent of according our admiration to the spirit and accuracy of his descriptions; while we must hesitate to assign to his little treatise, in its three divisions of thirty-seven pages in all, those more various and substantial merits which belong to the comprehensive works of Rösch,* of Huss,† and a few others. But Salvatori himself shows the merely metaphorical sense in which he applies to his disease of ebriosity the qualification of a mania, by distinctly reprehending those‡ who term it a disease of the mind rather than of the body, the affection of the mind, he insists, being only incidental to that of the body. It is thus that, while he does not hesitate to speak of an *oinomania*, he guards himself against adopting it as his selected designation, as well as against the notion of treating the disease as a true insanity; but some of those who have in so far followed him have not seen the necessity for a similar caution, while others have been inveigled into its neglect.

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* Quæstiones Medicinæ Forensis, p. 268.

† Commentatio pathologica de Ebriositate continua, remittente et intermittente, 1817; Comm. therapeutica, 1818; Additamentum, 1819: Comm. Soc. Phys. Med. apud Univ. Mosquensem, vol. ii. P. 2, 1821.

‡ Ueber die Trunksucht. Berlin, 1819.

It is, then, certainly nothing new in morals, or in the familiar interchange of society, that what we have entertained at first as a play or an efflervescence of the imagination, has grown with us insensibly into something with more the semblance of a reality. Nor is it easy, or even always desirable, for us to avoid, in common speech, that kind of exaggeration of phrase which gives force to our expressions, although not reducible, nor designed to be reduced, to philosophical accuracy and precision. The error is, when, familiarizing ourselves with the amplified or metaphorical phraseology we have currently adopted, we suffer at last our ideas to be chained to the literal signification of our language. Thus we say, forcibly, if not exactly, that a man is mad for music, for books, for versifying, or that he is a musomaniac, a bibliomaniac, or a metromaniac, when he so directs his life and habits that he gives to the pursuits connected with any of these an inordinate predominance, bending upon it all his thoughts, and only forsaking it with reluctance or difficulty for other common duties, or for the more intrinsic ones of family and society. M. Trélat even furnishes us (p. 171) with the history of a monomaniac, whose alleged peculiarity of insanity lay in a desire for carriage exercise; to which form of mental disorder, as he does not assign a title, we ourselves, yielding for the moment to the fashion of seeing specific differences in casual and non-essential variations, beg to attach that of *diphromania*, though we must not here forget that the individual in question, like all true monomaniacs, gave that evidence of mental obliquity or insanity otherwise, which would have well justified her being brought under a more extended, and yet a better defined category. We shall, indeed, only arrive at a just notion of what must be the real nature of a monomania by keeping in remembrance that the human mind, in every true and philosophical sense, can only subsist as a single and fundamental unity; and that, therefore, there can be no question of any other than an individual mental power. What we term its various faculties are merely forms of expression for the diversities of its external and internal relations, connecting it, mediately or immediately, with the world beyond. Internally, these relations are ideal; externally, they are real: but whether external or internal, a failure impairing the integrity of any of them necessarily reduces itself to a failure in the integrity of the mental oneness that is their common source or centre. Whenever a faculty, therefore, suffers, with whatever degree of intensity, whether it be a *facultas appetendi* or a *facultas judicandi*, it is but an exponent of a suffering mind, as working in one or other of its spheres of activity, whether sensory, intellectual, or rational.

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* Der Missbrauch geistiger Getränke. Tübingen, 1839.

† Alcoholismus Chronicus eller Chronisk Alkoholssjukdom. Stockholm, 1849, 1851.

‡ Op. cit., pp. 264, 295.

Perhaps, of all those wrapped up in vicious pursuits, no one is more justly entitled to the merely figurative and conventional appellation of a monomaniac than the gambler, who, flattered by occasional gains, and undeluded by more habitual losses, bends all his often remarkable though misdirected sagacity and energies to the course to which he has abandoned himself, and takes no other interest or care, in the midst of its cajolings and fascinations, than for the pursuit of that fortune which he sees is forsaking him; and which suffers him, often through criminality, and always, at last, through abasement and disgrace, to be drawn nearer and nearer to his ruin. Yet a Palmer would have had no success in pleading a mania for the dice or the racecourse as a justification, and but little in suggesting it as an excuse for the terrible descent in his career. Even the unthrift, as Sir Egerton Brydges has depicted him from his own experience, with the amplification of his debts, presents himself under similar fascinations. "The first involvement," he says, "multiplies at every move. It destroys the freedom of the intellect and the heart, and drives one into a state of mistiness which seeks extrication by the very means which augment it. It encourages self-delusions for the sake of momentary peace; and, like inebriety, buys oblivion at the expense of quickly succeeding pain and sickness." "Pecuniary embarrassment weakens and enchains the mind." "It was a sort of infatuation, which, having once been plunged into, I had not the courage to extricate myself from." Thus it is that the accomplished writer portrays his own feelings under a habitude. Figuratively, even he himself might not have objected to have termed his condition an insanity. Literally, he would have held it to be based upon an imprudence, and his creditors would attribute it to a fault.

It is especially, to pursue this consideration further, between the career of the gambler and that of the drinker that we discern many analogies: or where they differ, the variation, under our present point of view, is in favour of the latter, whose career, as it is the more naturally induced, is mentally the less anormal in itself, and therefore, to this very extent, the less allied to insanity. It is sufficient to retrace shortly the often described course of the drunkard, in order to arrive at this conclusion; divesting ourselves, meanwhile, of any prejudice attaching itself to the name of dipsomania, and considering the question in its isolation. Whatever be the modifications in the completed career of the drinker, few will deny that it is not at once that he rushes into the extreme of his excesses; or that it can be said as truly of him as of other offenders, that never did any mortal become suddenly thoroughly vicious. It is true that some writers on the subject have spoken of men of previously irreproachable morals, who, without apparent motive or cause, and with no prior interval of suspicion, have all at once, to the surprise of their friends and of the world, shown themselves in the guise of open drunkards; and they have advanced for these men the plea of insanity, as having impelled them on the instant, and thus in opposition to the "nemo repente fuit turpissimus" of the famous axiom of Juvenal, into a course utterly discordant with their former habits and principles. The character for talent and experience of some of those who have alleged such instances stands justly too high to admit of our questioning the accuracy of their statements: but the cases which they describe are unquestionably exceedingly rare; so rare, indeed, that they have escaped the notice of other observers, as we must admit they have eluded our own; or, possibly, they may have been by others differently viewed and interpreted. Assuredly we have seen men, and women too, whose addictedness to drinking, if not suddenly acquired, was at least suddenly discovered. A merchant of the highest character passed through life without a suspicion, and was only discovered by us to be a drinker during his last illness, when the habit and its results revealed themselves with unmistakeable distinctness. A lady who was also our patient, passed in her own family for hysterical and nervous, until it became our duty to give another and a truer explanation of her symptoms. We cannot, indeed, easily conceive a recourse to an indulgence in ardent spirits, without some previous consciousness of the kind of gratification that such an indulgence was fitted to produce; or, if it could have arisen on the instant from insanity, we should have expected and required those other tokens of mental disorder as its concomitant, that might have rendered the nature of the seizure less doubtful. Of the twenty examples already referred to, tabulated by us from among the chosen objects of seclusion and restraint, six only pretended to have rushed into their excesses rapidly; but they assigned a pretext for their error, and even of these we fear, as we have already hinted, that it is to stretch our confidence in their good faith beyond a prudent limit, if we admit more than that their previous degrees of familiarity with intoxicating liquors, in the form of a gradual initiation, had been somewhat less extended than it may have been with others.

Thus it is by the usual and natural course of a gradually increasing indulgence, that the drinker reaches that point when the vicious practices, which he had not been careful to keep undone, could no longer be kept unknown. The first sips might be from curiosity, from imitation, from conviviality, from the sense of temporary well-being they created; and they might be repeated for long, and by many always, with no consequences beyond. But with others, and especially with those who resorted to the stimulant for the sake of the luxury of its effects, and who thus desired to renew an enjoyment which they had taught themselves to appreciate, the course was one which naturally led them onwards. The excitement, like every other form of excitement when unduly solicited, left them, after its subsidence, at a lower standard than that from which they had proceeded. What wonder, then, if they had sought previously a condition of factitious well-being and hilarity for its own sake, that they should now, when there was a depression besides to lighten, not only revert to their former means for a renewal of their enjoyment, but find it necessary to resort to fuller and more frequent draughts in order to secure it? And so, by a series of inducements, than which none can be more simple, or in a certain sense more spontaneous, the practice adapts itself fitly to the motive; and the habit establishes itself with a twofold facility, both merely as a habit, and as creating the sense of the desirability, though assuredly not, at least as yet, the necessity, for its continuance. By and by a new order of events joins itself to the first more uncomplicated chain of causation, but with no change in the nature of the event. The excesses become deep enough to affect more substantially the health, after their agreeable effect is dissipated; and the sufferer, his state being still one of depression, again finds his relief in a repetition of the stimulant. Alternating back to soberness, the dejected victim of the *katzenjammer* once more feels uneasy qualms of nerves and stomach, as well as of conscience, which he still knows a return to his habitual potations will quiet for the while.

And if the relief sought be attained rapidly and certainly, as it really is, what is there unnatural in the mental condition that requires it? Man shrinks intuitively from suffering; and, with a present evil to be displaced by a present enjoyment, oblivion itself growing into an enjoyment, does not readily pause to balance against it a comparatively distant contingency. Did the pain instantly follow the stimulant, and precede the intoxication, we might justly suspect the soundness of the intellect of him who indulged in it; for a course so against nature would be against reason. But where the use of the stimulant has the obvious and real motive, that it removes a state of suffering and substitutes for it a condition of enjoyment, the act of drinking is consistent with the present circumstances of the drinker, and, in so far, is a rational one. We have not said that it is a prudent act; for that involves the consideration of a higher description of reason, such as man fails in throughout a multitude of his most momentous relations, beyond such as that now at issue, and yet without suggesting against himself the charge of insanity. He who defers his accountability has many companions, even in more inevitable matters than the results of drunkenness. What, indeed, to advert for an instant to the most serious of all considerations, is the whole life of the vast majority of mankind but a hankering after a present gratification, though it be but temporary, to the neglect of a real interest, though it be eternal? Yet death is far surer to all than ruin to the drunkard. Then are all insane? We have already hinted, besides, that it may be as truly said of every virtue that it has its own reward, as of every vice that it has its own punishment; and that, therefore, in the strictest sense, virtue is wisdom and vice folly. Even for the poorest man honesty is the best policy; and we may easily point to the career of the thief, however seducing to the adept its spirit of adventure and its transient successes, to show, independently of all abstract notion of right and wrong, how univerally complete is his final ruin and misery. But not for this is every thief insane. Just as little are we entitled to offer the plea of insanity for the drinker, because, for a present solace, he incurs, as he unquestionably does, the subsequent risk of an injury.

Some of those who admit the insanity of the drinker are yet inclined to restrict their idea of a dipsomaniac to those who, sober for more or less considerable intervals, burst out at the close of each into aggravated debauch, and only resume their previous condition of sobriety that it may be alternated once more with a like excess. This is evidently the notion of M. Trélat, as it was that previously held by Esquirol.* We fear, however, that, in nearly every instance, we have in this nothing more than a stage, though not an unfailing stage, in the ordinary career of intemperance. The individual who has been gradually advancing to the climax of a debauch, having at length reached it, can only maintain his orgies for a certain period, varying with his constitutional strength or peculiarities. The irregular hours, the interrupted sleep, the neglected meals, with the more direct action of the alcohol itself, sooner or later produce their effect on the nervous and digestive apparatus, and through them on the general system. The stomach of the drinker now rebels against every description of ingesta, and he rejects his spirituous liquors by vomiting, as well as the more fitting and real nutriment which his impaired digestion has now rendered him unable to assimilate, even if he could receive and retain it. He pauses, in this way, simply because he cannot proceed: the enjoyment has been abandoned when it had ceased to be an enjoyment, or had become impossible: he has yielded to a necessity: *lassatus, necdum satiatus, recessit.* It is now that, if he have been originally of fair principles, and have had any pretence to not a merely placid and so-called amiable, but to a loving disposition, or one in any degree honest for himself and self-sacrificing towards others, he will be taken to task by his conscience; and good resolutions, not inefficiently sustained for a while by the repugnance kept up by a nauseating stomach, will produce a period of usually entire abstinence. But habit, with him as with all, is only the expression of a natural law, by which every act, as it repeats itself, acquires intensity and power; with, besides, the tendency further to repeat itself, which a forcible interruption, howsoever caused, may suspend but does not annul. With his recovered health, a host of ready inducements, armed with more than all the strength of those that first misled him, and not the less efficient that he has no longer his self-respect and his consciousness of the respect of others to sustain him, tempt him to a breach of his not spontaneous forbearance. Possibly this might be intended to be only a slight one: but it has again disturbed the tenour of his life, the concords of which are once more broken; the old echoes are re-awakened; the chain of causation receives its former impulse; and stimulus leads to depression, and depression seeks stimulus, with the natural sequence that before attended it.

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* Des Maladies Mentales, tome ii. p. 74.

Still, what is there in all this, knowingly and determinately encountered, to justify the plea of a mental disorder? If drunkenness be an insanity, is a man to be accounted the more insane that, in the midst of his drinking, he sees his error, and desists from it for even but a transient period? Or is he insane because, having paused, he again commences: or, in other words, is he alike insane because he drinks and because he does not drink? Trace his career, however, a little further, and we find the irregular usually lapsing into the regular drinker, with no longer interruptions than those brief ones of the day that his necessities impose upon him. Salvatori admits that his intermitting and continuous ebriosity differ rather in degree than in quality, and such is the character of our own experience of the drunkard, while free. To recur to the twenty cases of secluded drunkards analysed by us; of these, fourteen were admitted to have been, from their first initiation to the habit, continuous drunkards; one had been an intermittent, and had become a continuous drunkard; and five might still be reckoned, though not in any strict sense, intermittent drunkards; but the history of these remains to be concluded. The intermittent drunkard, then, in so far as regards his drunkenness, cannot be regarded as more insane, if there be an insanity implicated, but is less insane than his more persistent comrade, whom, however, he is in the fair way ultimately to rival. And thus it is, and not otherwise, that as a habit utilized makes in the intellectual faculties a philosopher, in the moral qualities a philanthropist, or in the physical capabilities an athlete, so, from a habit misdirected, we have the gambler at his cards, the spendthrift at his shifts, or the thief at his devices; while, in the sensual appetites, it brings before us the gourmand at a feast, with the debauchee in the stews, to present us also with the drunkard at his orgies.

We have heard physicians of the highest intelligence maintain, that the very fact that a man, involved in the career of the drunkard, and fully conscious of the lamentable issues of that career, should yet be found to persist in it, stands as a sufficient proof of his insanity. But it is less remote issues than immediate consequences that rule the conduct of ordinary men; and "video meliora proboque, deteriora sequor," is no new adage; nor, however well it may describe what is sometimes the course of the drunkard, or of the slave of any other vicious habit, is either its language or its truth the language or the truth of the lunatic asylum. To assent to its applicability here, would be equivalent to adopting the somewhat singular conclusion, that the evidence of insanity lies in conduct pursued under the proved capacity of any one for the perfect comprehension of a subject, its relations, and its results. Others, fearing to define a *folie raisonnante* with so absolute a literalness, allege a form of insanity in which, not the perceptive or the reasoning faculties, but the will alone is implicated, which they seem thus to regard as something apart, or capable of being disassociated, from the judgment or the intelligence. The will in the insane drunkard, they tell us, is no longer free, but is placed under the irresistible control of a morbid appetite: or, as the terms have been converted, without, however, increasing their philosophical accuracy, he is the subject of a morbid, physical appetite, acting as an impulse, over which he possesses no moral power of restraint. The world loves that which has the imposing aspect of a general fact or deduction. But is this a true generalization, conveying a just *rationale* of the drunkard's condition; and, if it be, does it constitute it an insanity? Then, rigidly so considered, the indulgence of the thirst of the patient in fever, of the more unquenchable thirst of the sufferer from diabetes, or even of his aggravated sense of hunger, or of any other anormal craving, whether or not any of them be yielded to unstintingly, or, it may be, injuriously, becomes, in as far as it proceeds impulsively, the fair ground of a declaration of mental derangement. When our patient under asthma, or under heart-complaint, in one of his paroxysms of distress, peremptorily desires his window to be opened, or even rushes towards it, so as to inhale freely the dank and cold midnight air of November, in spite of our warning that the chill thus received upon the surface, or within the lungs, will lead to a congestion productive of far more mischief than will counterbalance the transient relief he invokes, we are not to ascribe his persistence to anything so simple as his earnest craving to be relieved from a present suffering, but we are to devise for it an implication of his soundness of mind. There is here, and we might give many analogous illustrations, a morbid, physical appetite which the individual cannot control; and, therefore, the consistent inference is, that there is insanity. And in many, if not in all, of such instances, the craving is really uncontrollable; while it is but an unjustifiable and a perilous begging of the question to allege this in and of the drunkard. It is the uneasy sensation in both that leads to the eagerness for a mode and means of relief, which may be contingently hurtful to the sick, and which is more certainly hurtful to the drunkard. But

> "suavia in præsentia
> Quæ essent prima habere, neque consulere in longitudinem"

is inconsiderateness and not insanity. Nor is it to deprive it of this character of inconsiderateness, but rather distinctly to confirm it, while it joins to it that of a vice, that the course, as in the drunkard, is followed knowingly, and to the manifest injury of an individual or of others, if the aims, that are at the last incommensurate with the results, be at first reconcileable with the motives.

Possibly it may be unnecessary, after this exposition, to pursue further the question of the implication of the will in the condition of the drunkard; but it is a knot which has been laid before us to untie, and we do not incline to prefer that it should be cut rather than loosened. Ay or No, says the sagacious Selden, never answered any question. It will not answer that of the relation of drunkenness to insanity. But, while we cannot proceed, or allow others to proceed, by a mere *saltus in concludendo*, our alternative shall certainly not be to entangle ourselves or our readers in any general discussion of the mysterious doctrines of necessity and free-will, with all their doubts and intricacies. Rather than pursue a course so theologically or transcendently metaphysical, it seems to us more consistent with the tenour of our present theme to keep in remembrance what Holberg has imagined of his subterranean people, whose rule, he tells us,* it was, that every one who ventured to dispute regarding God's essence and attributes, or regarding the nature of the rational spirit and of the soul, should be ordered to be bled and placed in the city hospital. Clearly here was in question, with the satirist, an implication of insanity. But if the drunkard be the unwilling thrall, or the puppet, of the circumstances under which he is placed, we are to recollect that this is merely what has been said by a large and flourishing class of metaphysicians regarding man everywhere; with whom every action of every individual is an effect which must have had its cause, that cause resting on the external or internal conditions to the influence of which the individual is subjected, and which have accumulated upon him or around him by the nature of things, independently of either his wishes or his opposition. Thus, with this kind of fatalism, in the general doctrine of which, however, in spite of its partial truth and plausibility, we must by no means be held as concurring, the will in the drunkard is neither more nor less the slave of his peculiar conditions than the will in the most abstemious of his fellow-mortals; and he is, therefore, on this score, as little the subject of insanity as any other of our race. His insanity, then, must be proved on other grounds. So, by a like consideration, if we have drinkers from the quasi-necessity of their position, we have thieves, through their loss of character having barred to them an honest course, and their perversion of habits having unfitted them to pursue it, far more obviously involved in a similar necessity. Have all our habitual thieves, therefore, on this ground also, ceased to be vicious, and become simply insane?

But further, in order to constitute an insanity of this class, it is an appetite, we are told, and not a motive, that must give the impulse that acts thus subversively of the will. Here, however, we have nothing more than an inaccurate mode of expressing the requirement, that the motive must have its basis in an appetite, and the intervention of the motive still being demanded, we acknowledge no essential distinction. There can be nothing in this, besides, to free us from what is the universal condition of our moral existence and of its responsibilities: that he who would hearken to his conscience must be deaf to his passions, and that he who succumbs to his passions must defy his conscience. Nor is there anything of a contrary quality in the idea that responsibility implies a power of choice, to act or to refrain, the obligation to resist resting on the ability to resist. The longing for ardent spirits cannot be termed an instinctive appetite, because it is one which is unknown to millions of human beings, and to the whole of the lower animals. It is an artificially created want, giving rise to an intelligent desire, and not to an impulse, or a mere animalism, like hunger or the sexual passion; the one of these universal heritages of all that moves being rendered indispensable for the preservation of the individual, and the other for the propagation of the species. There must be an understanding of the immediate results of the drinking of inebriants before there can be a yearning for the gratification; and the act is one of the volition and of intelligence, and not of a blind necessity, even if any course of action, although inexorably ruled under the latter supposition, were tantamount with an insanity, which it really is not. The very fact that the drinker reasons with himself, whether, while yet sober, he shall still abstain, or give way to the present temptation, proves his capacity of choice, and his consciousness of possessing it; and this power he retains till the liquor is within the limits of the pharynx, when the reflex action of the nerves of deglutition places the draught for the first time beyond his will. Thus, as he is cognizant of the issues of his conduct, and capable of answering as to its motives, he is liable to give that answer, and must be fully amenable to the judgment the direction of his volition entails upon him. Once, indeed, the conscience of the toper could scarcely, or could only partially, be arrayed against him, for he was taught to glory in his vice; but now, with our altered manners, this means of tranquillization hardly avails for him, and his responsibility is insomuch the less infringed upon. Volition, then, in the drunkard, being the direct offspring of his present feelings and circumstances, is not overruled or suppressed, but is rather, in reality, strong; for it has all the force that belongs to the near and the concrete over the remote and the abstract. But it is manifestly wrong to term this an impulse: for at no time is the will of the drunkard so completely under that subjection which is called an impulse to drink, but which we term a strongly motived desire, as it is, after a long debauch, and on the approach of an intermission, to the repugnance and nausea which constrain him not to drink. He knows, whatever he may allege or affect to believe, that he has a choice on the one occasion: he feels painfully that he has none on the other.

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* Neils Klims underjordiske Reise, C. vii. p. 260.

With a like force of conclusion as with regard to the will, reason, in the drunkard, not being annulled as in the insane, but simply disregarded, he is not insane. Many of the really insane, however, it will be urged, know the scope of their actions, and approach their accomplishment with premeditation and design. We shall not pause to insist that, were we to pursue this argument to the extreme it suggests to us, we might be constrained to conclude, that because, for example, the imbecile is sometimes a cunning thief, therefore the cunning thief is always an imbecile: not otherwise, or scarcely otherwise, than we have been required to admit that, because the insane man may be occasionally an unrestrainable drunkard, therefore the unrestrainable drunkard is always insane. But if the insane have occasionally this show of comprehension of their object, or of discretion in the selection of their methods and instruments, and it is inevitably, with regard to either, of a far less complete character than is usually alleged, they have no equivalent comprehension of their motive. The insane man, who plots a violence or a crime, is obviously not deprived of his moral liberty, in so far as he can comprehend its range. The more correct phrase is, that he is equally in possession of his free-will with any other individual; but he is not in the capacity of appreciating duly what are the relations in which he thus stands to his actions, in their origin and in their results. He is irresponsible, not because he was impelled to act, but because he was unfit to reason. And so, or nearly so, of the accountability of the child. The furious maniac, on the other hand, neither plans nor premeditates, though he too, at the instant, may be conscious of the nature of the action he perpetrates, or may show that narrow intelligence, the want of which would be a mere brutish imbecility, that enables him to resort to his fitting means. Yet to allege that the moral liberty is in abeyance, is, even with him, but a loose and unsatisfactory phraseology; for the truth is, that his will is free, rapid, and energetic, and it is only his power of direction that is really weakened, through that blending of hurry and incapacity which unfits him to pause, and to judge whether his motives be reasonable and adequate. In dementia, as there is annihilation of reason, there is annihilation of will, the one in its due relation to the other.

But none of this applies to the drunkard, who, as such, and while sober, labours under no enforced incapacity to reason, whether in premise, deduction, or sequence. That such men, as they tell us, if they are ever sane, are only sane when in an asylum, is not to the purpose, for this is never the character of insanity; though possibly it should only be understood as expressing by a hyperbole, what, in a qualified sense, no one will deny, that they continue insane while they continue drunk. Were the drinker to look for ardent spirits to descend like manna from heaven; or to believe, as we have known an instance, that some one near him possessed the power of changing, by a miracle, water into whisky, of which he was desirous to partake; or to anticipate from intoxicating liquors any action widely and preposterously differing from that which they are known to produce; or to drink, perhaps reluctantly otherwise, under the faith that he was commanded by an angel who had come down for the purpose, or that an unseen enemy stood ready to destroy him if he abstained; he might be reasonably judged insane, because, although the act was still consistent enough with his motives, the motives rested on a delusion: but when he merely seeks his draughts where and how they are ordinarily sought, to procure from them a great and immediate solace with the quality of which he has been long familiar, though he incur by this the risk of a subsequent evil, apparently disproportioned to the present benefit because it is vastly more intense, but really disproportioned to it in the inverse sense because it is at a greater distance, his conduct is at variance with that which characterizes insanity and which would serve it as a justification, and becomes blameable as a wilful imprudence which stamps it as a vice. As to the doctrine of an impulsion, in its more real sense, the very fact that a man believes himself impelled, though motiveless, to homicide or suicide, implies in itself an insane delusion, for the act itself, as it has no direct inducement, can promise no direct pleasure. The drinker cannot pretend to this belief, though he is now taught to foster it, for he knows that he drinks to obtain an often tried gratification. True, the inveterate sot is prone to use exaggerated phrases, and they are naturally to be expected from him; but it was not to be expected that grave thinkers should receive them as oracles. When he tells us, while yet sober, that he would drink were the fiend before him, or were hell opening at his feet, we have but a grosser specimen of that vapid rhetoric of which wiser men give us an example, when they declare their determination to make a journey "though it should rain old women and pikestaves," the contingency being anticipated as little in the one case as in the other. Not that he fails in perseverance and obstinacy in seeking his gratification, or that he is not perverse enough where he is thwarted by those whose authority he does not fear or does not recognise; yet his course proceeds, for the most part, with an even declivity, and is better characterized by the plain sense of Bishop Latimer, when he reminds us that, "certain it is that customable sinners have but small temptations." Easily led astray, he feigns a reluctance which, if it were as truly felt as it is occasionally loudly vociferated, would have amply sufficed to have enabled him to abstain.* We do not insist on the validity of the remark, though we think it worth introducing, that while women are considerably more liable than men to insanity, they are infinitely less addicted to intemperance. But is this wholly consistent with the notion of the influence of an insanity?

The drunkard, then, to be adjudged insane, must show some token of insanity, by delusion, hallucination, mental inconsequence, imbecility, or otherwise, beyond his proclivity to intemperate habits, and in whatever relation to them, as cause, as effect, or as simply coincident; and the criterion of his insanity must be sought to be decided accordingly, with an entire independence of the special propensity. We admit that the drunken man, as drunk, and while drunk, may be reasonably said to be always more or less of a maniac. Yet his actual fit of intoxication even, with all its affinity of resemblance to insanity, can be no further an insanity than that can be real which is factitious; or than the extrinsic can be the intrinsic; or the madness of an adventitious condition the madness of disease. To arrive at the conclusion which we believe to be the sound one, we have given only such meagre scope to metaphysical and ethical inquiry as the occasion seemed to demand from us, and as our space admitted. We know that there are many who turn from this description of discussion, to which, nevertheless, they may themselves have given the initiative, with a more than tacit acknowledgment that they are repelled by its difficulties: yet they should recollect that, if he that discusses without acting is a useless member of society, he that acts without discussing is a dangerous one; and that it is while they are maintaining the mind to be implicated, that they are content thus to cut the Gordian knot by declining all closer investigation, and so to reduce the whole question within little more than an assertion and a phrase, the adoption of which leads to the dilemma of either attempting impossible discriminations, or of diverging into vague and inconsistent conclusions. But we cannot separate inquiries into the operation of mind from the consideration of matters in relation to mind; and the points which we have rapidly surveyed are not those that we have selected ourselves, but those that we have been forced by others to admit into the question.

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* "Scio: tu coactus tua voluntate es."
Terence: *Andria*, act iv. sc.

We think that, in our consideration of these matters, we have shown that there is a vagueness and an inaccuracy that appertains to the application of any general idea of a subjugation of the will, and that the operation of the will is to attach itself neither necessarily nor inseparably to reason nor to passion, but, where these are at issue, to await the result of the contest between them, and become the instrument of the conqueror. It is to the quality of the reason, therefore, that all finally reduces itself. We have shown, in like manner, that the intemperate man, though he may do injustice to his reason, has not that direct want of capacity to reason which forms the essence of insanity. We have not denied that the insane man may be incidentally an inveterate drunkard: we have merely denied that the inveterate drunkard can be regarded as, *ipso facto*, necessarily or generally insane. We maintain that he is, on the contrary, with only a narrow minority of exceptions, each demanding its own specialities of proof, simply vicious, or sensual, or immoral, and that he is to be treated accordingly. Morals are the intelligent conception of the duties arising from the relations between man and man. That he who has no intelligence can have no morals, is, in its own sense, true; but assuredly the converse is not the truth, that he who has no morals has no intelligence. Never was great success achieved by a fiction; and, however amiable the design, or however excusable the recourse to a makeshift where a real difficulty may seem to present no other ready or effectual means of evasion, no great success can be achieved by it now. We may therefore resolutely adopt another course, and so proceeding, it will happily be allowable to dispense with all contemplation of the huge additional armament for the care of our lunatics which the legalized including of our myriads of drunkards under their category would inevitably entail upon us. Lastly, we think the nation may be thankful if it feel that it can arrive with us, as we believe it must and will, or already does, at the like conclusion.

Is there then no help for an evil which all admit to be so extensive, and which every one who is brought within its near contact feels so intensely? We should be sorry to admit this; for it would be, with us, to admit the inference that what we believe to be the just and the true course could prove the ineffectual one. That medical men should be willing to retreat from before the discussion of what they designate as a matter of mere speculation and subtle distinction may be an excusable remissness; but we maintain not the less that it is not excusable to hold the determination of the question, whether a man be mentally sane or insane, momentous as it is in all its relations, as simply a practical question of treatment and cure. In whichever way it may be decided, the means of appropriate treatment for either course remain open for recommendation and adoption; and these will not be the less efficient, that they do not perplex science by a fallacy, or wound our moral sense by an injustice. We insist that we are not entitled to describe the physical symptoms of a physical ailment which may be caused by excessive drinking, and then term it a mental disease. No one denies that drunkenness, as a habit, gives rise to functional or organic disorder: or that, to check the habit effectually, deprivation of stimulants must be resorted to, while the bodily ailments superinduced must be subjected to a treatment guided by the ordinary rules of our art. It is as little denied that means and opportunities ought to be afforded or obtainable somewhere for the fulfilment of these indications; nor is it to be forgotten to be urged, with a still more earnest vehemence, that efforts at prevention are here far more becoming the wisdom and dignity of a nation than mere palliations or efforts at cure. As to the choice of the methods of treatment, when we are gravely told that it is the cure alone that lies with us, and that we have no concern, metaphysically, theologically, or legally, with the consequences of the pretext under which we attempt it, we must again demur to a proposition so opposed alike to the ethics and to the science of healing. But we must proceed further: for the reality is, that this is a matter in which we cannot, if we would, divest ourselves of responsibility, or separate our actions from their results; and it is one in which our relations to the law will include us, and will direct us, whether we choose or not. It may be true, in the very limited sense of that truth, that whether we have to deal with an appetite, a habit, a disease, or a mania, the practical issue is the same, and the cure is seclusion. But the place and mode of seclusion is not indifferent. If it is to be in a lunatic asylum, we must discriminate the conditions, and prove the lunacy, and to this the law will justly hold us.

Let us look first at what is the present state of the law regarding the drinker, in as far as relates to the existing means of restraining him. We believe we must go back, in England, to the period of James I., in order to refer to any special statute law directed against drunkenness. By an Act passed in the twenty-first year of that ruler, drunkenness was punishable for a first offence with a penalty of five shillings, or with six hours in the stocks; while for a second offence, the delinquent might be further bound in a recognizance of ten pounds, with two sureties for future good behaviour. In Scotland, in terms of an Act of the twenty-second Parliament of the same monarch, all persons convicted of drunkenness, or haunting of taverns after ten o'clock at night, were to pay for the first fault three pounds Scots, to be applied to pious and necessary uses, or they were to be placed in the stocks or in gaol for six hours; and for the second fault, they were to be amerced in five pounds Scots, or to have twelve hours of the stocks or prison. The double of this latter penalty was enforced should the offence be repeated a third time; and the offenders, in case of further transgression, were to be placed in gaol, and to be detained there till they found security for their better conduct. The Church in Scotland appears to have earlier assumed an authority even more severe and arbitrary than this; of which, among many other examples, we may cite that from the register of the parish of Monifieth,* where we find it recorded that in February, 1563, a culprit, convicted of "ye presumful abus and vyc of drukinness," was sentenced by the Session to be "brankit, stockit, dukit, and banisit ye heile paris." All former acts of the State regarding drunkenness, that of James included, were ratified in the first Scottish Parliament of Charles II., with a regulation of the graduation of the penalties of from twenty pounds for the nobleman to twenty shillings for the servant, while an offending clergyman was to be mulcted in a fifth of his stipend.

Little, however, was to be hoped from enactments made under the sway of monarchs who but symbolized the manners of the times, when the one described himself as in "the castell of Croneburg, quhaire we are drinking and dryuing our in the auld maner;" and when the other found no better means of portraying the character of his nephew, Prince George of Denmark, than to say of him, that he had tried George sober, and he had tried him drunk, and, drunk or sober, there was nothing in him. The law is doubly a mockery where it has at once no foundation in the sincerity of the lawgiver, and where those who are to administer it share largely in the fault of those whom it condemns. Nor could it grow into efficiency, even with a better state of manners, so long as society remained inclined to judge leniently that which it had recently applauded warmly. Besides, it is the tendency of severity, wherever unduly proportioned to prevailing sympathies, to provoke opposition rather than win obedience. It was natural, therefore, that ordinances so rigorous should fall into abeyance, and that it should have become gradually the usual rule of the law, as it virtually remains now, that no one should be punished for mere drunkenness, even in public, unless he joined with it a riotous or indecent conduct. The drunken individual, otherwise, is merely taken into custody for his own protection. But men's minds have grown uneasy under the inheritance of ideas in the prevalent assent to which they no longer participate; and hence a striving after some means of remedy which they cannot find in the actual condition of the law, while it seems still impossible, if it would be advantageous, to revive the now obsolete, and never efficient, enactments of the seventeenth century, or of the times before it. It is, therefore, in search of something beyond the law, to attain what they justly believe to be an object of vital interest under the law, that they have resorted to the new pretext of an insanity, and that the seclusion of the lunatic asylum has been sought to be enforced as a substitute for the discipline of the mulct and the gaol. Not that this resort has received any universal approval. On the contrary, there are, as we have already pointed out, many thinking men within the profession, as there are myriads beyond it, who are as little satisfied as ourselves with what they regard as a mere evasion of a difficulty, which it would be better to encounter downrightly and in its real aspect.

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* Stat. Acc. of Scotland, vol. xi. p. 543.

But an intermediate provision, between the lunatic asylum and the gaol, has, it is well known, for long existed in this and in other countries, in the shape of places of voluntary retreat, into which the intemperate drinker, either at his own motion or at the instigation of his friends, has been induced to retire, with the view of entirely separating himself from the means of indulging his propensity, the choice being rather to rely for success on difficulties interposed by others than on the vigilance of his own conscience. Sometimes retreats of this description have been merely a temporary residence in the house of some individual, where only one or two inmates were received, and where there was little advantage beyond the breaking-up of old associations, and little restraint besides that of the generally remote locality selected, the secludedness of which rendered access to the ordinary temptations of the drinker less easily attainable. Sometimes, on the other hand, the retreats, instead of being restricted to this limited and private character, rose more into that of a specially constituted establishment, having for its immediate object the reception and cure of the drunkard. Of such establishments, none of which are as yet on so extensive a scale as to prove that they are considered to supply adequately a generally recognised want, we believe that one or two now exist in England, and a few also in Scotland, with the latter of which we chance to be the more familiarly acquainted. Of the two principal establishments in Scotland known to us, the one is in the little county of Clackmannan, where it has existed for about ten years, and with a number of inmates, of either sex, usually ranging at about twelve. It has not hitherto boasted of many cures, though the proprietor, who is not a medical man, pleads that they can be effected if the will be exerted: by which we are of course to understand, if it be exerted in the proper direction. Some of the inmates have resided there for years. The other establishment, which, unlike the former, is the property and under the superintendence of a medical practitioner, is in the island of Skye, where it has recently resumed its function after a partial interruption, and is for males exclusively. The terms in both are about a hundred guineas yearly, so that it is manifestly only those who have a moderate independence who can take advantage of them. An establishment of a widely divergent description is the so-called House of Refuge in Edinburgh, which, in addition to its leading design as a place of shelter and relief for the poor, receives into its care a number of intemperate drinkers of both sexes, and of different classes, though nearly exclusively of those approaching to that inferior grade of society, which, we have already insisted, is not to be excluded from the benefit of such arrangements, and least of all, if the admission be henceforward to be given under the ascription of insanity. The scale of charges in this establishment is graduated according to the more humble means of the applicants, but all are treated judiciously and kindly; while the general management, under a committee of the principal judges, magistrates, and resident gentry and citizens, many of whom intervene actively, along with the more immediate superintendence of an experienced medical attendant, who can always command the aid of one of the leading physicians of the city appointed as his consultant, leaves nothing to be desired that is likely to be amended under any other arrangement.

It is, however, across the Atlantic, and to Binghampton, a town of about ten thousand inhabitants in the State of New York, that we must pass, in order to meet with the most remarkable example extant of an asylum for the drunken. We chance to have before us an account of the laying of the corner-stone of the building designed for this purpose, which took place in 1858; as well as a drawing of the edifice in its completed condition. Dr. I. Edward Turner, of New York, whose energy and philanthropy have rendered him the main instrument in its foundation, does not appear to consider the propensity to inebriety as in itself an actual insanity, though he regards the habit as a prolific cause of insanity, both in the individual and in the offspring. He views it rather as a constitutional disease, of which, like Salvatori and Brühl-Cramer, he notes a certain morbid condition of stomach as a principal and peculiar feature. For the treatment of this disease, the lunatic asylum and the hospital, it is averred, had alike proved inefficient, and a special institution had become necessary. The able and venerable Dr. John Francis, in an address delivered on the occasion, stated beyond this, that in the United States the lunatic asylums are, like other establishments, beginning to reject the class of inmates in question, having repeatedly closed their doors against them as incompatible with their proper design. Dr. Turner, in urging the real nature of the necessity thus created, states that he is acquainted with many men who had occupied high positions in the Church, the State, and in the legal and medical professions, who had ceased all exercise of self-control through this malady; and that he had also the names of more than four hundred women, most of them educated and accomplished, and in the highest walks of life, all of whom are similarly infected. The institution is erected by a body of subscribers and shareholders, and is directed by a board of trustees. After raising a fund of more than fifty thousand dollars, the board, through their corresponding secretary, addressed a reclamation to the State legislature, suggesting, though we suspect with little success, that a half of the revenues arising from the Excise law should be appropriated to the maintenance of the asylum, on the plea that, if the State sanctions the traffic in the source of inebriety, it should also protect society from the outrages of the inebriate. The site of the establishment comprises 250 acres of land, in a pleasing locality, presented by the citizens of Binghampton for the purpose to which it has been devoted. The design of the building is commensurate in grandeur with the ample demesne within which it has been placed. It may be shortly described as a structure of stone and brick, 365 feet in length, in the Tudor style, three stories high, and with numerous massive embattled towers, the whole executed in a manner which, to judge from the details, can scarcely be termed less than magnificent. The asylum, though not, as we have seen, without its aspirations towards a State endowment, is designed to be a self-supporting institution, in which the rich will be required to pay according to the quality of their accommodation, but the poor are to be admitted without charge. The treatment is to be tonic, in connexion with a total abstinence from intoxicating liquors. In a work* published in 1861, the edifice appears to be spoken of as completed. Already applications for admission are said to have been made by thousands, and other similar institutions are reported to be contemplated in other States. Our retreats for the drunkard in this country are indeed insignificant beside this American palace of the dupes of Circe, remarkable at once for its destination and for the extent and sumptuousness of its plan.

Though a feeling of melancholy starts upon us when we contrast the splendours of such an edifice as this with the humiliating associations connected with its objects, it is still impossible to doubt that such an institution, zealously and judiciously conducted, must render valuable services, though, in too many instances, the benefits likely to accrue from it may be merely of a negative quality. The change of scene, the breaking-up of former opportunities, the removal from old haunts and companionships, the consciousness of an ever near authority that may be little displayed but that must be respected, the calming effects of the rural and other gently varied occupations, the regular hours, the bracing air and exercise, the insinuation of new objects of concern, the absence of a distressing sense of inferiority to the rest of the community, the fellowship in aims and hopes and interests, must all tend, first to subdue, and then to soothe, to invigorate, and to encourage. In union with this, where the bodily health has suffered, if it have not suffered irremediably, the means to be adopted, through medical treatment, to restore it from its depressed condition to its original elasticity, may be hoped to exert an important influence. Ere long, the chastened spirit may be expected to listen calmly to the warnings of those in charge, whose office it will be more to open up and enliven the prospects for the future than to reprove for the past. Thus, as the chain of former habits is gradually unlinked, new habits will be sought to be knit, more consistent with genuine individual happiness and with the claims of social duty. And when the individual quits the precincts of such an establishment, for the efficiency of which we have imagined no easy function, to be performed cursorily or at random, it will be with no painful reminiscences of the loud despair and the mournful scenes of deprivation, which, gloss them as we may, are the inevitable adjuncts of the ordinary lunatic asylum; and it will be with no sense of wrong and injustice that he should have been mixed among sufferers, to whose state his own, however melancholy, he was conscious bore no real resemblance. If he is to retain possession at his exit of the moral and physical conquest that it has been hoped to achieve for him, it must be through the feeling that he is at peace with his home circle, not less than at peace with himself. Any rankling bitterness of recollection that might survive would but remain as a germ, ready to spring up into new mischief on every occasion that favoured it, and be converted into a pretext or an incitement towards new excess.

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* Barber and Howe: Past and Present of the United States, 1861, vol. i. p. 453.

Thus the retreat of the drunkard, if it be to have the fullest prospects of benefit for him, must have been like that monastic seclusion to which diffident men resort, less to expiate faults than to learn to practise piety and wisdom, and to escape the temptations and follies of the world; and like that, it should be the offspring of a voluntary abnegation. But unlike the inhabitant of the monastery, the recluse must be admitted to rejoin the world, when again purified and strengthened enough to sustain its trials. Yet all is not encouraging in the prospect of advantage from this kind of seclusion. With some, no real reformation having been effected, the habit will have been stayed only because its gratification has been obstructed, and a relapse will follow close upon liberation. With others, and especially with the periodic drinker, it will be found that there are many who, however occasionally gross their debauches, have been yet the sole sustainers of their household: sometimes by their own actual exertions during their intervals of sobriety; and sometimes by their merely nominal position, while the weal of the family was maintained through the management of all business affairs by subordinates. In either case, it is possible that a protracted seclusion might at once prove ruinous, where ruin might have been deferred. The measure, therefore, as a spontaneous measure, should not be recommended or resorted to, unless with the concurrence of those the nearest interested; for if there be a difficulty at one time in inducing kindred to take a decided step of this description, there may be hazard in urging it, or in facilitating it, at another.

Such institutions, then, must continue to be, as they have hitherto been, in the main voluntary institutions, erected by benevolent associations or by private enterprise, and proportioned in extent and degree of costliness, like every other undertaking, to the want which shall have been shown to be felt for them on the part of the community. The design must expand as the appreciation of its utility declares itself. It cannot be forced into amplitude; but its extension will follow, and should be encouraged, in the measure that its advantages can be demonstrated. But they may be, in so far, institutions acknowledged by law; inasmuch as, while our jurisprudence has not considered it prudent to admit intoxication as lessening the accountability for any crime or offence committed under its influence, however little be the doubt that the actual guilt is palliated in the eye of the common intelligence, there ought, on this ground, to be the more anxiety to check the fault of drunkenness itself wherever publicly offensive and flagrant; and this, so soon as our growing civilization has thoroughly depoetized the vice, by diminishing our sympathies for the excesses of our forefathers, and blunting our memory of them, should be sought to be accomplished by the infliction of those penalties which were only inoperative before, because the magnitude of the offence was unseen through the blinding effects of a long inheritance of prejudice. But the magistrate might be authorized to permit an alternative between submission to an enforced fine for drunkenness, with imprisonment in the common gaol and at the public charges for a shorter period, and a voluntary subjection to the curative discipline of the retreat for the drunkard, in its proper sense of a reformatory, to be undergone at the proper cost of the offender for a considerably longer period; the duration of the seclusion, however, as of the incarceration, being beyond the option of the offender, and being extended in the measure of the frequency with which the openly repeated offence has been repeated. Naturally, the civil authority might be considered to have a right to superintend the management of an institution in which a vicarious sentence of this kind was under consummation: yet it is a right which it would be possibly prudent for it to pretermit, or to use forbearingly, unless under rare exceptions; contenting itself with exacting security that the stipulated time of probation should be fulfilled, and remembering that the institution is destined to be no place of durance for common crime. As to any further step, where notoriously extreme debasement demanded extreme proceedings, as the law finds no difficulty in assigning the estates of a debtor to trustees, that they may be guarded for the behoof of creditors, it seems a reasonable, and may often be a necessary, part of the control to be exercised, that there should be used a power of similarly assigning the property of a prodigal, as once provided by the Roman law,[*] that it may be permanently protected by trustees for the behoof of his family. But the legal executive would have no right, at least none that it would be expedient to assert, to interfere with insobriety that was not indecently flagrant, or to pry into merely private and personal failures. Meanwhile, the doors of the retreat would be open to the spontaneously penitent, to enter or to withdraw at pleasure; and the management of each establishment would be safe, because its credit with the public would rest upon its proper merits, and on the honest and effectual discharge of its functions. Were the institution to be mainly founded on an effort at benevolence, like that at Binghampton, it might aim, like it, at obtaining a surplus of revenue, to be expended on the reception of the poorer drunkard.

[*] Ulpian is nicer in his distinctions than our modern believers in a drunken insanity: "Sed solent hodie Prætores vel Præsides, si talem hominem invenerint, qui neque tempus neque finem expensarum habet, sed bona sua dilacerando et dissipando profudit, curatorem ei dare *exemplo furiosi:* et tamdiu erunt ambo in curatione, quamdiu vel furiosus *sanitatem*, vel ille *sanos mores* receperit."—Corp. Juris Civ.: Digestor., lib. xxvii. tit. x. § 1.

We want, then, reformatories, and not asylums, to meet this stupendous evil; and we desire these to be, to the extent we have imagined, under the sanction of the law and utilized by the law, while superintended, where superintendence is necessary, by the ordinary, and here appropriate, civil magistrate. Above all, it is prevention as much as cure, or rather than cure, that is chiefly wanted. But if the time be not ripe for such a disposition of the law, we must still await the arrival of an amount of conviction on the part of the public of its utility and justice, which will then be received as only another term for its necessity. Neither need we await this time with our hands folded, for we are not helpless now, nor are all other sources of hope shut out from us. If we seek a title to be convinced of the immense change which has already taken place in the general estimation of the worth of drinking habits and practices, and if we thence justly augur something still better for the future, we may proceed also upon no solitary source of confidence, for we are aware of many other momentous improvements which the growth of enlightenment has prevailed in securing for us, most of which must have been at one time as unanticipated by our progenitors as they are now invaluable to ourselves. The history is not yet old when Christians, professing a religion of love and charity, in the name of that love and charity burned their fellow-Christians. Down to still a recent time, all the piety and knowledge, of nearly all countries, did not suffice to prevent the most pious and the most enlightened from holding fast to a belief in witchcraft, to which they offered up innumerable victims by fire: of which sad examples occur to us in the executions at Würzburg, from 1627 to 1629, where 157 so-called witches suffered, and in those at Bamberg in 1659, where the cruel destruction reached the incredible number of a thousand; a truer and more atrocious pyromania by far, on the part of the judges, with regard to their fellow-men, than has ever existed since on the part of the alleged insane, and with reference to houses and farmyards. Almost in our own day, it was the universal rule of society that every one should be prepared to allow a hasty word to involve the hazard of his life; and the virtual homicidal mania of the duel, among the best and the noblest, had its murders, and made its widows and orphans, under the name of honour.

A like sanguinary spirit, as cruel and as senseless, gave, to pursue our instances, for long such a rigour to our laws, that the punishment of death was awarded indifferently to the most trivial and to the most heinous offences, with slight effect in repressing either; and we have ourselves conversed with an individual who, about 1784, saw fifteen men executed at once before Newgate (probably the same execution as that alluded to by Boswell in his 'Life of Johnson'), while Sydney Smith, so late as 1813, asks a person to whom he is writing to "conceive the horror of fourteen men hung yesterday." Nor was this exhibition one, the melancholy inconsiderateness of which was only rarely exceeded. It is scarcely less recent that the palaces of our nobility, and of royalty itself, were used as gaming-houses, where statesmen were not ashamed to risk fortunes upon a die, and smiling ladies and gentlemen addressed themselves to work each other's ruin at cards. And if, with our better knowledge, and our more humanized manners, the fruits of our advancing civilization, we have changed all this, shall we not hope also our favourable change, as the time recedes when ardent spirits, ranking almost as a panacea, were judged supremely fitted to strengthen and to ease toil, to save from the effects of heat and cold, to repel the risk of infection, and to cure many forms of actual disease; and were esteemed besides the best of all means to cheer our gloomy hours and to adorn our happy ones? It became no wonder, then, that they were so lavished, that, as moderation was reckoned a mean and foolish asceticism, so conviviality was accounted a virtue, strength of endurance a triumph, and the want of it a provocation to contempt, while intemperance a laudable jollity, or, at the most, an amiable failing. The drunken excess which consistently followed on the prevalence of such notions, the fertile origin of an unceasing medley of drinking customs, was not more an insanity than religious persecution, or superstition, or rigour in our judges, or the point of honour, or gaming, each more unwise than the other, was an insanity. It was rather, like them, a prone and natural sequence, belonging to our manners, and preserviug ever a due proportion to its cause. Already, as the influence of that cause sensibly declines among the higher classes, the intensity of the result as notably diminishes; nor have the lower classes, happily, failed to participate largely in the improvement.

Drunkenness, then, is but a trait left to us of a lower civilization. High civilization is not mere literary knowledge, or refinement of manners, or purity of morals. Any of these may exist separately, or with but little support from the others. It is the harmonious junction of all the three. As we acknowledge this the more, and as its realization advances the more among us, its boons will become extended. Let us strive towards these, in the issues before us, not by giving vogue to a fallacy, but by inculcating detestation of a vice. If we proceed otherwise, we shall have no enduring success, and we shall deserve none.

REVIEW II.

A Treatise on the Surgical Diseases of the Eye. By H. HAYNES WALTON, Surgeon to the Central London Ophthalmic Hospital, &c. &c. —*London*, 1861. 8vo, pp. 686.

THIS is a new edition of a work by Mr. Walton, published in 1852, under the title of 'Operative Ophthalmic Surgery;' which was received by the profession as a good practical introduction to the study of those eye-diseases which more immediately call for surgical interference. In the truest sense it is a new edition. The history of ophthalmic surgery, which formerly occupied forty-six pages, has been omitted. Many of the chapters have been thoroughly revised, or recast. Those on the use of chloroform in ophthalmic surgery, on affections of the excreting lacrymal organs, on strabismus, and on incision of the conjunctiva in purulent ophthalmia, have been re-written; while entirely new chapters have been added, on the use of the eye-douche, on sympathetic inflammation of the eyeball, on orbital and intra-cranial aneurisms, on iridectomy and other operations for the cure of glaucoma, and on the ophthalmoscope.

We purpose shortly to direct attention to some portions of Mr. Walton's work, bearing on subjects of great practical interest, on which new light has been shed by recent investigation, or on which our author has deemed it well to change from the views adopted in his first edition.

Our readers must be aware that, while every other means is in general found fruitless, two methods of treatment are available in cases of that insidious and very serious internal inflammation of one eye which results from previous injury of the other, and which has received the name of SYMPATHETIC OPHTHALMITIS. The one is free incision of the injured eye, with removal of a portion of its cornea, so as to give exit to any foreign body which may be concealed within its tunics; and the other, extirpation of the injured eyeball. On this important point of practice Mr. Walton expresses himself as follows:

> "No general treatment, no local application, no dietary system, is of avail in checking unequivocal sympathetic ophthalmitis. Nothing of the kind can be depended on; and while I thus speak from my own observation, I endorse the statement of all trustworthy observers. The affection can be stopped, or subdued, only by surgical treatment. A portion of the eyeball must be removed, whereby the products which have set up the irritation, or the cretaceous or ossified tissue which has acted as a foreign body, may be got rid of, or extirpation resorted to. When done early, this practice works wonders. If adopted before the sympathetic action has induced palpable structural changes, it will be all-effectual. At later stages, it may arrest progress, and stay the destruction. Even when the pupil has become adherent to the capsule of the lens, and the iris dull, I have seen a check. . . . . .
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> "Removing a portion of the eyeball will generally suffice, as it is frequently in the anterior part of the eye that the centre of the morbid action is seated. I have very frequently found the vitreous humour healthy; this portion of the eye, therefore, not being spoiled. With the reduction of the eyeball only, the deformity is very much less, and the case is better fitted for an artificial eye; and, in the early years of life, the destined growth of the orbit is less interfered with.
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> "When the entire eyeball is disorganized, posteriorly as well as anteriorly, especially when there is general enlargement, extirpation is the course to be adopted." (p. 115.)
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> "Extirpation of the eyeball within the ocular sheath . . . . is what would be called a more brilliant proceeding than abscission. . . . . Yet I am quite sure that, if the patient's ultimate welfare be considered, its adoption should be the rare exception. Even a button of collapsed tissues is far better than none, and a slightly reduced eyeball is vastly superior to an empty orbit; and I think it better that these should be secured, if it be even at the expense of longer time.
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> "But will abscission confer advantages equally lasting with extirpation, is a question likely to arise in the mind of the practical man. Answering from my own experience, I say, Yes. In no cases in which I have selected it as the proper operation have I been disappointed." (p. 116.)

The latter part of Chapter IX. and the whole of Chapter X. are devoted to PULSATING DISEASE in the ORBIT (till lately deemed aneurysmal), a subject of great interest; on the pathology of which new and important light has been breaking in, while much remains to be investigated.

Most of our readers are doubtless acquainted with the cases recorded by Mr. Travers in the second, and by Mr. Dalrymple in the sixth volume of the 'Medico-Chirurgical Transactions,' which were regarded by these gentlemen as instances of aneurysm by anastomosis in the orbit, and which were cured by tying the common carotid. The possibility of an aneurysm by anastomosis in the orbit is not denied; indeed, a case in an infant of two months, under Mr. Walton's care, cured by ligature of the carotid, goes to establish the fact of its occurring as a congenital affection. Still, it has been distinctly shown, by recent observers, that all the symptoms once regarded as diagnostic of aneurysm by anastomosis in the orbit—such as the sensation of a sudden snap in the head, protrusion and tension of the eyeball, swelling of the eyelids, varicosity of the conjunctiva, throbbing of the orbital bloodvessels sensible to touch and sight, pain in the region of the orbit, whizzing noise in the head synchronous with the pulse, obscurity of vision, aneurysmal *bruit* heard on application of the stethoscope, diminution of the exophthalmos on pressing back the eye, and cessation of the throbbing of the vessels by pressure over the common carotid in the neck—may arise from causes in which aneurysm by anastomosis, or aneurysm of any kind, has no part.

The facts and reasonings on which this important conclusion is founded are given in a condensed form by Mr. Walton, while reference is made to most of the original authors whose observations and dissections have led to the change from the doctrine of Travers and Dalrymple. This change the profession owes in a considerable measure to Mr. Nunneley, who, referring to the cases of those gentlemen, makes the following statement in a paper in the forty-second volume of the 'Medico-Chirurgical Transactions':

> "In hardly one particular do these cases of disease in the orbit resemble aneurysm by dilatation, or enlargement of the small bloodvessels in any other part of the body. 1st. It is very doubtful if aneurysm by anastomosis is ever developed unless it has a congenital origin. 2nd. Aneurysm by anastomosis does not appear suddenly; and when it is noticed, its increase is usually slow and gradual. 3rd. It is not caused by direct violence. 4th. All the bloodvessels in the neighbourhood of aneurysm by anastomosis appear to participate more or less in the increased action, as active agents, and not merely as passively dilated tubes. 5th. It is almost always, if not invariably, connected with the cutaneous or subcutaneous tissues. 6th. The result, where a single large distant artery has been tied in aneurysm by anastomosis, is not such as to lead to the supposition that all pulsation and tumefaction would instantly disappear on ligature of the carotid, if such a disease existed in the orbit; though a cure might follow, the effect would be gradual."

Taking the whole that has been recorded into account, it seems to be established that exophthalmos with orbital pulsation, and *bruit de forge*, may arise from the following causes:

1. Congenital aneurysm by anastomosis. (Walton.)

2. True aneurysm of the ophthalmic artery; either spontaneous (Guthrie), or traumatic (Busk.)

3. Dilatation of the carotid where it issues from its canal in the temporal bone, with coagula around and within the dilated part, and an atheromatous and dilated state of the ophthalmic artery. (Nunneley.)

4. Inflammation of the cavernous and other sinuses of the dura mater, with coagula in their canals, and varicose enlargement of the ophthalmic vein. (Hulke.)

Which of these causes occurs most frequently, remains to be shewn by future observation. We shall not be surprised if it turns out to be the last-mentioned; namely, obstruction to the return of the blood from the orbit through one or other of the veins or sinuses; a state of matters which will necessarily give rise to an increased quantity of blood in the orbit, an augmented pulsatory movement of the branches of the ophthalmic artery, and a thrusting forwards of the eyeball and eyelids.

"It will be an interesting subject for future investigation," observes Mr. Walton, "to ascertain the signs by which intra-cranial and orbital aneurysms may be distinguished from each other, and from obstruction to the return of blood by the ophthalmic vein. It seems probable that, in the cases which most closely simulate aneurysm by anastomosis—as when the pulsation seems to spread beyond the margin of the orbit, or where it reappears after ligature of the carotid—the causes will be found to be venous obstructions, either from pressure on the ophthalmic vein or from disease in the cavernous sinus; and when the affection has a traumatic origin from blows on the head, it is more likely to be the carotid artery that is injured when in close relation with the bone in the cavernous sinus, than the ophthalmic, lodged in the soft tissues of the orbit, so that I should expect the intra-cranial origin of orbital pulsation to be by far the most frequent." (p. 238.)

Out of fourteen cases referred to by Mr. Walton, in which ligature of the carotid was performed, a cure appears to have been obtained in twelve. A fatal result followed the operation in two cases only; in one, from hæmorrhage; in another, from continued inflammation of the dura mater.

An important fact, not noticed by Mr. Walton, is, that a spontaneous cure has sometimes taken place. An instance of this, related by Dr. Fiske, of St. John's, New Brunswick, will be found in the 'Dublin Medical Press' for August 24, 1859, copied from the 'New York Journal of Medicine.' The patient was a female, aged forty; the disease, which affected the left orbit, began with the feeling of a sudden snap, followed by immediate protrusion of the globe, œdematous swelling of the lids, and loss of vision on the affected side; after three weeks a small pulsating tumour appeared under the superciliary ridge; it soon increased, and became so distinct as to be seen at several yards' distance. The patient had a constant whizzing noise in the ear. Pressure on the common carotid immediately arrested the pulsation of the tumour. The patient declined to have the artery tied. By and bye, some favourable changes took place; the angular vein, which had been very turgid, became collapsed, and there was less thrill in the tumour. During the next four years, there was a slight increase in its size; but nothing of importance occurred, till Dr. Fiske was called one day in great haste to see the patient, as she was very ill. He found the pulsation had entirely ceased, and the distended veins had collapsed. The pain in the head, which was great, was relieved in a few hours, the whizzing sound ceased altogether, the pulsation never returned, and in the course of three months the tumour entirely wasted away. What for six years had been a great deformity, and a cause of much distress, had in a very short time, been entirely obviated, without the interference of art. The eye was restored to its natural position, and excepting the loss of vision in the affected organ, there were no remaining ill consequences.

Such a case appears to strengthen the conjecture we have already hinted at—namely, that pulsating disease in the orbit is probably in most instances the result of a clot-like deposit from the blood in one of the veins or sinuses leading from the eye towards the jugular: a cause undeniably of a dangerous nature, but not beyond the reach of a natural cure.

> "When examining a vein which is plugged by a tough and adhering coagulum," observes Dr. Humphry, "one can scarcely be persuaded that the circulation could ever have been re-established through it, if the patient had survived; yet there can be no doubt that this does take place, and that a vessel may in process of time resume its functions, and be restored nearly, if not entirely, to its natural condition, after its channel has been completely or to a considerable extent blocked up by a clot." . . . . "The clots," he adds, "may soften and become intimately connected with the walls of the vessels, and may lead to the complete and permanent obliteration of their canals. More commonly, however, they are removed, or shrink into delicate bands or fibres, which offer little or no obstruction to the circulation."*

Substances which are known to possess an influence in retarding the coagulation of fibrine, such as ammonia, are exhibited in cases in which clot-like deposits are suspected to have taken place during life, in the hope that they may operate within the living vessels in a similar way to what they do when mixed with blood removed from the body. Much dependence cannot be placed on such medication. Still, the urgency of the disease vindicates a trial of every means which holds out a prospect of being of use.

The object in tying the carotid in pulsating disease of the orbit, is merely to reduce for a time the contents of the branches of the ophthalmic artery, not to obliterate these vessels. This object appears, in one instance, to have been accomplished artificially, without cutting down upon or tying the artery, for in a case of aneurysm of the ophthalmic artery, Professor Gioppi, of Padua, is stated to have employed digital compression of the carotid with success.† Tedious, painful, and uncertain as this means may prove, it is still worthy of being kept in view, although in a case in which it was lately tried, under the care of Mr. Bowman,* it failed, so that recourse was obliged at last to be had to the ligature of the carotid.

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* On the Coagulation of the Blood in the Venous System during Life, pp. 13, 23. Cambridge, 1859.

† Quoted from the Giornale d'Oftalmologia Italiano, in the Lancet, Sept. 17th, 1859, p. 286.

Chapter XIV. is devoted to the DISEASES OF THE EXCRETING LACRYMAL ORGANS.

Displacement of the lower punctum Mr. Walton distinguishes as being either simple eversion, or malposition associated with structural change in the tarsus. When the displacement is simple, he recommends us to dissect off the palpebral conjunctiva from a spot just posterior to the canaliculus, regulating the amount to be removed by the degree of the eversion. By the ensuing contraction the eyelid is restored to its place, and the previous stillicidium removed. When, on the other hand, with displacement of the punctum the tarsal edge is rounded or so changed by chronic inflammation, that re-adaptation of parts seems impossible, he adopts Mr. Bowman's plan of slitting up the punctum and canaliculus, on into the lacrymal sac. In some cases, he combines the two methods, ensuring a cure not attainable by any other measures. (p. 341.)

Chronic dacryocystitis Mr. Walton treats of under the head of "Obstruction of the Lacrymal Duct:" an improper title, as the name "lacrymal duct" has long been appropriated to the ducts leading from the lacrymal gland to the surface of the conjunctiva. He regards the disease as generally of scrofulous origin. The treatment he describes as threefold: constitutional, such as will restore healthy nutrition; local, such as will abate inflammation; and mechanical, such as will remove stricture of the nasal duct. As indicative of stricture, he relies on the two following symptoms:

> "When one of the canaliculi is choked at the inner end, especially the lower one, there is, as far as I have observed, almost invariably stricture of some part of the duct as well. Again, when there is decided thickening of the parts over and about the lacrymal sac, so that the edges of the bones cannot be felt as in health, there is that condition that needs dilatation. . . . . A profuse discharge, even of purulent matter through the puncta, although frequently associated with stricture, is not in itself, as I have frequently ascertained by actual exploration, an unerring sign of obstruction. Again, all degrees of narrowing of the duct, and even complete occlusion, may exist, without any escape of pus, and but little of any secretion.
>
> "Being satisfied, from any circumstance," proceeds our author, "that dilatation is necessary, we must proceed to effect it by the natural channel; this is done by getting at the duct through the lower canaliculus. The first thing, therefore, is to slit it up, and to maintain it open. . . . . Mr. Bowman, to whom we owe the plan, is, so far as I can gather, an advocate for gradual dilatation with probes of different sizes. The process is, however, as I find, tedious, and so disagreeable, that but few persons in public or in private will submit to it sufficiently long. . . . . My practice in general, therefore, is to dilate for a short time only, using in the first instance the smaller probe, and afterwards the larger one, and then to introduce a style.
>
> "I am thoroughly convinced that where there is really that degree of change in this conduit which imperatively calls for instrumental treatment, the wearing of a style is the less irksome, the more beneficial, and the quicker plan.

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* Ophthalmic Hospital Reports, July, 1861, p. 234.

"In some instances, when my patient's time was short, I have introduced the style at once." (p. 346.)

Mr. Walton recommends styles of pure silver to be kept, of different diameters, because it may be advisable to commence with a small one. The figure he gives of the style which he employs shows a piece of silver wire about a thirteenth of an inch thick and two inches and a quarter long, having its upper end flattened and bent to a rounded right angle. The flat bent portion is to prevent the instrument from slipping out of sight, and lies outside the lower eyelid. This form of style was proposed, it seems, by Mr. Taylor, when ten or more years ago he attempted dilatation through the sac inside the eyelid, and with marked success. Mr. Taylor's operation, we presume, must have been the same as was recommended by Pouteau, a hundred years ago, in the following terms:

"Une jeune dame avoit un anchilops du côté droit. En le pressant on faisoit sortir par les points lacrymaux une sérosité purulente. Ayant inutilement tenté . . . . d'enfiler les points lacrymaux, il me restoit à proposer l'ouverture du sac. Je n'osai pas cependant le faire, parce que j'étois bien sûr de trouver la plus forte opposition de la part de cette dame. La nécessité m'indiqua la route suivante, et je promis à la malade qu'on ne verroit jamais aucune trace de sa maladie. Je plongeai une lancette dans le sac lacrymal, en passant entre la caruncule lacrymale et la paupière inférieure intérieurement. Je donnai à la lancette une direction oblique vers le fond du sac, et la plongeai sans crainte assez profondément: le pus sortit pas les côtés de la lancette. Je glissai alors une sonde à aiguille sur le plat de la lancette dans le canal nazal; et après avoir retiré la lancette, je débouchai facilement le canal, en poussant la sonde perpendiculairement, et je parvins ainsi dans le nez. La malade a été parfaitement guéric, sans autre accident qu'un petit engorgement avec échimose. En cherchant quelles pouvoient en être les causes, je reconnus que la membrane appellée conjonctive qui recouvre l'intérieur des paupières, et s'étend vers le globe de l'œil, étant extrêmement lâche, n'avoit pas été assez incisée. J'ai évité en d'autres occasions cet inconvénient, en augmentant l'incision de la conjonctive du côté du petit angle de l'œil; ce qui se fait en retirant la lancette vers la paupière inférieure.

"On ne peut pas contester que la route que je viens d'indiquer pour parvenir dans le sac lacrymal, n'ait de grands avantages. 1. Elle laisse les points lacrymaux dans toute leur intégrité. 2. Elle vuide les matières purulentes contenues dans le sac lacrymal. 3. Il ne reste après elle aucune apparence de cicatrice. 4. Elle permet de se servir d'une sonde plus grosse et moins flexible que celle dont on le sert pour les points lacrymaux, et on peut porter cette sonde plus directement contre les obstacles qui embarrassent le canal nazal. Cette méthode enfin a l'avantage d'être très-facile dans la pratique."*

So far as concerns the preservation of the lower punctum and canaliculus, parts not diseased, and which therefore, agreeably to the general principles of surgery, ought, if possible, not to be interfered with, the method of Pouteau seems to be superior to that of Mr. Bowman. If in the hands of Mr. Taylor, Pouteau's method was attended with "marked success," why was it abandoned for a method which is complicated and tedious, and which involves the destruction of parts so beautifully adapted to their office as are the punctum and canaliculus? It is acknowledged to be difficult to keep open the sort of gutter left after slitting the canaliculus, the adhesions formed by its sides requiring to be torn up from day to day with the probe, while the punctum we do not suppose is ever restored to its integrity, but remains disfigured and useless. Mr. Walton urges, that "among the several advantages of dilating an obstructed duct through the natural passages," that is, in Mr. Bowman's method, "there is no after trace of treatment, no marks on the face, no stains, no fistula to heal up, no sinuses to destroy." (p. 348.) If this were true of Mr. Bowman's method, it would be equally true of Pouteau's, invented, as this expressly was, to save the beauty of the "jeune dame de Lyon." But Mr. Walton's statement is not quite correct. Indelible traces of treatment must and do remain in the state of the punctum and canaliculus, and we have known Mr. Bowman's method to excite severe inflammation, terminating in abscess of the sac, which burst through the skin above the tendon of the orbicularis palpebrarum, and left a troublesome fistula.

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* Mélanges de Chirurgie, par M. Claude Pouteau, p. 100. Lyon, 1760.

In cases of an acute inflammatory attack being grafted on an obstructed nasal duct, Mr. Walton proceeds in the usual way. He opens the sac through the skin, and introduces a style.

As for probing the duct from the nose, in the manner of La Forest, Morgan, and others, his opinion is, that "the difficulty of the proceeding, from the anatomical intricacies of the part, the necessary tortuosity of the instrument, and the injury to the duct that is inseparable from the operation, are more than sufficient to banish it from practice." (p. 358.)

That to apprehend the presence of CONICAL CORNEA in its early stage requires close attention on the part of the practitioner is generally acknowledged. The oculist must have felt himself somewhat of a simpleton, when the lady who had been consulting him, and whom he had pronounced amaurotic, told him, as he bowed her out and received his fee, that she understood her disease to be rather conical cornea. Mr. Walton has found the ophthalmoscope of use in assisting him to a diagnosis:—

> "Soon after the ophthalmoscope was introduced," he says, "I examined a young woman whose defective sight could not be accounted for. She seemed near-sighted; but, then, concave glasses scarcely gave benefit. I now learned for the first time that this instrument could detect the conical change, the true cause of the impaired vision, in a degree so incipient, that in the first instance I had failed to notice it. I pointed this out to some of my colleagues. When the reflection from the instrument was made to play about the cornea at different angles, a darkened circle was seen on the sides not in the focus." (p. 361.)

No notice is here taken of what is really the most remarkable manifestation when an eye affected with incipient conical cornea is examined with the ophthalmoscope—namely, the change of figure which the optic disc seems to undergo as we pass from viewing it through the normal portion of the cornea to view it through the diseased portion, and the sudden and great diminution of its apparent size when seen through the apex of the cone.

"It was long since pointed out," says Mr. Walton, "by Sir David Brewster, that the cone is not quite regular, but more or less undulated; and this is best shown by means of a lens. The multiplying effect is due to this inequality." (p. 362.)

The candle or catoptrical test affords the best means of detecting the irregularity of the cone, and was that used by Brewster. The image of the candle, as it is reflected by the wrinkles of the cone, appears alternately expanded and contracted.

Mr. Walton notices the partial benefit which the eye affected with conicity of the cornea derives from a deep concave lens, a diaphragm, or the combination of these.

> "I was not a little pleased," he adds, "when, some years ago, I read this paragraph on conical cornea in the late Dr. Hull's work on the 'Morbid Eye.' 'In as bad a case as ever I saw, I have known most benefit received through an instrument made by a Mr. Abraham, an optician, in Bartlett-street, Bath. It is formed of two lenses, with an adjustment. The farthest and largest lens is convex. The lens near the eye is smaller and doubly concave.' I immediately wrote to Mr. Abraham, but he had been long dead; his son, who has left Bath and carries on the same trade in Clifton, never heard of his father's ingenuity." (p. 370.)

If Mr. Abraham's contrivance was merely a combination of a convex object-glass with a concave eye-glass, capable of being moved nearer to or farther from each other, it was nothing more than a Galilean telescope or opera glass, for such is the well-known construction of the optical instrument to which these names are given.

Mr. Walton adds his testimony in favour of giving to the pupil of the eye affected with conical cornea an elongated form, like the pupil of a cat, by tying the iris to the upper and lower edge of the cornea, in the manner practised by Mr. Bowman and Mr. Critchett:

> "A well-grown and robust farmer's daughter, twenty-four years old, was brought to me by Dr. Forester with conical cornea in both eyes. In the left the disease was less advanced, and she could yet read and use her needle. The cone in the right was so prominent that the apex was slightly opaque, and all useful vision extinct. She could not count my fingers. I made a linear pupil directly downwards by iriddesis. A fortnight later I operated above in the same manner with perfect success. I tested the vision directly that the irritability of the eye had passed away, and had the pleasure of finding that some sight had been restored; but the greatest effect was when a deep concave glass was used, she could now read the large words in title-pages. Heretofore this appliance was useless. She determined to return to town and have the other eye operated on in like manner, should it get more defective." (p. 270.)

The subject of CATARACT is fully discussed in Chapter XXIII.

The cataract-knife formerly used by Mr. Walton was one in which the cutting edge met the back at an angle of no less than 27°. He tells us he found this breadth too great, and now prefers Beer's knife. In it the cutting edge and the back form an angle of only 15°.

Speaking of the errors into which the operator is apt to fall in making the section of the cornea, Mr. Walton remarks, that

> "The most common, and therefore that to be most guarded against, is an imperfect counter-puncture; to obviate which, very ingenious, but very useless,

and even injurious instruments have been invented to fix the globe of the eye. It would be difficult to name a contrivance that has not been recommended. Certainly the most efficient and safe method, when an operator needs such assistance, is for an assistant to lay hold of the conjunctiva at the lower part of the eyeball, with a pair of toothed forceps, as has been much recommended by Mr. France. But except there be actual necessity for any such help, it should not be employed, from the pain and annoyance it causes. I have heard patients speak of it as the worst part of the operation." (p. 539.)

We consider the use of forceps for steadying the eye in the operation of extraction, proposed by Mr. France, a valuable improvement, enabling the surgeon to execute the most difficult and hazardous step of the operation with an ease and security no other means has been found to afford. The forceps need not be toothed, so as to penetrate or lacerate the conjunctiva; the blades require merely to be roughened near the point, to keep hold of the membrane. If the assistant holds the forceps, he must be instructed to do the office correctly, taking care not to drag at the eye, nor to press on it, after he has laid hold of it and brought it into the central position, lest by doing so he might cause rupture of the vitreous humour.

Mr. Walton sends his patient to bed immediately after the operation is over, having applied a strip of court plaster to each eye, even when only one has been operated on. (p. 538.) This is the only means he uses for maintaining the adaptation of the cut surfaces. He adds:

"I have for some years discontinued the usually prescribed practice of binding the eyes with a compress and bandage; for any degree of pressure on a wounded eye is injurious, and the heat and discomfort inseparable from such coverings must often give a disposition to, if it does not directly set up, inflammatory action." (p. 546.)

Our experience leads us to advise, in addition to the strips of court-plaster, that each eye be covered with a soft pad of carded cotton, and this supported gently by a double-headed roller, going twice round the head to the temples, and hence over the vertex and under the chin. We think this dressing ensures rest to the eyes, guards them from being touched or meddled with, and promotes the immediate union of the cornea.

On bleeding from the choroïd after extraction, Mr. Walton makes the following observations:

"It never occurs, of course, except in diseased eyes; and in all but one of the cases that I have seen, there has been undue vascularity of the surface of the eyeball, than which there is no greater evidence of an unhealthy state of the vascular apparatus within. In the exceptional one there was merely darkness of the sclerotic coat. I operated on one of the eyes of the patient in whom this existed, removed the cataract with perfect satisfaction, and was about to close the eye, when bleeding commenced, and continuing, forced out the vitreous humour, till the whole of it escaped; ultimately the retina was evacuated. A firm compress on the eye stopped further discharge. This was the first case of the kind I had seen associated with so little external change. I extracted the cataract from the other eye a few weeks afterwards, and precisely the same results ensued. In reviewing the several subjective symptoms throughout the entire duration of the cataract, there was nothing to indicate unhealthiness of the interior of the eye, beyond the rather frequent occurrence in the latter stage of small coloured spots. This, certainly more than the dusky hue of the surface of the eye, made me suspicious of some complication; and the expression of my opinion to that effect, together with some doubt about success, removed some of my responsibility, and saved no little annoyance." (p. 543.)

We must take the liberty to say, that the nature and seat of those "small coloured spots," which made Mr. Walton suspicious of some complication, ought to have been more clearly explained; but let them have been what and where they might, and let the suspicion to which they gave rise have been grave or slight, the circumstance that hæmorrhagy had destroyed the first eye ought to have warned him sufficiently that the second ought not to have been operated on by extraction. Mr. Walton's case is not the first of the kind of which we have heard. The same fate, we have been told, befel an English nobleman many years ago, in the hands of a most successful operator, since deceased. The first eye burst and bled during the night after the operation. After some time the second eye was operated on in the same way, and was destroyed by the very same accident. Would it not have been wise, after the loss of one eye, to have chosen some other method of operating for the second?

On the diet of the patient after extraction, our author offers the following remarks:

"The nature of the aliment during the first few days is important: the usual practice is to prescribe slops, more under the idea, I believe, that actual chewing is hurtful to the eye than anything else. In man the action of the muscles of mastication cannot in any way influence the eyeball. The more I practise, the more I am confirmed in the propriety of allowing, from the first, the usual diet at the usual times, only in rather less quantities. An old person who is confined and fed on slops is almost certain to have his digestive organs deranged, and if so, will most assuredly suffer from injurious prostration of strength, which may more or less interfere with success. I have had proof enough that starvation is no safeguard against inflammation, while it seems frequently to retard or to prevent the needed reparation. I am not aware that wholesome food, given in a state of system capable of assimilating it, will produce diseased action. I have had several opportunities of comparing the effects of a low and of a generous diet, and witnessing the advantages of the latter. Accustomed stimuli should not be absolutely prohibited; in some cases they may be requisite at the beginning. Aged persons may have a degree of prostration directly after the operation that demands alcoholic and other stimulants, followed by full diet." (p. 548.)

IRIDECTOMY, AND OTHER OPERATIONS FOR THE CURE OF GLAUCOMA, form the subject of Chapter XXVI.

Mr. Walton commences with an abstract of the three Memoirs by Professor Von Graefe, well known to English readers by Mr. Windsor's excellent translation, published by the New Sydenham Society. In these Memoirs it is explained how the Professor was led to employ excision of a portion of the iris, in various diseased states of the eye, from observing the good effects which flowed collaterally from the operation, when practised for the formation of an artificial pupil in cases originating in internal inflammation. The whole condition of the organ was thereby improved, and relapses prevented; the choroïd in an especial manner relieved, the vitreous humour restored, and even atrophic cases ameliorated beyond expectation. An account follows, in the Professor's Memoirs, of the symptoms of acute and chronic glaucoma, in which there is little absolutely new, except the ophthalmoscopic signs of excavation of the optic papilla and pulsation of the central artery of the retina, which, however, are not absolutely pathognomonic of the disease, but attend other morbid states of the eye. That the stony hardness of the eye in glaucoma is due to an inordinate secretion of vitreous fluid, the consequence of a disordered condition of the choroïdal circulation, that by pressure this serous apoplexy of the eye (as it may be called) causes severe pain, and destroys the sensibility of the retina, and that relief is obtained by puncturing either the sclerotica or the cornea, are facts familiar of old. To relieve the symptoms of acute glaucoma, Professor Graefe revived the operation of paracentesis corneæ, and through ignorance or inadvertence, claimed as original the application of the practice to this disease. The case in which this claim is set up is deserving of attention on several accounts, and will be found quoted at length in a former article of this Review.* The relief in this case, from simple evacuation of the aqueous humour, appears to have been complete and permanent. In other cases, we presume he must have failed in obtaining the same benefit, and was therefore led, judging from the good effects he had found to follow excision of a portion of the iris in cases of closed pupil after irido-choroïditis, to try iridectomy. In acute glaucoma, the results were very striking. It is well known that the violent pain of acute choroïditis, commonly called acute glaucoma, may, in a single night, be attended by a total loss of vision; while all ordinary antiphlogistic and anodyne means will in general fail to give relief. The pain Professor Graefe found to cease immediately after the operation of iridectomy, while in the course of a few weeks vision was perfectly restored, in all the cases in which the operation was performed within a fortnight from the occurrence of the attack. In chronic glaucoma, the results were, as might be expected, not so satisfactory.

Admitting, as Mr. Walton does, and as every candid inquirer must do, that beneficial effects have followed iridectomy in acute and chronic glaucoma, a question arises: To what are these effects to be essentially attributed? We think this question deserves more attention than has hitherto been bestowed on it. We must confess we are inclined to doubt whether the loss of a piece of the iris is by any means the chief factor in the proceeding. We are disposed to attribute no small share in the beneficial result, *first*, to the action of the chloroform under the influence of which the operation is generally performed, and which of itself proves so valuable a remedy in many painful affections of the eye; *secondly*, to the relief of tension afforded by the incision of the consolidative tunic, and that whether the incision implicates the cornea, the sclerotica, or both of these structures; *thirdly*, to the sudden evacuation of the aqueous humour and reduction of the fluid contents of the eyeball; *fourthly*, to the bleeding which follows the excision of the portion of the iris, and which is often considerable in quantity; and *fifthly*, to the communication which, in exceptional cases, is established between the vitreous humour and the posterior chamber, and by which the superabundant vitreous fluid must be partially drained away.

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* See No. for July, 1859, p. 77.

Mr. Walton's abstract of Professor Graefe's Memoirs is followed by a notice of Mr. Hulke's valuable contributions to the pathology of glaucoma, founded chiefly on dissection. One of these is the firmness of the vitreous humour in the early stage of the disease, in contradiction to the abnormal fluidity ascribed to it by some authors. "It is only in old cases, where all the other tissues are involved in a common atrophy, that it becomes diffluent." (p. 638.)

Our author then proceeds to speak, from his own personal observation and experience, of the results of iridectomy in glaucoma. He states, that he has had frequent proof that the operation has been recklessly applied to cases to which it was wholly inapplicable; that he had seen several instances in which it was applied where there was no glaucoma; that in most of these it was done "in anticipation of the disease," and to the injury of the patients; that he had saved many patients from being subjected to it, in whom there was merely slight defective sight from haziness of the vitreous humour, or whose eyes were affected only with sclerotic inflammation.

"A gentleman who had been submitted to iridectomy in one eye," says he, "applied to me. Both eyes, it appeared, had been the subject of some disease that produced mistiness of vision, but he would allow only one to be operated on. The disease disappeared, and the eye that was untouched quite recovered. The other, so far as I could tell from my examination, was defective only to the extent occasioned by the loss of the iris.

"I attended a private patient for three attacks of severe rheumatic inflammation of the eyeball. When I saw her first, the iris was already more or less adherent to the capsule of the lens, and the retina almost insensible; the last accession of inflammation completed the adhesion of any portion of the pupil that had been free, and destroyed all sight. The lens was not opaque. She was in course of attendance on me, when she was induced by a physician to consult another surgeon for this eye, and it so happened that the morning on which she called, the other eye, as she expressed it, was rather red, and felt a little weak; but sight was not in the least impaired. Iridectomy was proposed, and the advantages so forcibly placed before her, and so admirably contrasted with the inevitable blindness that would follow, were it neglected, that the terms were accepted. The operation was immediately executed on both eyes. I saw the lady four months afterwards. The disorganized eye was of course no better. The other was rendered so very imperfect by the operation—that is, by the excision of so large a portion of the iris—that she could not read the largest type, nor do any kind of plain or worsted work, nor see anything distinctly." (p. 646.)

"All those things," observes Mr. Walton, "tell nothing against iridectomy, if it be a valuable operation." Certainly not; they only demonstrate a lamentable ignorance of diagnosis, and a wretched greed of gain.

"It remains for me," says he, "to give the result of my experience from what I have seen of the operation in glaucomatous eyes, in the practice of others, and from my own. Respecting the first, the cases have been, with but few exceptions, of the chronic form of the disease. In some of them there was certainly a slight improvement of vision for a few days, but in none has this been more than temporary. Pain has also been relieved, and in a few has been so long absent, that it has been supposed to be for ever removed, when with sad disappointment it has returned. . . . . Of the acute kind I can give no better report." (pp. 642, 643.)

He expresses his opinion that all the good which follows iridectomy may be attributed to the mere tapping of the aqueous humour, and closes this part of the subject with a quotation from Mr. Dixon's 'Practical Guide to the Diseases of the Eye,' in which the same conclusion is adopted.*

Mr. Walton next proceeds to mention some other operations proposed for the cure of glaucoma—namely, Mr. Hancock's division of the ciliary muscle, by a cut through the sclerotica; Mr. Solomon's intra-ocular myotomy, by pushing a cataract-knife through the corneo-sclerotic junction in the direction of the annulus albidus; and Mr. Nunneley's incision, about an eighth of an inch long, half through the sclerotica, half through the cornea, by which the tension of the tunics is relieved, and the aqueous humour evacuated. All three are proposed as substitutes for iridectomy, and are lauded by their several authors as exempt from the objections to that operation, while possessing all its advantages. If this be true, iridectomy, for the cure of glaucoma, must be set down as a barbarity. Mr. Solomon's procedure seems a sort of operative panacea for the eye, for it is supposed to cure not merely glaucoma, acute and chronic, but conical cornea, myopia and presbyopia, asthenopia, &c. We remember similar claims being put forth for division of the external muscles of the eye, some time after the operation for strabismus came into vogue, but they were soon given up.

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* The reader who wishes to pursue the early history of eye-tapping, may consult Mauchart, de Paracentesi Oculi, contained in the first volume of Haller's Disputationes Chirurgicæ. He will there find a notice of all that was done in this line by Wesem, Nuck, Hovius, Heister, Woolhouse, Turberville, and others.

Mauchart makes no mention of glaucoma, but he recommends (§ vii.) paracentesis scleroticæ as a means of relief in what he terms "corporis vitrei serosa turgescentia," which may be presumed to be much the same as the "intra-ocular pressure" of to-day.

Whyte, an army surgeon, who afterwards fell a victim to his zeal in investigating the plague, dating his communication (Mode of Managing Ocular Inflammations; Medical and Physical Journal, March, 1802, p. 209) from the Bay of Aboukir, July 8th, 1801, advises to pierce the tunics with a couching needle, and enter the posterior chamber of the aqueous humour, by an incision parallel to and behind the iris, permitting, he says, an outlet proportioned to the existing expansion. He states that he practised this frequently, with success, and ever with impunity.

Wardrop recommended paracentesis corneæ, in cases where the eye was distended, prominent, and inflamed. Edinburgh Medical and Surgical Journal, January, 1807, p. 56.

Mackenzie (on Glaucoma: Glasgow Medical Journal, August, 1830, p. 265) ascribes the preternatural firmness of the eye to over-distension of the tunics by fluid occupying the place of the vitreous humour, and recommends paracentesis scleroticæ, repeating this in the first three editions of his Practical Treatise on the Diseases of the Eye. In his fourth edition, London, 1854, pp. 571, 899, he advises paracentesis of the cornea or of the sclerotica, in acute and chronic glaucoma, to relieve the pain, and take off the pressure of the accumulated fluid on the retina.

Middlemore (Treatise on Diseases of the Eye, vol. ii. p. 19, London, 1835) recommends puncture of the sclerotica to relieve preternatural fulness of the globe in glaucoma. Also, after subsidence of the inflammatory symptoms, he proposes puncture of the sclerotica to let out the turbid vitreous humour, replacing it by an injection of clear lukewarm water.

Von Graefe (Archiv für Ophthalmologie, Band i. Abth. ii. p. 302, Berlin, 1855) announces paracentesis corneæ as a new remedy for acute glaucoma. He states that immediately after the evacuation of the aqueous humour, the iris and pupil appeared much clearer, that a corresponding improvement of sight instantly took place, and that the ultimate result was such good vision as enabled the patient to read No. 3 of Jäger's test-types.

The physiological effects of paracentesis oculi, both corneal and sclerotic, were investigated experimentally by Wedl (Rudiments of Pathological Histology, translated by Busk, London, 1855, p. 19). He showed that these operations, particularly puncturing of the cornea, gave rise to stasis of the iridal and choroïdal vessels; a result which may at first sight seem to speak against the therapeutical value of paracentesis oculi, but in reality does so no more than the congestion of the conjunctiva, caused by dropping in a solution of lunar caustic, speaks against the therapeutical value of that application in catarrhal ophthalmia.

Mr. Walton concludes with a chapter on the OPHTHALMOSCOPE, in which he explains the nature of the instrument, the method of using it, and the information obtained by its employment, succinctly, intelligibly, and on the whole accurately.

He commences with an account of Mr. Cumming's discovery of the luminosity of the human eye, and of its application to the detection of disease in the fundus oculi. He then notices the inventions of Helmholtz, Ruete, Coccius, and Anagnostakis, whereby the interior of the eye may be illuminated and seen with facility; describes the most common ophthalmoscope as consisting of a concave mirror and a biconvex lens; mentions the kind of light to be employed; recommends the gas-lamp used by the watch-makers of Clerkenwell, with a light-blue chimney; and directs the examination to be made in a room otherwise dark, and with the patient's pupil dilated by means of a solution of half a grain of the sulphate of atropia in an ounce of water. A good and obvious piece of advice given by Mr. Walton is, that a study should be made of the healthy eye, preliminary to that of diseased ones; the ophthalmoscopist will naturally save himself from many blunders, who makes himself acquainted, in all their varieties, with the normal appearances of the internal structures of the organ.

Proceeding to describe the manner of conducting an examination, he tells us that the observer, looking through the central aperture, "moves the instrument forwards, and endeavours to get the focus. A diffused reddish glare shows that the interior of the eye is illuminated. With a little adjusting, the retinal vessels or the optic disc may be seen." (p. 660.) Here it would have been well if Mr. Walton had added the direction given a few pages on (p. 664); namely, if the right eye is under examination, to make the patient look towards the observer's right ear; if the left, towards his left ear. By following this simple direction, the optic disc starts into view. From want of knowing it, beginners often fail in finding the disc, and even throw down the ophthalmoscope in despair.

Mr. Walton does not pretend to examine the subject of the ophthalmoscope scientifically, but refers us to the valuable and accurate work of Dr. George Rainy for the theory of the instrument.* We should have been glad, however, had our author escaped such an error as to tell us, in describing what is termed the indirect method, that "to magnify the parts at the fundus, the lens is now used." (p. 660.) The fact is, that in the direct or erect method, in which we view the fundus without the aid of any extraneous lens, or with that of a biconcave lens, the parts on the fundus, such as the papilla optica, appear magnified as much as fourteen or fifteen diameters, whereas with the biconvex lenses commonly made use of in the indirect or inverted method, those parts are magnified from two to five diameters only. The use of the biconvex lens in the indirect method is not to magnify, but to reduce, so that we may see a larger portion of the fundus at the same time, and with better definition; whereas, without the lens, the optic disc fills more than the whole pupil, we can see only a part of it in one view, and it appears indistinct from being too much magnified. We suspect Mr. Walton's ideas on the difference of the two ophthalmoscopic methods are not very clear; he makes no mention of the use of the concave lens in correcting the convergence of the rays in the direct method; in a quotation (page 671, line 21), we observe he erroneously substitutes the word "direct" for "indirect," and misled, perhaps, by the vulgar name *magnifying-glass*, he plainly mistakes the effect of the convex lens in the indirect method. Speaking of the lenses which compose the apparatus of some surgeons, he makes the following remark:—"That these varieties can be brought into requisition, according to one's fancy, is undoubted; but I believe that they possess no utility, no decided practical advantage." (p. 657.) Now, fancy has nothing to do in the matter. If the observer really comprehends what he is about, he will select the lenses he employs according to the quality of his own vision and that of his patient, whether normal, myopic, or presbyopic, according to the adjusting power of his own eye and of his patient's eye, and according as he desires to have a general view of the fundus oculi well defined, or to examine in detail its minutest parts, greatly magnified. These are the reasons why the apparatus of the ophthalmoscopist contains both divergent and convergent lenses of various powers.

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* See the number of this Review for July, 1861.

The following sensible remarks we would commend to the attention of ophthalmoscopic beginners:

> "An examination should not be uselessly or unreasonably long. It would be unpleasant to any one, to say the least of it, to have a concentrated light on his retina for the greater part of an hour, and this persons seem to forget when they are learning to use the ophthalmoscope. We know that it is not very uncommon for the fundus of the eye to become preternaturally red during an ordinary inspection—a fact always to be remembered, and to be guarded against by avoiding a lengthened sitting, or by resting the eye for a few seconds when the process is necessarily prolonged. No surgeon with any common sense would employ the ophthalmoscope when it would give pain or cause any uneasiness. It is just in such cases that the internal examination of the eye is not needed; there is enough indication to direct our treatment. The intolerance to light is a significant symptom; and, if it be associated with any surface-redness, there is evidence of inflammation of the eyeball.
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> "On occasions when there is slight sensitiveness to light—so slight as to warrant a careful and brief examination,—I reduce the lamp-flame, and illuminate less: toleration will then embolden us to employ more light, should it be required.
>
> "The examiner is likely to fatigue his own eye by the consecutive inspection

of several subjects. I have known indistinctness of vision to be produced, and to last for days. The prevention is, to avoid continuous work, or to use the eyes alternately." (p. 663.)

Mr. Walton proceeds to describe the appearances presented in the normal state by the optic disc, the central artery, the retina, the macula lutea, the choroïd, the cornea, the crystalline, and the vitreous humour. He then directs attention to the morbid appearances. He notices anæmia, hyperæmia, apoplexy, extravasations, and pigmental changes in the disc; its cupped appearance in atrophy of the optic nerve and in glaucoma, with the seeming break in the vessels as they bend out from the cup upon the retina. The signs of hyperæmia and of inflammation of the retina are next enumerated, and of ecchymosis, hæmorrhagy, and exudation affecting that membrane; also of fatty degeneration, pigmental deposits, atrophy, and detachment from the choroïd. The alterations of the choroïd are next described, originating, as they do, chiefly in inflammation; alterations affecting the pigment, which is sometimes abnormally accumulated, sometimes absorbed, or variously changed; colloïd degeneration of the elastic lamina of the choroïd; hyperæmia of the capillary layer; plastic exudation; rupture of the choroïdal vessels, with and without perforation of the retina by blood; atrophy of the choroïdal vessels; detachment of the choroïd from the sclerotica. Lastly, posterior staphyloma is noticed—a diseased state in which the sclerotica bulges out behind, so as to elongate the antero-posterior axis of the eyeball, and become the cause of a peculiar variety of myopia. Some remarks on the morbid appearances of the cornea, crystalline, and vitreous body, had previously been introduced, along with the description of the normal eye.

The whole account of the opthalmoscopic appearances of the eye, healthy and diseased, is executed with so much care and perspicuity, as highly to commend itself to the attention of the reader. We should willingly have extracted the remarks on the ophthalmoscopic signs of cataract (p. 668), had our limits allowed.

Happy to congratulate Mr. Walton on his work having reached a second edition, we have only to repeat what we said of the first, that, on the whole, we know of no treatise on the same subject better deserving a place in the library of the surgeon.

REVIEW III.

A Practical Treatise on Diseases of the Skin in Children. From the French of CAILLAULT. With Notes, by R. H. BLAKE, M.R.C.S. Lond.—*London,* 1861. pp. 277.

IT must be acknowledged, that an English book on the Skin Diseases of Children is addressed to a real and not a merely imaginary want. There are no branches of medical knowledge in which the English student is so deficient as in the diseases of the skin, and the diseases of childhood; none in which he would so much suffer on comparison with a student educated in the hospitals of Paris. This cannot be attributed to any lack either of industry or of ability. The cause must be sought in the much greater facilities offered in Paris than in London for the study of these diseases. The magnificent hospitals of St. Louis and the Enfants Malades, each with many hundreds of beds, present to the industrious student ample and accessible fields for clinical observation. In London we have nothing to set against them but the small establishments in Great Ormond-street and in Blackfriars. Both of these are quite inadequate for the instruction of the large numbers of students educated in London. The former has indeed been lately somewhat increased in size; and by the praiseworthy efforts of Drs. West and Jenner, courses of lectures have been established, the previous want of which is evinced by the crowded audience that avails itself of them. The hospital is, however, still too small for the purposes of education, containing, even now that it is enlarged, only fifty beds. The case is still worse with the hospital for diseases of the skin. Here there are no beds at all for male, and six only for female patients. Students are indeed admitted within its walls, but they find there no lectures nor systematic instruction of any kind. Of the therapeutic skill of its medical officers we have that best of evidence, the ever-increasing number of out-patients that flock to them for relief. But this very multitude of out-patients is a hindrance to the proper instruction of the student. In a small and ill-lighted room, he sees on each occasion some two or three hundred patients pass by in rapid succession. The medical officer can, of course, only give to each of them that minute portion of time which enables his experienced glance to detect the nature of the affection. This is quite insufficient for the unpractised eye of the student, whose difficulties are still further increased by the use of a special and complicated pharmacopœia. Under these circumstances, we can hardly be astonished when a gentleman who has much experience of medical students informs us, that they constantly come to the end of their hospital life without being able to name or recognise with certainty the most common diseases of the skin.* The knowledge which has been entirely neglected during the period of studentship is seldom acquired in the busy years of later life; so that we believe we are correct in stating, that there are no branches of medical knowledge so neglected and so little understood by English practitioners, as the diagnosis and treatment of skin diseases. The very books we use are almost entirely of foreign origin. For one by an English author, there are on our shelves four or five of French or German production. From time to time one of these appears in an English form; and it is to these translations that the student owes such little knowledge as he possesses of cutaneous pathology. The book before us is a fresh addition to this class. Its author, M. Caillault, was for some years "interne" in the hospital for sick children in Paris, and there it was that he gathered materials for his treatise.

To say that M. Caillault is a Frenchman, is almost to say that he is not a follower of Willan. Nationality shows itself even in the classification of diseases. The English dermatologists, almost without exception, have adopted the method of their compatriot Willan, whose classification was based exclusively on the anatomical lesions of the skin. The French doctors, on the contrary, though at first they accepted Willan's arrangement, have in the present day universally abandoned it in favour of the more natural method, of which their countrymen Lorry and Alibert may be considered the authors. In this no one single character is selected as a sufficient basis for classification, but all the features of a disease are taken into consideration—its cause, its predominant symptoms, its usual course, its treatment; and a place is assigned to it in virtue of the general analogies and resemblances presented by it. This latter method is manifestly the more philosophical one. It stands in the same relation to the system of Willan and Bateman, as does the natural botanical arrangement of De Jussieu to the artificial one invented by Linnæus. Both Linnæus and Willan adopted a single anatomical character as the basis of their classification. The result in both cases is, that under the same head are found grouped together individuals that have no second point of resemblance; while others are widely separated, in spite of their presenting the most striking affinity to each other. The arrangement of the botanist had, however, this advantage over that of the dermatologist,—the character selected by him as a ground of classification was in reality a most important one, with which other characters were often inseparably bound up, so that it served as a key to them; and thus it not infrequently happened that his artificial groups corresponded exactly to the natural ones of De Jussieu—as, for instance, the Tetradynamia to the Cruciferæ, or the Syngenesia to the Compositæ. In dermatology, on the contrary, the anatomical element is of such secondary importance, that not one single group based upon it can in any way be considered a natural one. Those who adopt it as the basis of their classification, while they acknowledge that the natural system is the better, *theoretically* considered, urge that such an arrangement is practically impossible, inasmuch as our knowledge of skin diseases is not precise enough for us to be able to refer any given one to its proper heading. This is in too many cases true, though not so often as is implied. But only so far as we can thus refer them is classification of any practical use whatsoever. Moreover, the same objection might be urged with still greater force against the artificial divisions of Willan. It is frequently impossible to decide with certainty what was the external character of the original eruption. Still more frequently, the anatomical elements of several distinct classes are united in the same individual, who must therefore be supposed to suffer at the same time from several different cutaneous diseases.

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* Dr. Jenner: Medical Times, 1857.

A still greater objection to this classification of Willan is, that it is of no use therapeutically. Even if it were possible in each instance to discover what was the original lesion, and to refer the case with certainty to its class; yet when this had been done, the practitioner would have gained nothing towards its treatment, inasmuch as the artificial groups are not, like the natural ones, therapeutic unities, but are at best only convenient modes of nomenclature.

Our space will not allow us to consider the various attempts which have been made, with more or less success, to form natural classifications. We must content ourselves with a brief account of the method adopted in the volume before us. M. Caillault arranges the skin diseases of childhood under nine heads: 1. Syphilis; 2. Strophulous diseases; 3. Lymphatic; 4. Scrofulous; 5. Dartrous; 6. Parasitical; 7. Hæmorrhagic; 8. Cachectic; 9. Inflammatory. The sequence in which these classes are arranged is not an accidental one. It represents the order in which they succeed each other as age advances, each period having, so to speak, its peculiar affections. Thus the first group manifests itself chiefly during the earliest period of infancy. From this we advance to the diseases which appear during the time of dentition, and so gradually on through successive periods up to puberty, until we terminate with those affections which manifest themselves indiscriminately at all ages of infancy.

The first class includes acquired and congenital syphilis. Acquired syphilis is naturally of very rare occurrence in childhood, and differs in nothing from the syphilis of adults. Congenital syphilis is pre-eminently the skin disease of earliest infancy. The usual period at which it manifests itself is from a month to six weeks after birth. Sometimes its appearance is delayed till so late as the seventh month, and, according to M. Hardy,* occasionally even till after puberty; but in the vast majority of cases it declares itself before the end of the third month. This was the case in 146 out of 158 instances collected by Diday. Perhaps the best part of M. Caillault's treatise is that devoted to this disease. Our space will only permit us briefly to point out the peculiarities in his views.

According to M. Caillault, congenital syphilis is altogether cutaneous, exclusively confined to the skin. He takes no account of the various visceral lesions described by different observers—such as the infiltration of the liver, pointed out by Gubler; or the lesions of the lungs and thymus gland, described by Dupont and Dubois. The appearance of premature decrepitude, the "look of little old men," which has been so often described, and which is usually considered a characteristic sign of syphilis, is, according to M. Caillault, of no such diagnostic value, but is a result of cachexias, of whatever kind they may be. The symptoms of congenital syphilis, thus limited to the skin, are still further restricted by him to one single external manifestation—viz., the mucous patch, or, as it has been variously styled, the mucous tubercle, the flat tubercle, the syphilitic patch, the humid papular syphilide. All the other infantile eruptions usually ascribed to syphilis—as, for instance, the pemphigus neonatorum—are to be considered as mere signs of cachexia, syphilitic or other. The contrary opinion, he says, results from the constant comparison drawn by authors between the syphilis of infants and that of adults, and the desire to find in the former all the pathological phenomena presented by the latter. Now that M. Caillault is right when he says that symptoms are frequently ascribed to congenital syphilis which are in reality the result of simple cachexia, is, we think, extremely probable; but to neither of his chief propositions can we give our assent. We cannot agree with him that congenital syphilis is exclusively confined to the skin; nor yet that its sole cutaneous manifestation is the mucous tubercle. Whatever may be the case with pemphigus, on which authorities are at variance, all observers are agreed as to the existence and frequency of a syphilitic roseola in infants. As regards the other point—namely, the limitation of the disease to the skin, we agree with Dr. West that the evidence of the dependence of the affection of the thymus and of the liver on the syphilitic poison must be regarded as conclusive. We may also refer to the researches of Mr. Hutchinson on the alteration of the teeth in syphilitic children, and to the case given by Dr. West of the destruction of the bony palate from syphilis in an infant of a few months old.* Still, the mucous patch is certainly, as Diday states, incomparably the most frequent, as well as the earliest symptom of syphilis in new-born children, and its diagnosis, therefore, becomes of great importance. For a full description of it we must refer our readers to the pages of Diday and Caillault, contenting ourselves with pointing out the peculiarities presented by it in children. In the first place, the coppery tint, or, as it has been better described, the "lean of ham" tint, on which so much reliance is placed in the diagnosis of syphilitic eruptions, is almost invariably *absent* in children. In the second place, this affection, which in adults occupies, by preference, the mucous membranes, or those parts of the skin which are thin, moist, and exposed to constant friction, affects in young children the most varied situations, and may be found on any part of the body. The peculiar anatomical conditions of the skin in infancy are a sufficient explanation of this. In other respects the mucous patch, or flat pustule of infants, resembles entirely that of adults. On the disputed question of the contagious nature of this eruption, M. Caillault takes a middle course. He will not go so far as Diday in considering that it is contagious in the highest degree, while he admits that it is occasionally communicated from the infant to the nurse, and from the latter to her nursling.

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* Leçons sur les Mal. de la Peau, tom. i. p. 167.

The second group, the strophulous diseases, includes all those ephemeral eruptions which arise under the influence of dentition. The most frequent of these is the papular eruption, which gives its name to the class. But this anatomical character is by no means invariable, vesicles, pustules, and erythemata, not infrequently appearing under the same influence. None of these eruptions are contagious. They are all accompanied by intense itching, and usually by derangement of the intestinal canal, but they seldom require the interference of the physician. Mr. Blake recommends tepid alkaline baths, or lotions containing glycerine, as an effectual relief to the itching.

The third and fourth classes are formed by the lymphatic and scrofulous diseases—two groups whose limits are so vague and indeterminate that it is impossible to define where one begins and the other ends. The lymphatic constitution, however, as a rule, betrays

* West's Lectures, p. 672.

itself at an earlier period of life than does the scrofulous, the latter retarding its appearance usually till after the second year; whereas the achores of the face and scalp, which are so common in the former, appear ordinarily at a much earlier date. Among the lymphatic affections, M. Caillault has ranged the curious affection first described by Bateman under the name of Molluscum contagiosum, and afterwards by Baziu under that of Acne varioliforme. Although Bateman had pointed out the contagious nature of this disease, pathologists seem to have entertained doubts of his accuracy, when, in 1851, M. Caillault re-established the fact in a paper contributed to the Archives de Médecine, having seen the disease spread from bed to bed in the Children's Hospital, till no less than thirty children were attacked by it. Cases have also been recorded by Dr. Patterson of Leith,* and by M. Hardy,† in which infants presented on their faces molluscous tumours, while the nurses that suckled them had similar tumours on their breasts. No doubt, therefore, can be entertained of the correctness of Bateman's statement. M. Hardy attributes the contagious character of the disease to the spores of a cryptogamous plant, which he states himself to have discovered in the contents of the tumours. We were ourselves unable to detect their presence in the only case we have examined microscopically. But should his observation turn out to be correct, this affection must clearly be transferred to the group of parasitical diseases. An affection more properly classed in this group is the chilblain, or, in scientific language, the erythema pernio. A large proportion of the skin diseases of childhood are scrofulous, especially in large towns. They make their first appearance after the second year of life, and are most frequent, according to Bazin, between the fifth and the fifteenth. The anatomical characters of these eruptions are most varied. Some, as lupus and cutaneous tubercle, are to be found in the class Tubercula; others, as eczema, under Vesiculæ; while scrofulous impetigo is separated from both under Pustulæ, where it finds itself classified with small-pox. Yet no one can doubt of the pathological unity of these scrofulous affections, nor of the propriety of collecting them under one head.

The fifth group are the dartrous affections. This name is an unfortunate one. The word "dartres" is in popular use in France, but without any very precise signification. It is most ordinarily employed, we believe, in contradistinction to "teignes," the word applied to chronic diseases of the scalp, to denote any chronic affection of other parts of the skin. Owing to this want of precision in its meaning, the word had long been banished from scientific treatises, when Hardy of late years called it back into service to designate a group of diseases which, according to him, were the results of a particular state of the economy, to which he gave the name of the dartrous diathesis. The characters of this group of diseases are the following: They are usually chronic, are not contagious, are hereditary, frequently re-occur, often co-exist in the same individual, have a great tendency to spread over a large surface, are attended by itching, and leave no scars after they are healed, though they often lead to ulceration. M. Hardy classes four diseases only under this heading—eczema, lichen, psoriasis, pityriasis. We do not know why these only are admitted by him. There are other affections which seem to us to come equally under his definition of "dartres;" for instance, prurigo, which is accordingly ranged by Caillault under this heading, together with ichthyosis. The characters laid down by Hardy as distinctive of dartres are very important ones. Yet, in all probability, under this name are confounded two or more distinct families. In a treatise published last year, M. Bazin has attempted to show that this is the case. According to his view, Hardy's "dartres" include three distinct groups—the "scrofulides benignes," the arthritic (including gout and rheumatism), and the dartrous proper or herpetic. Eczema, for instance, which Hardy considers as always dartrous, is frequently, and in children almost invariably, of a scrofulous nature. Besides this scrofulous kind, there are in adults two other eczemas—one of arthritic, the other of dartrous origin; and so also with psoriasis and the other dartrous affections. The question then arises, how are the arthritic to be distinguished from the dartrous varieties. M. Bazin has discussed this point in great detail. Rarely does the anatomical lesion at once decide the question; such, however, is sometimes the case. The oval tumours of erythema nodosum are invariably referable to a rheumatic constitution. Such also in children, though not in adults, is stated to be the case with herpes zoster.* Usually, however, as we said, the anatomical elements are insufficient to guide us. Still, according to M. Bazin, the eruption presents characters which enable us to form the diagnosis. The arthritic eruptions are frequently not symmetrical, whereas the true dartrous ones are nearly always so. The former manifest a preference for certain sites, have certain "lieux de prédilection;" the latter appear indiscriminately on all parts of the body. Again, the former are often developed after exposure to cold, the latter after some violent emotion. But the best guide in determining the nature of a given case is to be found in the antecedents of the patient. Should all these means fail, still, in accordance with the maxim of Hippocrates, the effect of remedies may betray the nature of the disease. The true dartrous affections improve, as a rule, under the treatment of arsenic, whereas this drug is useless in arthritic cases, which are benefited by colchicum, alkalies, &c. It must, however, be remembered that the two conditions are by no means incompatible; they may co-exist, like syphilis and scrofula, a combination which is not uncommon.

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* Dr. Patterson's observations were published ten years before M. Caillault's paper appeared in the Archives. Edinburgh Medical and Surgical Journal, vol. lvi. p. 281.

† Leçons sur les Mal. de la Peau, deuxième partie, p. 98.

We have said this much on this class, because the word "dartres" is now much used by French dermatologists, and it may be of service to our readers to know what sense is attached by them to it. But childhood is by no means the period at which these affections most usually appear. They may be found then, and indeed at all ages, but they increase in frequency when childhood is past.

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* See Bazin's work on Scrofulous, Arthritic, and Dartrous Affections of the Skin, p. 125.

The sixth group are the parasitical diseases. Common at all ages, these are more especially so in childhood. Of some forms indeed—viz., Favus and Tinea tonsurans, this age has almost the monopoly. But as a compensation it is free from Sycosis, the exclusive appanage of adults. Great progress has been made of late years in the diagnosis and treatment of these affections; much of which is due to the researches of M. Bazin, which are embodied by M. Caillault in his book. The animal parasites that are found commonly in the skin of children are two, the Pediculus capitis, and the Acarus scabiei. The former is found sometimes in incredible abundance in cases of impetigo granulata; and it is a question whether the insect or the eruption is the originator of the mischief. The best way to get rid of this parasite is to cut the hair short and to powder the scalp liberally with staphysagria. Scabies in infants differs in nothing from the same disease in adults, excepting in its site. In adult men the affection ordinarily begins on the genital organs, and is thence communicated to the hands during scratching. In young infants the buttocks are as a rule the first part attacked, the acarus being transmitted from the hands of the nurse that supports them. The treatment has been reduced at St. Louis to a minimum of time, not occupying more than an hour and a half. Of its efficacy we have convinced ourselves. The plan adopted there is the following. The patient is first rubbed for half an hour from head to foot with black soap. Then he is immersed in a warm bath for a second half-hour, and lastly is rubbed all over with an ointment, containing 300 parts of lard to 50 of sulphur and 25 of subcarbonate of potash. To prevent re-infection, the clothes are subjected to a high temperature, or to sulphurous fumigations.

More numerous and less easy of cure are the vegetable parasites. The chief of these are the Achorion of Schœnlein, the Trichophyton and the Microsporon of Audouin. These respectively give rise to Favus, to Tinea tonsurans and Herpes circinnatus, and to Porrigo decalvans. In all, the great aim of the medical man is to destroy the parasite. This done, the eruption vanishes. There are abundance of applications capable of destroying the cryptogamous vegetations when brought into direct contact with them; but the difficulty is to effect this. The fungus is not confined to the surface of the scalp, but occupies the hair follicles, and even penetrates into the interior of the hairs themselves, where it is safe from any external application. The only sure mode of dealing with such cases is to pull the hairs out, and then apply the parasiticidal ointments. This is not very difficult in favus, but becomes exceedingly so in tinea tonsurans, where the hairs are so brittle that they break off under the forceps. It is not, however, absolutely impossible, as Dr. Jenner states it to be, and is certainly accomplished by the practised hands of the St. Louis "épileurs." The difficulty is still greater in the case of P. decalvans, and requires great skill and perseverance. The plan adopted in favus at St. Louis is the following, and will serve to show how these ailments are generally treated.

The crusts are first removed by poultices, and the hairs are then cut very short and pulled out one by one with a pair of pincers. Care must be taken to pull them out in the direction of their growth. This operation requires several sittings, varying in number with the sensibility of the patient and the extent of surface affected. M. Bazin smears the scalp with oil of cade, and thinks that the depilation is thus rendered less painful. But M. Hardy is of a different opinion. A lotion containing corrosive sublimate (one grain in the ounce), is freely used during the process of depilation. When the patch has been entirely stripped of its hairs, it is covered night and morning with an ointment* of turbith mineral, or with oil of cade. This treatment is to be continued for about a month, after which time the favus may re-appear, preceded by pustules and redness of the scalp. A new depilation, more or less general, followed by the same treatment, is then requisite. After a certain time, some favous crusts may still appear, which necessitate one or two partial depilations. Generally speaking, a steady cure without any danger of relapse is affected by two general and one or two partial depilations. (p. 173.)

In a well-marked case of any of these affections, the diagnosis is easy at first sight. The dry, sulphur, cup-shaped crusts of favus, with their central hairs and mouselike smell, are unmistakeable. So, too, are the round greyish patches of T. tonsurans, with their short brittle hairs, the free ends of which are split and crooked, while their stumps are frequently enveloped in a white sheath formed of the spores of the Tricophyton. Not less easily recognised are the bald, downy patches of Porrigo decalvans, especially when, as is often the case, the skin is somewhat swollen and discoloured, and scantily sprinkled with a greyish powder. But the characters are not always so well marked, and in these cases aid must be sought from the microscope. In Bazin's treatise will be found full accounts of the fungi, with illustrations of the appearance presented by them under the microscope.

The disease which we have spoken of as Tinea tonsurans is found chiefly on the hairy scalp, sometimes on the chin. The same fungus may, however, lodge on other parts of the skin, and then gives rise to the common Herpes circinnatus. Consequently these two affections are to be regarded as mere varieties of one and the same complaint, differing in aspect owing to the difference of their sites. Such, at least, is the opinion of the physicians of St. Louis; and the arguments are strong in favour of this view. The two are not infrequently found on the same individual. Sometimes a circle has been noticed, situated half on the hairy scalp, half on the neck, the first segment presenting the characters of T. tonsurans, the latter of H. circinnatus. Circles of herpes are also often seen on the hands of mothers whose children at the same time present patches of Tinea tonsurans. Hardy and Bazin even consider sycosis to be a third variety of the same affection. But in this they have not the support of their colleagues.

There remain yet three other groups in M. Caillault's classification, which our limits compel us to dismiss with very brief notice. Under the head of cutaneous hæmorrhagic diseases, M. Caillault has brought together two affections which resemble each other in their external appearance, and in nothing else. In so doing he has departed from the principle of classification which he had adopted, and has formed a purely artificial class. The eighth group is a most important one, and one we wish we could treat at greater length. It is formed of the cachectic diseases of the skin. Among these M. Caillault places, as we observed before, some affections usually ascribed to congenital syphilis. Here, too, is placed phagedænic gangrene, and cutaneous diphtheria, which M. Caillault considers to be quite distinct from the serious general disease known by that name. The ninth and last class comprises the inflammatory diseases. The characters ascribed to this group are, that they are not contagious; that they excite more or less general symptoms; that they are often under the influence of disturbed digestion; and that they do not destroy the liability to a second attack. These characters do not seem to us of sufficient importance to form a natural group. In fact, several of the diseases classed in this group clearly belong to other divisions. Such, for instance, is the case with zona and erythema nodosum. These, as we have already noticed, are in children invariably of rheumatic origin, and should be placed in the fifth group of Caillault.

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* Hog's lard, 15 grammes; almond oil, 2 grammes; turbith (sulphate of deutoxide of mercury) 0·50 centigramme.

In bringing this article to an end, we must state that Mr. Blake has done his part well. The translation is very fair, and the few notes added to the text are sensible and to the point.

REVIEW IV.

Traité Philosophique et Physiologique de l'Hérédité Naturelle dans les états de santé et de maladie du Système Nerveux, avec l'application méthodique des lois de la Procréation au Traitement général des Affections dont elle est le Principe. Ouvrage où la question est considérée dans ses rapports avec les Lois Primordiales, les théories de la Génération, les causes déterminantes de la Sexualité, les modifications acquises de la nature originelle des êtres, et les diverses formes de Névropathie et d'Aliénation Mentale. Par le Dr. PROSPER LUCAS. Tomes II. 8vo.—*Paris*, 1847–1850. pp. 1562.

A Philosophical and Physiological Treatise on Natural Inheritance in Healthy and Diseased States of the Nervous System; with Methodical Application of the Laws of Procreation to the General Treatment of Affections based thereupon, &c. &c. By Dr. PROSPER LUCAS.

THIS is a most interesting and elaborate treatise on a most interesting and elaborate subject. There is not, indeed, within the entire range of philosophical physiology, any question more profoundly suggestive than that which embraces the law of Natural Inheritance. We can well appreciate the enthusiasm with which an earnest and scientific spirit would surrender itself to this great and varied field of investigation. Nor do we fail to perceive at once some measure of the difficulties which are in store for it—arising, as they do, not from any paucity of materials relating thereto, but from the enormous number of empirical facts with which the subject is crowded. For the preservation, rejection, arrangement, and generalization of these facts is required no mean capacity. Some of them are the accumulation of ages, and their original observation may be traced even to the remotest antiquity. Their repetition in all succeeding ages has at once increased the number of objective verities, without increasing the perceptive faculties of those who have placed those verities on record. In short, "rayons ajoutés aux rayons" have dazzled the eyes of each successive observer, and destroyed his power of harmonious co-ordination. Moreover, the anomalies and *bizarreries* of this subject are so strange and so frequent as almost to defy, at first sight, anything like a reduction to the beautiful order of primordial law. At times the path appears smooth and unobstructed, and we seem to be on the way to great discoveries. Some startling circumstance then meets us, which upsets our previous calculations, nullifies the exactness of our classification, and darkens with thick doubts what was but a moment since the fruitful land of promise. So that men of the greatest intellectual endowments, and of the calmest judgments, have been forced into a belief of the emptiness and insufficiency of all scientific theories upon the question which has here engaged the attention of Dr. Prosper Lucas.

"Nous n'avons," said the famous Montaigne, nearly three centuries ago, "que faire d'aller trier des miracles et des difficultez estrangières. Il me semble que parmy les choses que nous veoyons ordinairement, il y a des estrangetez si incompréhensibles qu'elles surpassent toute la difficulté des miracles. Quel monstre est-ce, que cette goutte de semence de quoy nous sommes produicts, porte en soy les impressions non de la forme corporelle seulement, mais des pensements et des inclinations de nos pères? Cette goutte d'eau, où loge elle ce nombre infiny de formes? et comment porte elle ces resemblances d'un progrez si téméraire et si desreiglé que l'arrière-fils respondra à son bisayeul, le nepveu à l'oncle?"*

Even the illustrious Isidore Geoffroy Saint-Hilaire, in his 'History of Anomalies,' confesses that, in spite of the lessons taught by the subject, "l'explication complète de tous ces faits est hors de la portée de la science actuelle." And another authority alludes to the entire question of hereditary transmission as belonging less to science than to the mass of empirical facts which too frequently make up "the baggage of practical medicine." Nevertheless, a comprehensive study of the entire question will teach us very much, under the guidance of Dr. Lucas, and enable the observer to predicate with tolerable accuracy certain sequences from certain antecedents, in the relationship of cause and effect. He will learn much, and yet desire with each advancing step to learn more, inasmuch as, in the language of Vauvenargues, "plus on s'élève plus l'horizon s'étend," although with that extension will come (as our author truly observes) an overwhelming sense of the comparative powerlessness of our scientific grasp.

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* Montaigne, Essais, liv. ii. chap. 37.

It is obvious, at the very outset, that this great subject has a *double* aspect.

> "The first," says our author, "is that of the mechanism of our organization—that form of life designated by Burdach under the name of *plastic* existence; an expression which in his opinion, as in ours, embraces the configuration and material composition of the body. The second is that of the *dynamism* or essential power of the organic structure—a power identical with that of existence itself, and which comprises every faculty of animated being, as also every method of its manifestation. Life is incessantly regenerated under these two forms; they are both, therefore, necessary forms of inheritance. Generation transmits by the first or *plastic* path, the various characteristics and states of all the elements of organic existence—the fluids, tissues, systems, organs, and material conformation. By the second, or *dynamic* path are transmitted the various characters and states of every faculty and inherent energy of being. It is the latter which we specially propose to study, and whose history we shall unfold." (pp. 7, 8.)

At the same time, the perfect isolation and independence of the *physique* from the *morale* is quite impossible; for in dealing with the *dynamic* we tread immediately upon the vast complications of the nervous system, and are carried headlong into the very centre of the *plastic*. In fact, says Lafon, "le corps humain vivant et animé n'est que le système nerveux lui-même qui, par ses distributions, par son exercise des fonctions mentales et vitales, par la construction physique et organique de toutes les parties, en constitue l'unité individuelle." It seems indeed, to us, that though Dr. Lucas professes to deal only or chiefly with the dynamic, he finds its union with the plastic so inseparable, that on reaching the end of his elaborate treatise we feel we have had nearly as much to do with the one form as the other.

And not only is life generated under these dualistic conditions: it is generated also under the rigorous application of two unfailing laws. In our author's own words:

> "La première de ces lois est *l'invention:* c'est celle où notre esprit ne suit aucun modèle, où il improvise, ou il compose de soi, où il imagine, en un mot, où il *crée.*
>
> "La seconde de ces lois est *l'imitation:* c'est celle où notre esprit se soumet à l'exemple, celle où il copie, celle où il se souvient, celle où il *répète.*" (p. 24.)

That is to say, the first law is that under which a new individual life is a free and insubordinate creation, isolated even from the inheritance of predisposition. It is an original composition, a fresh type, having no dependence upon the source from which it sprang, and no conformity thereto. The second is that of a regulated and subordinate creation, having a composition analogous to other types; in fact, a repetition of itself, conforming more or less to ancestral or other well recognised characteristics. Every animal born into the world must be classified under one of these two heads. It is either *improvised* by nature as a new type of individuality, to be or not to be repeated in the generations which are to follow; or it is an imitation of an existing type, stamped with the unmistakeable die of some living reality, linked or not with remote or immediate progenitorship.

Let us look for a moment at the "law of invention" as it affects the purely plastic or physical conformation. Now, admitting the truthfulness of the narrative which traces all mankind from Adam and Eve, it is clear from the history of their two firstborns that "invention" obtained very soon after the primæval pair caparisoned themselves with fig-leaves. We have at once an argument for the improvising and inventive powers of what we call Nature in the two distinct types of being, physically and dynamically, which Cain and Abel present to us. She begins her varieties at the very beginning of the Mosaic record. She invests a wolf and a lamb with humanity at the very outset of her creative career. And truly under the human garb these two great types have been perpetuated, with endless and subtle varieties, from that period until now. In fact, diversity of species of race, and of individuality, is a living and primordial law. Independently of every other fact than what we term individuality, it is within the experience of all of us that in the plastic characteristics of life—configuration, lineaments, structure, proportion—everything changes, differs, and transforms itself under endless relationship and in endless degrees. For in every species, and race, and personality, there is a measure of tendency to resist the coërcing restrictions of typical identity and resemblance. The Roman poet had an infinite sense of this variety of type when he wrote:

> "Præterea genus humanum, mutæque natantes,
> Squammigerum pecudes, et læta arbusta, feræque,
> Et variæ volucres, lætantia quæ loca aquarum
> Concelebrant circum ripas, fontesque lacusque
> Et quæ pervolgant nemora avia pervolitantes
> Horum unum quod vis *generatim* sumere perge:
> Invenies tamen inter se distare figuris:
> Nec ratione aliâ proles cognoscere matrem,
> Nec mater posset prolem." . . . .
>
> Lucretius: *De Rerum Naturâ*, lib. ii.

The same law obtains likewise under the dynamic form of existence. In spite of the perpetuation of individual types, there is an ever-recurring diversity and tendency to what is termed invention or originality. And in such diversity Nature is unquestionably richer than our most fertile imaginations. The law of imitation, however, is far more comprehensive and universal than the law of which it is the antagonism. The correspondence of an animal form with that from which it springs has its analogue (as Sir Isaac Newton remarked) in the parity of the two sides of the body, and (as Winslow demonstrated) in the system of the double and single bones of the skeleton.[*] Nor does the analogy cease here. Many of our compound organs and tissues are mere multiples of their simple and diminished selves. The muscles are merely repeated fibrous bundles of simple fibres; the salivary glands are rounded masses, the result of accumulated granulations of smaller but precisely similar conformations. The same may be said, temporarily, of the kidney and its capillary vessels, the latter repeating the grape-like arrangements of the former; so also is the formation of the embryo, which repeats itself either transitorily or permanently in every organ. Thus, Nature paves the way for, and, as it were, preludes the repetition of type of animal existence, by the repetition of type of individual organs, and by the analogy of every element of which those organs are composed. Thus, imitation, the original principle of repetition, is a law of organization and of terrestrial life, so profound and so primordial, that life and organization are inseparable from it; and at the very dawn of embryotic development this principle establishes itself, and weaves its influence into the elements of the plastic being.

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[*] Brouzet, in the last century, went so far as to inquire whether the fœtus which gradually developed itself in the womb was not the result of two entire bodies, one of which belonged to the father, and the other to the mother. "Cette idée," he remarks, "ferait présumer que notre corps est double, et que nous sommes composés de deux corps finis artistement adossés l'un à l'autre. La symétrie de nos organes, l'arrangement des parties extérieures, et les phénomènes de plusieurs maladies semblent confirmer cette opinion."—Essais sur l'Education Médicinale des Enfants et sur leurs Maladies. Paris, 1754. See also Professor Flourens, in his Mémoires d'Anat. et de Physiol. Comparées. Paris, 1844.

But it is not only with the mechanism of life that this primordial law obtains; nor is its operation confined exclusively to a physical sphere. It presides likewise over the entire dynamic range, and conditions the moral and intellectual being of untold generations.

> "It is sufficient to state," says our author, "that uniformity, as diversity, is at once dynamically revealed under the type of species and individuality; and that under these two types, for the very same reasons which demonstrate the primordial nature of the uniformity of the plastic characteristics which constitute species, uniformity of moral attributes is necessarily primordial also."*

We now recur to the law of *invention*, for the purpose of inquiring to what extent and in what relationships it passes, under the name of "innateness," from *creation* to *procreation:* that is to say, to what degree beings are born different from the individual and specific type of those who engender them. Here, in fact, we are at once brought face to face with Mr. Darwin, and may be led into endless speculations on the 'Origin of Species.' Dr. Prosper Lucas asserts energetically and dogmatically, that fixity of species is absolute, and that no natural fact is to be met with which can invalidate this great primordial law. He dissents from the opinions of De Maillet, Bauman, Robinet, De Lamarck, Geoffroy Saint-Hilaire, and Barduch (who is more reserved and cautious than the others), and observes: "No; *species* neither appear as new, nor change, nor thus disappear in generation; so that each of them has had a distinct creation, each of them remains faithful to its nature, and to its physical law of existence; and so long as they live, they remain what they were, or they are no more."

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* Speusippus, the nephew of Plato, composed a work in ten books, entitled *Διαλόγοι τῶν περὶ τὴν πραγματείαν ὁμοίων*, in which he endeavours to prove that the principle of *similarity* is the one ruling feature of nature, and where he tries to determine the resemblance between species of animals and species of plants.—Histoire de la Philosophie, tom. ii. p. 398.

It may be desirable to observe that great confusion has arisen from a misapplication of the terms "unity" and "uniformity," as though they were synonymous. This was peculiarly the case with Cuvier and Geoffroy Saint-Hilaire. "On a confondu," says Dr. Lucas, "sous le nom *d'unité* deux idées très-distinctes: les idées *d'unité* et *d'uniformité*." "Animality" really represents and embraces both; both receive from it an expression of life, and assume thereby a real and objective existence; but the great question to determine is, what they are by themselves, and where they so exist. *Unity* is in itself the harmony of similar and dissimilar. Thus, all nature is in unity, but not in uniformity, for the latter is the correspondence of like with like, and is restricted to the less comprehensive range of classes, orders, individuals, &c.

> "Logical induction, observation, time, history—these are the monuments of life and death, upon which is based the fundamental fact of the immutability of species; and the principle underlying it is the proof, as it is the reason, of the general formula which we desire to establish: *que dans la procréation, sous le type spécifique, l'invention n'agit plus; que sous ce type, en un mot, il n'existe point d'innéité normale et générale dans l'être.*" (pp. 99-100.)

Is this position confirmed by what we know of the geological succession of organic beings?

> "New species (says Mr. Darwin) have appeared very slowly, one after another, both on the land and in the waters. Lyell has shown that it is hardly possible to resist the evidence on this head in the case of the several tertiary stages; and every year tends to fill up the blanks between them, and to make the per-centage system of lost and new forms more gradual."*

This distinguished naturalist admits, however, that "when a species has once disappeared from the face of the earth, we have reason to believe that the same identical form never reappears." Its very extinction is the result of its immutability; for modification and improvement are the best assurance of perpetuity; and every organic form which evades the law of change is liable to ultimate extermination. The entire subject, in Mr. Darwin's opinion, is a "gratuitous mystery," because we do not classify and co-ordinate the facts relating thereto which really lie at our disposal. Facts, however, are viewed so differently, even by the same observers at different periods and under different phases of opinion, that mystery seems almost inevitable. Even Dr. Lucas himself appears to have modified his convictions, since in the second volume of the work before us (published three years later than vol. i.) he alludes to the question as dark and pregnant with difficulties, embracing at once the most obscure points of the physiology and pathology of natural inheritance, and modifying species and individuals. (vol. ii. p. 437 *et seq.*)

It may be more interesting, therefore, to leave the large and vexed question of mutability of species, in order to note some of the changes and diversities which crowd the more limited field of personal or *individual* life. The frequent want of similarity between parents and children, and between children of the same parents, is unquestionably very striking. Even twins, Pierre Bailly has observed, are different in voice, lineaments, gesture, carriage, &c.; and the Siamese celebrities who were united by the umbilicus, were neither of the same height nor of the same character of expression. Not uncommonly the evidence afforded by varying stature towards the illustration of the law of diversity is well worthy of note. Dwarfs are begotten by parents of ample development and size, and subjects of remarkable height acknowledge the progenitorship of insignificant stature.[*] In the same way, children of the most engaging appearance have a double parentage of the commonplace and even unprepossessing kind, and at the same time the coarsest and even most repulsive children are begotten of parents who have everything to recommend them in the way of personal attractions.[†] Again: there is endless variety in the temperaments of children born of the same parents. Pierre Bailly, indeed, says that they nearly always differ from each other, but it is probable that this statement is a considerable exaggeration. Müller and Blumenbach are, though in a more restricted sense, of the same opinion; and the latter author mentions the case of twin sisters united at the bottom of the back, who lived to the age of twenty-two, and whose temperaments were extremely different, although the same blood flowed in their veins, for their vascular systems were connected by a very large communication.

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* On the Origin of Species, &c., by Charles Darwin, M.A., p. 312. London, 1859.

> "Experience (says Dr. Lucas) has conducted physiology, philosophy, and theology to the same conclusion, in humanity; for all three are of one accord as to the fact of native diversity of character and intelligence. It was to this effect that the Sorbonne and the majority of theologians pronounced when they entertained the long-disputed question of the original equality or inequality of souls. They denied the natural equality: they even denied that inequality proceeded from differences of organization." (vol. i. p. 146.)

We confess to a very limited respect for the opinion of theologians touching any large and comprehensive question which extends beyond the limits of their peculiar province; and certainly the Sorbonne is never presented to our minds without the accompaniment of Casaubon's answer, "and what have they settled?" when he was shown over the famous Hall in Paris, and told that there "the Doctors met and *disputed* for three hundred years." That they were partly right, however, in respect of the present question, there can be little doubt. Native differences are a historic fact which is not to be disturbed by Mr. Buckle's theory of "progress of opportunity," however much light "progress of opportunity" may cast upon moral and intellectual diversities."[‡]

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[*] No observant traveller in Italy can have failed to note the frequency of dwarfs among a well-grown and handsome people. The subject is alluded to (if we remember rightly) in that charming book, The Diary of an Invalid. There is reason to believe that these dwarfs are chiefly accidents of individual life escaped from the coercement of hereditary predisposition; for their parents are generally tall and well-developed. In Milan, where, perhaps, in both sexes, Italian humanity attains its most perfect development, the number of dwarfs is quite extraordinary. The fact that they are born of tall and handsome parents is yet further confirmed by the circumstance that the procreative faculty is frequently limited in dwarfs themselves.

[†] Travellers have frequently been astonished at the ravishing beauty of some of the Roman female peasantry, and contrasted the same with the hideousness of their maternal hags. But it is not sufficiently recognised that the Italian physiognomy is of an unenduring type. Beauty fades away very rapidly under the Italian skies.

[‡] Authors have endeavoured to point out remarkable differences in the characters and qualities of famous brothers. Livy, Suetonius, and Plutarch are replete with traditions to this effect from Roman history, and instance in particular the families of the Gracchi, Tarquin, Vespasian, and Severus. In France, the brothers of Charles IX., in the Valois branch, and the brothers of Louis XIII., Louis XIV., and Louis XVI., in the Bourbon branch, had no equality, nor conformity of tastes, capacity, or character. So also in the Buonaparte family. None of the brothers had anything in common with the great Napoleon.

And to such an extent are these mental and moral differences sometimes carried, that all trace of similarity is occasionally lost among members of the same family. Parents of the most limited faculties and unprepossessing characters are gifted with children of the happiest dispositions and most unquestionable talents.[*] So also the most gifted parents are often doomed to the trying discipline of unintellectual and even idiotic children. These wonderful contrasts have given rise to the proverb: *Heroum filii noxæ et amentes Hippocratis filii.* What, then, is the explanation of the fact that so many imbeciles engender men of great capacity, and so many capable men beget imbeciles?

> "Par quel singulier jeu de la nature, s'écrie un auteur, du sage Périclès, peut-il sortir deux sots, comme Parale et Xanthippe, un furieux comme Clinias? de l'intègre Aristide un infâme Lysimaque? du grave Thucydide un inepte Milésias, un stupide Stéphane? du tempérant Phocion, un dissolu Phocas? de Sophocle, d'Aristarque, d'Aristippe, de Thémistocle, et de Socrate, des fils plus vils que la pituite?" (vol. i. pp. 156-7.)

It may reasonably be questioned whether M. Moreau has not furnished an approximate solution of this difficult problem in his remarkable treatise on nervous diseases, in their relation to the "philosophy of history," which we took occasion to review in this journal some time since.[†] Our readers may remember that the burden of M. Moreau's elaborate work was an endeavour to prove that the highest state of intellectuality was neither more nor less than a most dangerous disease of nerve-tissue. And certainly the many instances recorded by the French psychologist furnished no slight evidence of the justice of his theory. It would seem that Nehusins and Burdach are much of the same opinion, for they affirm that superior men are generally the offspring of simple parents; while, conversely, the most brilliant parents are afflicted with the most mediocre and stupid children.[‡] We have no space to enter upon the various causes which Dr. Lucas has enumerated as exercising an influence upon the diversities of character and individual type everywhere to be met with. The researches which he has made upon this part of his subject betray an immense amount of labour, and evidence as intimate an acquaintance with ancient as with modern literature. But while we admit the fact of diversity, we are by no means prepared to indorse all the positions advanced by our author, or subscribe unhesitatingly to the dictum of M. Bonnet: "Le germe porte l'empreinte originale de l'espèce, et non celle de l'individualité: c'est très en petit un homme, un cheval, un taureau, etc. Mais ce n'est pas *un certain homme, un certain cheval, un certain taureau.*"[*] For we are persuaded that in the majority of instances the individual is as strongly marked with the individual as the specific type; and that frequently the coercements of hereditary predisposition are so tremendous, even from the first hour of life, as materially to interfere with the free agency of the subject. Where, however, diversity is apparent, as it constantly is, the fact should be acknowledged as a law only of a less certain character than the law which is its antagonism. -It can never be right (as Dr. Lucas observes) to designate variety in itself, an anomaly of nature, a simple accident, or an aberration from the law of hereditary transmission, although Aristotle has given the great weight of his name to such a mode of thought and classification.[†]

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[*] When the celebrated Tillotson was installed Archbishop of Canterbury, he offered up public thanks to God, in that He had given him some talent, and preserved to him his reason. He made special allusion to the circumstance of his mother's long-standing insanity, and thereby of his own chance of receiving by the path of seminal transmission this terrible infirmity.—Bishop Burnet.

[†] Brit. and For. Med.-Ch. Rev., Oct. 1859. Art. French and German Psychology.

[‡] The *bizarreries* of birth are sometimes as remarkable as they are inexplicable, as the experience of many of us can testify. Peibroux and Fernel both record instances of deaf and dumb born from parents of the most perfect sensorial organizations, whilst Bouvyer Desmortiers has related an example of parents endowed with integrity of speech and hearing begetting, by a kind of periodicity, deaf and dumb offspring, and offspring equally gifted as themselves. More curious still: in the Anecdotes of Medicine (tome ii. p. 241) we read of a family who resided alternately at Paris and Bordeaux, the children of whom born in the latter city were all deaf and dumb, whilst those born in the former were all gifted with a highly-cultivated sense of hearing.

It is not true, in any sense, that variety is an anomaly of generation, or that a deviation from the hereditary type constitutes a claim to monstrosity. In fact, hereditary differences are not varieties of the dissimilar, but only alternations in similitudes, constituting a beautiful and important end in the providential arrangements of the highest form of organic life. They are constant and regular phenomena of every stage and degree of procreation, dependent, in numberless cases, upon influences superior and anterior to the specific act which initiates fœtal development. Moreover, diversity of individuals in the unity of families has the strictest analogy in diversities of families with unity of race; in diversities of race with unity of species; in diversities of genera with unity of orders; and in diversities of orders with unity of class. We do not seek, then, for the solution of this problem in the far-off and mystic dreams of the astrologist, nor in the ignorant superstitions of those who believe in demoniac influences; nor yet in that unscientific theory which would dwarf the spirit of inquiry by attributing everything which has the least semblance of the inexplicable to the direct and personal interference of the Deity. "Remonter à Dieu, c'est remonter à l'auteur et non pas à la loi, et c'est la loi que la science aspire à pénétrer." It is to be found in the page of the natural world which is ever open to the earnest searcher after truth, and which, day by day, reveals its treasures more abundantly to those who duly value and appreciate them.

We leave now the question of diversity for that of identity, merely observing, *en passant*, that to the eye of the philosophic physiologist there is strict identity between the power which *creates* and the power which *procreates*. The active principle of the one is the active principle of the other.

> "Ce ne sont point les êtres, à proprement parler, qui se reproduisent, c'est a Nature qui crée, en eux et par eux, dans la génération: organes imperson-

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[*] Considérations sur les corps organisés, tom. ii. ch. 7, p. 219.

[†] "He who has no resemblance to his parents is a sort of monster, for nature has deviated in him from his species; it is the first degree of degeneration."—De Gener. Anim. iv. 3.

nels de son activité, ils ne sont, devant elle, que de simples instruments encore tout pleins du Dieu de vie qui la possède, et par l'intermédiaire desquels elle renouvelle, selon les lois primordiales de l'institution des êtres, l'œuvre qui les a crées." (vol. i. p. 189.)

And first, as regards the hereditary transmission of the physical characteristics, and the various material elements by which identity of structure is made manifest. The resemblance of figure and general conformation in all animals to the parental type has been recognised by Hippocrates, Aristotle, Galen, and almost every ancient and modern observer. Even the distinguished Bonnet, who minutely investigated this subject, and whose leanings were all to the opposite side of the question, admitted, at the close of the last century, that he could no longer withstand the evidence in favour of transmitted identity which he everywhere met with.* This resemblance extends itself to physiognomy, gait, and all the minutiæ of physical structure. It was its recognition, in the largest sense, which inspired the pen of Horace, when, marking only by the comparative degree the difference between youth and age, he wrote: "O matre pulchrâ filia pulchrior."† The graceful lines of Virgil, in which Juno addresses Æolus, more abundantly testify to the same verity:

"Sunt mihi bis septem præstanti corpore Nymphæ,
Quarum, quæ formâ pulcherrima Deïopeiam
Connubio jungam stabili, propriamque dicabo:
. . . . . . Et pulchrâ faciat te prole parentem."‡

In a more real and substantial manner have Haller, and Portal, Giron, Burdach, and countless physiologists evidenced what the "tuneful numbers" of the Roman poets have recorded. So strongly was the truth of this physiological rule impressed upon the inhabitants of Crete, that they enacted a law by which the most beautiful of every generation were forced into matrimonial alliances which would best ensure the propagation of their respective types. Our author does not fail to allude to the circumstance, that the great perfection to which we have brought stock-breeding in England is owing to an instinctive perception, verified by ample experience, of the capacity which every organic being has for begetting a duplicate of itself. And he specially mentions (what he terms) "*The Leicester-spire improved bread*" of sheep, as the result of a careful study of the physiological law of hereditary transmission, and of the animo-chemical law which demands variety of alimentation.

It is obvious that, however well marked may be the physical characteristics transmitted by a parent to its offspring, our opportunities of marking the force of the law upon which the fact is based are largely multiplied in the field of morbid anatomy. The type of a particular disease is often reproduced with a marvellous faithfulness in succeeding generations. Facts crowd upon us from every side to attest this verity, and to warn us against the adoption of any course of indiscretion or sensuality by which we may originate in ourselves what, once developed, may become a fatal heirloom to those who may hereafter trace from us a direct lineal descent. So unfailing is the power of the hereditary law in certain families, that observers have remarked that in transmitted diseases (such as hysteria and chlorosis) which are almost the peculiar privilege of the weaker sex, the brothers of sisters so affected have by their pallid skins and excito-motory irritability given double evidence of a blood alliance with the victims of the hysteric and chlorotic diatheses.* That there are occasional intervals (so to speak) of physical integrity, we are well aware. That is to say, there are *individual* instances in many *families* in which an immunity is afforded to a particular member of the same from the morbid scourge to which the collective group is hereditarily obnoxious. So also a like immunity is afforded to an entire *family* or *generation*, the disease having but suspended for awhile its influence, to re-appear in the subsequent generation with undiminished vigour.†

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* Op. cit. tom. ii. ch. viii. p. 219 et seq.

† Lib. i. Od. 14.

‡ Æneid, lib. i.

In connexion with the hereditary aspect of the pathological field we may allude to certain idiosyncrasies, frequently of so capricious a character as to attract the notice of the most unobservant. In some there is a peculiar susceptibility to the influence of certain poisons and drugs. A brief and transitory smell of paint will, it is said, in a few hours define a blue mark upon the gums. The minutest quantity of mercury will salivate, or the smallest dose of opium or coffee produce an urticaria, or some form of convulsion. In the same way, and upon the same principle, so susceptible are some persons of certain of those exanthematous poisons which ordinarily, once expending their virulence upon the organic system, shield the sufferer from the danger of subsequent exposure to the same, that it is not uncommon for them to pass several times through the stages of small-pox, scarlatina, or measles. On the other hand, there are certain states of system, yielding no nidus wherein can be deposited the fatal exanthematous ova, pregnant to so many with the germs of death. And these idiosyncrasies are transmitted with wonderful fidelity from one generation to another.* Again, there are numberless instances of an hereditary dislike to the smell of cheese or beer. The famous Montaigne had a most extraordinary antipathy against medicine, which he acknowledges having received through the hereditary channel from his father, grandfather, and greatgrandfather, and which he carried to such an extreme, that when told by his physicians that death was certain unless he took something which they had prescribed for him, he exclaimed, "Je suis donc que mort."†

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* See Dr. Marshall Hall, 'On the Diseases and Derangements of the Nervous System.' London, 1841. See also Dr. Ashwell's Memoir on Chlorosis and its Complications.

† There is a very remarkable case recorded in the Cyclopædia of Practical Medicine (vol. ii. p. 418), which well illustrates the above phenomenon, and shows (what Dr. Lucas calls) "le double concours de l'innéité et de l'hérédité à sa production." The father of a family lived to the age of eighty-six and always enjoyed perfect health. By marriage with a healthy woman he had twelve children—five sons and seven daughters—of whom three sons and a daughter died of hæmorrhage. The youngest of the daughters, *who never had a symptom of this predisposition*, married a healthy young man, by whom she had four sons and two daughters. Three out of the four sons also died of hæmorrhage. There was no evidence to prove that a trace of this disease existed in either parents, nor on either side, prior to the generation of which the latter mother was the youngest.

Those who are at all familiar with the breeding of animals are aware how a particular colour, or some other characteristic, will fail in a particular generation, and appear again in the next. It is common, for instance, for the progeny of a perfectly white boar and sow to be parti-coloured, or even quite black. Enquiry in these cases into ancestral characteristics will in general elicit the circumstance that colour existed in the previous generation.

One of the most important and curious divisions of this interesting subject is that which involves the duration of life. It is scarcely necessary to remark that the tables of the many assurance offices with which England is now flooded are based upon a general calculation of averages. And it is obvious that in so colossal a matter no other sort of computation could, in fairness, be admitted. At the same time, the actuaries of the various offices are instructed, by the printed forms in use, to institute inquiries as to the longevity of the parents of those seeking assurance, so that the company may be furnished with every legitimate reason for declining the life presented to it. In many families, where a minute physical examination of its individual members might give no evidence of disease, the limit of life could be predicted from hereditary antecedents. "Precocious deaths," says Burdach, "are in some social circles so common, and so expected, as to render precautionary measures absolutely useless." So impressed is Dr. Prosper Lucas with the truth of this assertion, that he thus appeals to us to fortify his own conclusions:

> "What physician, nay, what man, is there who cannot call to mind like examples, and whose experience does not testify to the powerlessness of art to put off this premature approach of the closing hours of existence, or to prolong for even a few moments the last pulsations in those doomed and mournful families where life has but one age, and death has but one form?" (vol. i. p. 256.)

Equally true, as the statistics of many countries can testify, are the relations of prolonged life extending itself over many generations, defying the calculations of actuaries, and disappointing the hopes of expectant heirs. With the increasing civilization (which is only another term for the grinding pressure) of the age, will probably diminish the number of modern Methuselahs, and the recurrence of anything so ludicrous as the following out of many anecdotes with which our author has enlivened this part of his work:

"On the 31st of July, 1554, the Cardinal d'Armagnac, passing along the street, saw an old man, eighty-one years of age, weeping at the door of a house. On his Eminence demanding what was the matter, the old man replied that his father had just been beating him for passing his grandfather in the street without saluting him. The father was 103, and the grandfather 123 years of age."* (From the 'Etrennes Historiques de Gessey, 1753.')

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* "Il parait certain, que nous contractons de nos parents la disposition à avoir ou à n'avoir pas la petite vérole, d'après des exemples très-nombreux des générations qui n'ont jamais eu cette maladie, malgré qu'elles n'aient rien fait pour l'éviter, malgré *la tentative répétée de l'inoculation.*" Fodéré, Traité de Médecine Légale, 2nd edit., 1813. Tom. v. p. 360. Fodéré cites some most remarkable cases, in which for several generations every member of a family enjoyed a transmitted immunity from the influence of small-pox.

† Essais, liv. iv. ch. 37.

There are few of us, it may be, who have not received in childhood, upon parental authority, the "early to bed and early to rise" maxim, fortified by the additional advice of certain dietary precautions. Without implying for a moment that the advice is not very good, or that its propagation is not to be encouraged, we are persuaded that too great an efficacy has been attributed thereto, and that early rising, sobriety, and alimentation, have but a secondary influence in the production of longevity. By which we mean, that a family to whom has been given, hereditarily, a prolonged tenure of life, will, individually and collectively, enjoy and transmit that longevity to the succeeding generation, in spite of deviations (not too extravagant) from the strict rule of health, which might prove detrimental to others not so protected. Again, the most careful observance of dietetic and hygienic rules cannot protract the briefer span which, through the hereditary channel, has become the lot of others. "Beaucoup de sobriété, nulle inquiétude, les sens et l'esprit également calmes," is a very good receipt, but it is not infallible. There are natures born to different things, and requiring different things: and there are natures also upon whom is stamped so legibly the die of longevity, that they can defy, within a reasonable limitation, all the rules of the dietetic, and despise the conventional traditions of the sanitarian. In fact (as our author truly remarks), "everything demonstrates that longevity is an inherent power of vitality belonging to certain privileged individuals. That vitality is so peculiar, and its energy so generally and profoundly impressed upon their natures, that it characterizes all the attributes of their organizations." (vol. i. p. 279.)

The maximum of longevity is reached, according to Burdach,† in climates where extremes of temperature do not obtain. Dr. Prichard‡ is of the same opinion, while Lejoncourt includes Russia in his category of centenarian countries. He says, in that most remarkable book, the 'Galérie des Centenaires'—

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* A French author, Charles Lejoncourt, has instituted a comparison between the family of Abraham and that of a modern circle, which gives but a slight preponderance of longevity to the "father of the faithful."

	Years.
Sarah Dessen, wife of John Rowir, lived	164
John Rowir	172
Their eldest son (still living, but lost sight of at the beginning of the present century)	115
	451

	Years.
Sarah, the wife of Abraham, lived .	127
Abraham	175
Isaac	160
	462

† Traité de Physiologie, tom. v. p. 396.

‡ Natural History of Man, vol. i. p. 245.

> "A l'exception des parties de l'Inde, où règne un printemps perpétuel, et où la vie de l'homme atteint quelquefois à ses dernières limites, il est prouvé que la patrie des centenaires se trouve en Europe, dans les régions du nord, telles que la Grande-Bretagne, l'Allemagne, et la Russie, tandis que l'existence est, en général, de peu de durée dans les climats chauds, tels que l'Espagne ou l'Italie, et que la France, située à l'est, tient le milieu."

Buffon, on the other hand, says that "la différence des races, des climats, des nourritures, des commodités, n'en fait aucune à la durée de la vie."*

Passing by that part of Dr. Prosper Lucas's treatise which is occupied with the transmission of anomalies of specific types of organization, whether arising from excess or deficiency of development, we enter upon the large and interesting subject of the lineal inheritance of our moral natures. If it be true (as we think it is) that our immaterial attributes are the result and consequence of our material organization, and that (to use the energetic expression of Voltaire) "le physique est le père du moral," it is obvious that anything approaching to a proof of the hereditary character of the one, is necessarily a proof of the other. But it is a vast and intricate question which we need not apologize for attempting to elucidate. This may be done by seeing what testimony is borne to the affirmative of dynamic transmission, by proofs of authority, and by proofs of experience. By the former (to which we shall but very briefly allude) is meant the influence of religions, institutions, and morals, of men, and times, and peoples. By the second is meant the pure and simple observation of facts, the fundamental source and common basis of all theories and systems.

1. The strict dependance of the moral nature of beings upon those from whom they have been procreated, is a fact recognised from all antiquity, and engraved upon the most ancient monuments of religious faith. The Vedas and the sacred codes of the Hindoos push this principle even to the most intimate identity of paternal and filial personalities. "Un mari, en fécondant le sein de sa femme, y renaît sous la forme d'un fœtus, et l'épouse est nommée Djaia, parce que son mari naît (Djaîati) en elle une seconde fois."†

And there is no doubt that the institution of castes and the prohibition of sexual intercourse between them, is as much a civil and social as a sarcerdotal invention, having its origin in the recognition of natural inheritance.

> "Without pretending (says Dr. Lucas) to reject auxiliary causes, such as conquest, differences of race, &c., which favour the translation, in law and in social fact, of this instinctive faith of humanity, it seems to us, for our part, impossible to deny either the sincerity or the intervention of this natural belief, in the original institution of castes, in presence of the texts themselves

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* Histoire Naturelle, tom. iv. p. 357. Fifth edit. 12mo.

† Manava-Dharma-Sastra, *Lois de Manou*, comprenant les institutions religieuses et civiles des Indiens, traduites du Sanskrit, et accompagnées de notes explicatives, par Loiseleur des Longchamps, 1 vol. in 8vo, Paris, 1833, liv. ix. st. 8. (Largely quoted by Dr. Lucas.)

by which they are established. All doubts should fall to the ground before the most ancient code, where we may read an exposition of the motives for this monstrous distribution of men—before the 'Manava-Dharma-Sastra,' which traces itself back as far as the thirteenth century before the Christian era." (vol. i. p. 344.)

One of these texts assigns a particular and specific origin in the body of the Universal Brahma to the four primitive castes still existing in India : "Pour la propagation de la race humaine, de sa *bouche*, de son *bras*, de sa *cuisse*, et de son *pied*, il produisit le Brahmane, le Kchatriya, le Vaysia, et le Soûdra." Or, in other words, these four specified parts represent severally the sacerdotal type of humanity, the warlike type, the agricultural and commercial type, and, lastly, the type of the slave. Another text speaks of the absolute impossibility on the part of the offspring to escape from the procreative type, and points to the coercing power of the hereditary law.

> "Un homme d'une naissance abjecte prend le mauvais naturel de son père, ou celui de sa mère, ou de tous les deux à la fois: jamais il ne peut cacher son origine."

Again :

> "Quelque distinguée que soit la famille d'un homme, s'il doit sa naissance au mélange des classes il participe à un degré plus ou moins marqué du naturel pervers de ses parents."

And now *raceless* classes are collectively designated by the law as *impure* classes : all are equally excluded from sacrifices—all predestined to the vilest functions, and counted to be of no more value than the beasts of the field. So that the very thing most calculated to effect a physiological, and thereby a moral and intellectual improvement in mankind—that of enlarging the field of selection, and multiplying the possibilities of new types—is forbidden by the stringency of fanaticism, and the miserable coercements of a traditional faith.

We are very far from feeling sure that in our own land something akin to this influence is not, though in a more subtle manner, in extensive operation. There is, indeed, an instinctive social propriety in the limited exercise of such an influence ; but it is obvious how it may be permitted, under the exacting discipline of certain creeds and opinions, to attain too great a magnitude. There is a more intimate connexion than many will acknowledge, between physiological instincts and modes of spiritual aspiration. Particular "schools" of religion have an undue tendency to exclusiveness, both as regards the kingdom of earth and the kingdom of heaven. The members of these respective schools, whose surroundings in all the details of social and sentimental life have a most impressive sameness, pair like pigeons, and beget increasingly degenerate types, with no capacity for original thought, and no vigour for independent action. Such and so great at times is the power of educational sameness, together with sameness of faith and profession, that a force equal to that of consanguineous alliance is brought to exercise its deteriorating influence upon men in the highest position and authority.

Without pretending to any great lore on the science of Lavater, we are persuaded that we could, in numberless instances, unlock the religious tendencies by the detective key of physiognomy. And we have been impressed with a belief that certain physical temperaments are parcelled out into certain forms of religious belief. Hysteria, for the more part, inclines to take one view of religious duties; and diseases of a less excito-motory character, another. Whereas, for a thoroughly healthy balance of all the elements which make up the socio-religious fabric, and ensure the perpetuity of the species, the moral and physiological borders of selection should be greatly extended. Such opinions, indeed, are at variance with the wisdom of Manava-Dharma-Sastra; but they are not on that account necessarily untrue, even in the face of the text, which says: "Toute contrée où naissent ces hommes de race mêlée, qui corrompent la pureté des classes, est bientôt détruite, ainsi que ceux qui l'habitent." We would wish even to extend the application of these remarks to that legal obligation of children in all the primitive castes, and even in impure and mixed classes, to follow the employment of their progenitors.[*] Aptitudes are not so hereditary, nor arts so difficult to acquire, as to render it at all necessary to limit the ambition of the young, or dwarf the development of diversities of natural tastes. In fact, under a perfect system of education, every effort would be made to free young minds from the thraldom of the hereditary law, and afford them opportunities for the culture of an entirely new field of observation. By lessening the power of the hereditary law we at once lessen the moral responsibility of parents, and augment that of their children. And we cause to stand out more prominently from the historic page those penal enactments which, in China and Japan, render an entire family responsible for certain crimes committed by one of its members.

From the tenor of the above observations it will be gathered, that while we admit the instinctive sense of the hereditary law to be fortified by history, we are not prepared to adopt the conclusions drawn therefrom by many observers. The physiological answer given to those who would multiply indefinitely the field of selection, is this: "By doing so you mix pure with impure blood, and destroy classes." But this is hardly so, as long as the great ethnological types are kept distinct. Moreover, the analogy between the family of man and the larger field of the entire animal creation, is by no means complete.[†] The characters of nations, and the traits of families, and the individualities of those who make up the collective group, are all capable of shedding an immense influence upon alliances of blood. The annals of the English "stud-book," to which our author has alluded, do not, in our opinion, support the position antagonistic of that which we are desirous of maintaining. The purity of a breed (*vide* "Stud-book" and "Peerage") is no proof of its usefulness, nor of its aptitude for general purposes. We are disposed, indeed, to think that in the breeding of race-horses, the great expense which notoriously attends it, arises as much from the immense number of losses and failures of stock incidental to (what is termed) highly scientific cultivation of blood, as from anything else. The number of wretched "weedy" animals by sires, and from mares, of the purest breed, surpasses the belief of those who know only the traditional glories of the "Derby" and the "Cup" days, but who have never been initiated into the mysteries of the stable. On the other hand, a cross between a highly-bred race-horse and an animal of less refined appearance and pretensions, produces a useful animal, partaking, for the more part, of the properties of either parent. So, also, in the breeding of pigs and sheep. Of the former, our most approved breeds are now admitted to be the result of a variety of crosses between breeds having distinct qualities, whose blending developes into the ponderous triumph of pinguitude and solidity. So also with our sheep, though it is generally admitted that they are not so susceptible of happy crosses, gifted with the same power of perpetuating themselves, as the porcine tribe. Farmers, however, who breed for general purposes of usefulness (which is remuneration), cross one variety of sheep with another. In the same way, many of the finest poultry for the table are crosses. In America, none of the best table birds (poultry) are of pure breed. But there is little doubt that many kinds of animals (sheep in particular), the result of a blood-cross, are not able to perpetuate themselves, save under the fiat of inevitable degeneration. It may well be questioned whether such a fiat has extended to the human race. History does not prove it to be so. It only proves the instinctive perception and institution of the great hereditary law, which comprises the lesser law of class and family and individual grouping. Certainly, if we bring ourselves within the confines of a limited field, we cannot fail to notice the evil effects of too close an alliance of *similia* with *similia*. To discover sexual opposites which are capable of being cultivated into mutual adaptation, and proclaim to them the primæval mandate, "increase and multiply," is to make no little advance towards the improvement of the species. If the conventional requirements of European courts, and prejudices, and politico-religious differences, were not what they are, we should not now be indulged with the spectacle of monarchs presiding over several of them on the very verge of imbecility, from too intimate alliances of blood.*

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[*] This natural tendency to hereditary professions likewise obtained in Ancient Greece, and even extended itself to all the arts. In Turkey the administration of justice is the property (so to speak) of certain families, who hereditarily exercise its functions. Herodotus also alludes (lib. ii.) to the seven castes then in ancient Æthiopia and Ægypt as remaining faithful to the professions of their fathers.

[†] Professor Lordat, of Montpellier, says, that so certain and unfailing is the transmission of like qualities in animals, that no conclusions therefrom are applicable to the human race. Dr. P. Lucas is of a different opinion. See Les lois de l'Hérédité Physiologique sont-elles les mêmes chez les bêtes et chez l'homme?—Montpellier, 1842.

2. We come now to the *experimental* proofs of the inheritance of

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* Greek and Roman history are not wanting in instances of an analogous kind, where

the moral nature. But we are of necessity compelled to pass over many subdivisions which might engage our attention; merely remarking that Dr. Prosper Lucas records many remarkable instances of the hereditary transmission of certain characteristics peculiar to various modes of sensorial activity. We shall confine ourselves to a more stringent definition than that which comprises the action of the senses, though one sufficiently comprehensive to include "la pluralité du principe de la vie." Who that has heard of Stoics, and Platonists, and Peripatetics, will doubt that as these several schools disputed not the original part played by the hereditary law upon man's higher nature, so abundant evidence is everywhere to be met with to the same effect?[*] Numerous as must necessarily be the exceptions to such a rule, there is no doubt that as a father and mother transmit to their children physical resemblance to themselves, so likewise they transmit the attributes of mind and soul. This is admitted even by those who do not study the question in a material point of view, or regard identity of the psychical, as necessarily dependent upon identity of the physical nature. Compelled at once, "de *philosopher* et de *christianiser* tout ensemble" (as Dr. P. Lucas happily expresses it), Zacchias thus writes:

> "Il est certain que *la plupart* des penchants et des affections de l'*âme* naissent de la semence des parents, ainsi que le corps, *bien que l'âme vienne du dehors, et qu'elle n'émane point d'une force de la matière, comme la vérité catholique l'enseigne le plus généralement.* L'être colère donne le jour à un être colère, l'envieux à un envieux, le superbe à un superbe, le timide à un timide, et l'audacieux à un audacieux, &c."[†]

Some authors, on the contrary, divide the moral qualities of the body and those of the soul, admitting the hereditary character of the one ("la force vitale,") and not of the other ("le sens intime.") Some, again (as Jacob Boëhme), affirm that the soul is propagated hereditarily through the physical channel. When both parents (says this writer) are bad and "devil-possessed" at the time of fruitful copulation, a bad soul is sown. And in the ardour of his convictions he winds up by this apostrophe: "Faites attention à ceci, vous, mauvais

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the law of succession has been so limited as to render necessary the evil of consanguineous marriages. M. Girou de Buzareingues says:—"Les rois d'Egypte épousaient presque toujours leurs sœurs. On croirait, dit Anquetil, que ces alliances perpétuées dans les familles, de race en race, auraient dû être un gage perpétuel d'amitié et de concorde: ce fut, au contraire, le germe des haines qui, non-seulement ensanglantèrent le trône, mais qui firent le malheur des peuples, entraînés par leurs princes dans les guerres civiles."—(Philosophie Physiologique, pp. 312-13. Paris, 1828.)

[*] "Platon, dans les Timée (says Dr. Lucas), va jusqu'à professer que les mauvais penchants ne sont dus qu'à une mauvaise qualité du corps et à une éducation vicieuse, en sorte qu'on devrait plutôt accuser le *père* et *l'instituteur* du méchant, que le méchant lui-même." This opinion is borne out by modern observation, and is in a large measure proved by the statistics of drunkenness and insanity. A distinguished French psychologist, M. Morel, forcibly dwells upon it, as also does the present able Professor of Modern History in the University of Cambridge. See also on this subject, Quetelet's Recherches Statistiques sur le Royaume des Pays-Bas, Bruxelles, 1819; also his Essai sur le penchant au Crime aux différents âges.

[†] Pauli Zacchiæ, *Quæstion. Medico-legal.*, Avenione, 1755, lib. i. pp. 115-16-17-20.

parents; vous ramassez de l'or pour vos enfants, ramassez leur une bonne âme, cela leur sera plus utile."[*]

Lastly, among those who have believed in the complete transmission of all the moral and spiritual faculties through the medium of the material organization, are Voltaire, Lamettrie, Gall, Spurzheim, Portal, Giron, Burdach, and Müller. And what other channel is there so natural or so obvious, for the inclosure of the hereditary stream of the psychical, as that by which we learn and marvel at the mighty torrent of the physiological? Species, and race, and family, and individuality are included in it; and a like transmission of natural character is attested by all ethnologists. Nor is a full recognition of the rolling of this hereditary stream other than consistent with (to continue our metaphor), the occasional eddies and backwaters which represent the less comprehensive law of the "Innate." To this law we have before alluded as at times influencing the *dynamic* as well as the *plastic* forms of life, and largely diversifying the morals, inclinations, and qualities of children, however well marked and different may be those of their parents.[†]

We have neither time nor space, nor indeed inclination, for any discussion of the various theories which have obtained respecting the origin and transmission of the soul. All that Origen, and St. Jerome, and Tertullian, and Luther, and Mallebranche, and Leibnitz (for theologians and philosophers have alike essayed to solve the problem) have written upon the subject, do not in any sense make it more clear, but only indicate man's incapacity to discuss what he can never understand, because he can institute therewith no finite comparisons. The soul may (as some have supposed) have pre-existed from all eternity; or (as others have thought) it may be a distinct act of creation coëval with the birth of every man born into the world; or (as yet another school have believed) the spiritual part of each created being may be transmitted with his plastic organization. It does not matter. And it is better to confess that "through a glass darkly" we can only study the problem here, until "face to face" the great mystery is made known to us hereafter.

Of the other part of man's higher nature—of the mind as distinct from the soul—it is given to us to know more; and hereditary facts multiply with an increased study of the subject, forcing us to assent to the position of Voltaire, that genealogies are "écrites sur les visages et manifestées dans les mœurs."* Bischoff has truly said, in his 'Treatise on the Development of Man and Mammiferous Animals,' that, alarmed by the idea of what is called "materialism," we have been accustomed to view this question of cerebral identity in a very narrow manner, placing the organ of the mind under different laws to those which we apply to other parts of our physical structure. Even a distinguished French physiologist has written:

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[*] Jacob Boëhme: Quarante Questions sur l'origine, l'essence, l'être, la nature, et la propriété de l'âme; traduit de l'Allemand. 1 vol. Paris, 1807. (Quoted by Dr. Lucas.)

[†] So persuaded was Plutarch as to general identity of transmitted structure, that he says:—"Generated beings have no resemblance to productions of art. In respect of the latter, as soon as they are finished, they are immediately separated from the hand of the workman, and have no more connexion with him. They are made *by* him, but not *of* him. On the contrary, that which is engendered proceeds from the very substance of the generating being, in such a manner that it holds from the generator something which is very justly punished or recompensed for him, for that something is himself. The children of vicious men are derived from the very essence of their fathers. That which the latter possess of the principle of life, that which lives and nourishes them, and constitutes their very thought and speech, is precisely what they give to their children. There is nothing strange, then, in the fact, nor is it difficult to believe that there is between the generator and the generated a sort of occult identity, capable of subjecting the one to all the consequences of an act committed by the other."

> "Les générations, dans l'espèce humaine, héritent naturellement de leurs parents les formes corporelles, les traits physionomiques, le teint, la constitution chimique, la crase vitale, des diathèses, des dispositions, des maladies futures, le tempérament, les idiosyncracies, soit vulgaires, soit excentriques, et les parties du caractère qui tiennent aux modes saillants de l'instinct; *mais elles n'héritent pas des modes radieaux du sens intime, le génie, la supériorité distinctive*. . . . . Oui, dans l'espèce humaine, l'auteur, *en tant qu'intelligent*, ne transmet aux descendants que *la substance sur laquelle résident les attributs essentiels du sens intime*. Il leur donne sa *spécialité*, son *humanité*, et *les met dans la continuité de la chaîne des enfants d'Adam*."†

For ourselves, we confess that we entirely dissent from these opinions. We believe not only in the constant *resemblance*, but in the very *identity*,‡ of that which constitutes the "sens intime" of our natures. We think that Dr. Rush wrote something approaching the truth when he said, "It is probable that the mental and physical qualities of adults which we so often see in their offspring gifted with great intelligence, may be fixed and determined; and perhaps we shall some day be able to predict with certainty the intellectual character of children, when we know the specific nature of the various intellectual faculties of their parents."§ Nor would the chances of successful prediction be diminished, nor the law of identity be invalidated, by the circumstance previously alluded to, of the frequent begetting of imbeciles by highly gifted individuals. For if M. Moreau's theory be correct, and superior intellectuality not only becomes dangerous by its proximity to, but is actually identical with, disease, these seeming anomalies are not really such, but rather a fuller and more complete subordination to a natural law which points at once to the paths of precariousness and safety. Vandermonde, and Spurzheim, and Da Gama Machado even thought that by attending to the required laws, and employing the same means as were resorted to in ancient Crete, and as exist in modern establishments for the breed of various animals, a *race* of talented humanity might be perpetuated. If under the varying social and educational systems which cover the wide range of history, it is not apparent that great men have transmitted lineally the qualities which made them such, to what cause are we to attribute the circumstance, and what explanation can we offer of the fact? Is it that the *innate* law is superior to the law of *inheritance?* Is it that greatness of any kind is the result of so perilous a combination of material atoms that in transmitting itself it becomes weakness? Is it that, however great a resemblance there may be of outward conformation, or even identity of structure, the accidents of climate and food and education, together with the absence of exciting causes or occasions, interfere with the manifestation of intellectual power similar to that of the parental type? Is it that the individuality of the (so to say) intelligentially weaker parent has, in the seminal blending which was conception and initiated fœtal life, overpowered the individuality of the stronger, and given a preponderance to the animalism which constitutes a larger part of the human family? Is it that the consciousness of transmitted greatness is ever accompanied by that which exercises so deteriorating an influence as to undermine the most colossal genius, and destroy the blossoms of the most hopeful promise? Or does the explanation lie with more probability in some sort of combination of these, and (as Burdach suggests) in that love of harmony which is nature's most pleasing feature, and which leads her to adjust and equalize in the course of ages those inequalities which distinguish shorter periods of time? "Nature tends everywhere to harmony; so, after having taken a great start in certain individuals, she returns promptly to her ordinary measure. Talents die out in a family to whom it is left to live upon the glory of its ancestors, and while ancient races degenerate, new ones are raised up which re-establish a perfect equilibrium."

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* See Dictionnaire Philosophique, Art. Caton et Suicide.

† Lordat, Les lois de l'Hérédité Physiologique sont-elles les mêmes chez les Bêtes et chez l'Homme? pp. 19-23.

‡ It is of importance that these two expressions should not be considered as synonymous. Our author gives their distinctions very happily: "*La ressemblance* n'exprime que les analogies, que les points de rapport et de conformité de deux choses, êtres, corps, ou objets différents. Dans toute la rigeur de son expression *l'identité* exclut non plus la différence mais la distinction, mais la dualité même."—Vol. i. pp. 572–3.

§ On the Influence of Physical Causes on the Intellectual Faculties, p. 119.

Whatever may be the explanation of the fact, certain it is, not only that distinguished men in every department of science and art, and in the spheres of professional life, do not perpetuate their greatness, but that they even do not perpetuate their species.* So striking was the contrast between father and son in the family of the famous Buffon, that it gave rise to the witticism of Rivarol, who, speaking of the younger, remarked:—"C'est le plus pauvre chapitre de l'Histoire Naturelle de son père." M. Benviston de Châteauneuf has instituted some very curious inquiries into this subject, and published the results in his 'Mémoire sur la Durée des Familles Nobles en France.'† Their tendency is to prove the remarkable rapidity with which families rich in all that illustrates greatness in its most comprehensive sense die out and become extinct. The nobility of the sword and of the robe are not alone, he says, obnoxious to the influence of this law. In science, in letters, and in the fine arts, the most celebrated names have disappeared in a few years, leaving behind them but a subjective glory. The family of Boileau lasted scarcely two hundred years, although he had thirteen male children; that of Crebillon counted but one, that of Racine but three generations. Molière died childless; Corneille did not marry; Danville, Bailly, Lavoisier, Condorcet, had only daughters through whom to transmit the stream. And where are the descendants of Byron and Moore? Upon whom has fallen the mantle of the mighty Humboldt? Were they not childless?

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* "Il est digne de remarque, (says Dr. Prosper Lucas) que le mouvement ascendant des hautes facultés d'un assez grand nombre de fondateurs de races, s'arrête presque toujours a la troisième, se continue rarement jusqu'à la quatrième, et presque jamais ne dépasse la cinquième génération. Nous le voyons s'élever, jusqu'à ce dernier terme, dans la race de Pépin, où, de père en fils, se suivent Pépin de Landen, Pépin d'Héristal, Charles-Martel, Pepin-le-Bref, et enfin, Charlemagne, chez lequel il n'arrive à son épanouissement que pour décliner. Il s'arrête au second terme, dans les fondateurs de la race Capétienne, de Robert, duc de France, à Huges-Capet, roi; de même, dans celle des Guises, quelques instants si riche en hommes remarquables, il ne va pas au delà victime de Blois."—Vol. i. pp. 590, 591.

† Annales d'Hygiène publique et de Médecine Légale, 1846, tom. xxxv. p. 27 et seq.

And this kind of evolution of inheritance is not peculiar to the mental faculties, but it receives a fresh sanction from pathological genealogies. Theory also confirms it in every point. Everywhere we find this law of progression and retrogression in the seminal transport of the normal or abnormal attributes of the individual type of being. Nor is it right to say that, studied in its largest grasp, this law subverts the law of natural inheritance. Dr. Prosper Lucas well says:

> "Ainsi, loin que ce fait renverse le principe de la propagation des facultés mentales, il ne fait qu'ajouter à la vérité et à la portée de l'induction de Neuhss, qui ne voit, avec raison, dans ce mouvement évident de l'hérédité de l'intelligence, qu'une nouvelle preuve de sa réalité et comme de sa vie: 'Atque idèo quidem certa est illa paternæ indolis in posteritatem transitio ut, in claris familiis, illa suos veluti natales habeat, et sumpto incremento, adolescat et, senior confecta, deficiat et commoriatur. Eximit se subitò aliqua de vulgo familia, et secundus gratiæ auris, ad conspicuam lucem, ab ignotis tenebris emergit. Eadem, statim obsolescente venustatis splendore, vix majorum gloriam tuetur. Tùm magis magisque extabescens, et eruta hominum sensibus, sempiterna oblivione deletur.'"*

It is not necessary for us to dwell upon the many transmissions of gait, and voice, and manner, which are within the experience of every one. Children who have never seen one parent will suddenly call a tear into the eye of the other. By the inheritance and manifestation of some little trait or gesture the treasured past is unlocked, and we live again in our little ones. In fact, there is no exception to the power which nature, in her love of conformity and imitation, has of repeating herself in all the *active* characteristics of individual beings; and to humanity is applicable in its most extended signification the exact and rigorous proposition of Girou de Buzareingues:—"Il n'y a rien, dans l'animal, qui ne puisse se transmettre par génération."

We wish we had space to enter into some of the elaborate questions which engage the attention of our author in the second and more bulky volume of this remarkable treatise. It would interest us to discuss with him the part played by the two sexes in procreation, as regards the distribution of paternal or maternal type in the physical and moral nature of descendants. We smile at some of the physiological and mechanical speculations of Aristotle as to the proportion of factorship to be attributed to either parent. It is beyond a doubt that the relative proportion both of character and of sex is extremely unequal, and that in many families the individuality of one parent is so prominently stamped upon the offspring as to swallow up the individuality of the other.* In some, again, the power of begetting is limited to the production of one sex, that sex not necessarily charged with the characteristics of the parent from whom the sexual distinction is derived. In other cases there is a happy blending of parental faculties and forms, and equally happy balancing of sexual proportion. We must leave it to the curiosity of our readers to fortify, if they think fit, their own family experiences upon this physiologico-social riddle, by an immediate reference to the work of Dr. Lucas itself. The investigation may cost them some time and trouble, but it will not be unrewarded.

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* Theatrum Ingenii Humani, lib. i. p. 326, et lib. ii. p. 209.

And we shall conclude by expressing our belief that it is not sufficiently recognised in the home and scholastic management of children, in the assignment to them of particular duties and professions, in the cruel and unmeasured condemnation of certain faults, and the undue laudation of certain better qualities, how tremendously coercing may be the power of hereditary predisposition. The utter failure of education, and the miserable disappointment of religious zeal, are too often to be attributed to an ignorance of principles and facts which are legible upon the page of history to all who are not puffed up by the self-importance of the traditional pedagogue, or blinded by the feeble incomprehensiveness of conventional theology. We are persuaded that there would be better schoolmasters if those who profess to teach one generation would more study to learn something of the generation which preceded it. For, says Lavater, "does it not constantly happen that we find, trait for trait, in the son the character, temperament, and most of the moral qualities of the father? And how frequently does not the character of the mother re-appear in the daughter or the son, or that of the father in the daughter?"

We believe, also, that there would be a better and higher, because more practical, class as well of hearers as preachers, if it was more considered by them how large is the influence of the law we have discussed, and how intimately it is bound up with our entire social economy. Its non-recognition makes applicable almost to every age, and certainly to our own immediate day, the satire of Remusat:

> "Parmi nous un homme religieux est trop souvent un homme qui se croit entouré d'ennemis, qui voit avec défiance ou scandale les événements et les institutions du siècle, qui se désole d'être né dans les jours maudits, et qui a besoin d'un grand fond de bonté innée pour empêcher ses pieuses aversions de devenir de mortelles haînes."

It is the compilation and production of such a masterly and comprehensive work as that over which we have spent much time, and to which we have given great reflection, which floods with light the entire field of knowledge, shows us how intricate is the dependence of one part of the social fabric upon another, how every science bears in some way upon the whole, and how every individual member of the great aggregate may, if he will but be at the pains, better his own condition, and that also of those among whom he extends his individual interest. True indeed are the words of De Quatrefages:

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* We have ourselves seen a famous white boar, who is gifted with this power of transmitting his individuality to his progeny. It is related of him, that however black may be the sow whom he favours with sexual attentions, the resulting pigs are always and without exception *white*.

> "En effet, tout individu, pour pouvoir pleinement se développer, doit être en harmonie complète avec les conditions d'existence, avec le milieu où il vit; toute espèce, pour se propager et s'étendre, doit satisfaire à la même exigence. Du moindre désaccord entre ces deux termes résulte la souffrance pour l'individu, l'amoindrissement pour l'espèce."

REVIEW V.

Copy of the Papers relating to Quarantine communicated to the Board of Trade on the 30*th day of July,* 1861. *Ordered by the House of Commons to be printed* 6*th of August,* 1861. pp. 48.

THESE papers on Quarantine we can earnestly recommend to the attention of our medical brethren, and had we an influential voice, we would recommend them, and even more earnestly, to the notice of statesmen and merchants, inasmuch as they relate to matters of the first importance in their bearing not merely on commerce and the public health, but also on the liberty of the subject and the cause of humanity throughout the world.

Having in a recent number of this Review discussed quarantine at some length, we must restrict ourselves at present to a brief notice of these documents, which are well described by Dr. Gavin Milroy, the honorary and able secretary to the Quarantine Committee, as the complement to those which were communicated last year to the Board of Trade, and of which we gave an account in the article referred to.

The first of the papers, entitled "Additional Abstracts of Information from the Despatches of Her Majesty's Consuls, and from the Governors of British Colonies, addressed to the Foreign and Colonial Principal Secretaries of State," are chiefly interesting as disclosing further the evils of the present system of quarantine as conducted in many countries, especially those under arbitrary governments. We noted in the margin, by a word here and there, the impression we received in reading the details, and we may repeat them for brevity sake, not doubting that other readers would feel like ourselves in the perusal: they, the words, were, *cruelly severe, very lax, inhuman, mercenary, illogical, inconsistent.* Nor is it surprising that these terms should be merited, considering what quarantine is, based on no sound principles, and conducted, as too often it has been, with sinister intents, in connexion with an arbitrary police and by mercenary officials.

The second documents consist of the "Report of the Quarantine Committee of the National Association for the Promotion of Social Science, and of an Appendix," the former drawn up, as we infer, by the honorary secretary, to whom also we are indebted for an "Historical Sketch of Quarantine Legislation and Practice in Great Britain," this the subject of the appendix.

In our former article we have given the names of this committee. Their labours have, it would seem, been in no wise commensurate with their responsibility. We make the remark, seeing that the majority of them have merely signed the documents, thereby expressing their approval after it had been submitted to each, without offering any suggestion or addition; and, amongst the small number who constitute the exception, we rather regret to notice that there is not perfect harmony of opinion.

The report itself we consider a very able one, and admirably adapted to fulfil the objects in view—viz., the making fully known how quarantine is really conducted at present in all parts of the world, and its multifarious bearings on the interests of society. It is founded on the Parliamentary Papers already alluded to, and consists of "Queries proposed," of "Answers to these Queries, and of explanatory observations," followed by "illustrations." As an example of the illustrations, we shall quote those of Query II. :—

Query.—"What are the diseases which render all arrivals, without exception, whether sick or well, from a place or country where such diseases are existing, subject to quarantine in the port of?

Illustration.—"The diseases, against the importation of which, from countries where they exist, or are alleged to exist, quarantine is chiefly directed in European ports are, the plague, yellow fever, and Asiatic cholera; and wherever the quarantine system is vigorously maintained, as in Spain, Portugal, Naples, Greece, &c., all arrivals, without exception, and whether any sickness has occurred during the voyage or not, are then liable to detention before pratique is granted?

"The periods of detention recommended by the International Conference of Paris, and generally adopted by the above nations, are these—

From 10 to 15 days in case of the plague.
„ 5 to 15 „ „ yellow fever.
„ 3 to 5 „ „ cholera.

"In Neapolitan ports, the quarantine, on account of the plague, is from 15 to 20 days, and that on account of yellow fever and the cholera from 10 to 15 days. These lengthened periods have of recent years been enforced in other Mediterranean, and also in some oceanic ports, as in those of Portugal and her colonies, &c.

"(*a*) *Plague.*—In the summer of 1858, on the first public announcement of the existence of the plague in the district of Bengazi, on the Barbary coast (the disease had been existing for months before its real nature was recognised), restrictive measures of extraordinary rigour were at once put in force throughout the whole of the Mediterranean and Black Seas, and in all the ports of Spain and Portugal, not only upon arrivals from the infected locality and other parts of the African coast, but upon contiguous countries and other places which might be supposed to have direct communication with the seat of the fever, however healthy these places might continue to be.

"*Malta,* from its position and its trade with Tunis, &c., was exposed to special suspicion; and accordingly the most stringent quarantine was enforced, even in various Ottoman ports, upon vessels arriving from or communicating with it; although at the time a quarantine of from 5 to 15 days was kept up by Malta upon all arrivals from the infected district of Barbary.

"*Gibraltar* also was similarly treated. A quarantine of 21 days' duration was imposed in the ports of Naples, Greece, Portugal, &c., upon all vessels coming from, or which had touched, at Gibraltar. Not that any disease existed there, or that the health of the 'Rock' was bad; but merely because it continued to hold communication with Morocco, which was also at the time in a healthy state, and quite free from any pestilential malady.

"However unwillingly obliged to act in accordance with the rules and practice of other Mediterranean ports from the fear of retaliatory measures, Malta and Gibraltar had actually to put each other in quarantine; Malta, because Gibraltar had intercourse with Tangiers, &c.; and Gibraltar, because Malta had intercourse with Tunis and Bengazi, both British colonies being all the while in perfect health.

"No case of the disease was observed beyond the districts around Bengazi, where it first appeared." (p. 14.)

Did space permit, we could wish to give other instances of the manner in which quarantine is conducted. As regards the badness of the system it discloses, it needs no comment.

The same reason, want of space, prevents our giving the general conclusions deducible from the evidence contained in the body of the Report. They are comprised under nine separate heads, and are followed by a certain number of recommendations.

These conclusions and recommendations, we hope, have not been drawn up and given in vain. That they will command universal assent and approval is too much to expect. We have expressed some regret that amongst the members themselves there appear to be dissenting voices on some particulars which we will not stop to specify. We also see with some regret that cholera and yellow fever are held, at least by one of the number, to be equally fit subjects for quarantine, implying that they are communicable, and their outbreak preventible by restrictive measures, measures which in so many instances have been found futile. We must declare, with Dr. W. Farr, adopting his words, that we are "unable to assume that the introduction of dangerous diseases can be prevented by any quarantine regulations." This, his opinion, is appended to the first recommendation of the committee, ending with the words, "our object is to amend and utilize, not to discontinue or to abolish, the existing machinery of action." Such a proviso as that expressed by Dr. Farr, is surely *in limine* of the first importance, and for a real reform to be effected must be carefully considered.

Now we are expressing regrets, we will not conceal another that we have felt arising out of the circumstance that one of the members excuses himself for not signing the Report, on the ground that he has "recently been employed by the Government in a confidential inquiry into some questions connected with quarantine," and is thereby "precluded from expressing any opinion on the Report of the Committee," and of entering upon a discussion of the various points with sufficient freedom to render his remarks of any value." We, for our part, should have thought that this his inquiry would have rendered him more competent to advise in so important a matter, the great and final object being the elucidation of truth and the clearing of the way for the establishment of a system, if quarantine be necessary, as efficient as possible and as little injurious as possible. We hardly know what to understand by "confidential inquiry" in such a matter as seems to need the most perfect publicity.

Another name is wanting amongst the signatures of the members, that of Sir William Pym, the late and the first Superintendent-General of Quarantine, whose death occurred before the Report could have reached him. May his successor, should a successor to the office be appointed, have all his good qualities, and an inquiring mind as much as possible free from bias, and enlightened by science. Were an unsuitable person to follow him, pledged to certain doctrines and determined to uphold them, great might be the resulting evils. This country, instead of setting an example of an enlightened policy to other countries in matters of quarantine, might be in danger of the reverse and of a retrograde movement—a thing not impossible, considering how little the public attention is given to the subject of quarantine, and how happily the mass of the people are ignorant of its enormities and abuses.

The appendix to the Report is an able and interesting document, and it might be quoted in support of what has just been said of the danger of a relapse or retrograde movement. It relates how this country, till a comparatively recent period of its history, was exempt from quarantine; how, when restrictions were first enforced, they were not extrinsic, but merely local and for the occasion—an outbreak of plague, and this up to 1710, the year when the first quarantine legislative enactment was made; and how afterwards, according to the hypothetical views entertained of the nature of infection, and the public alarm produced by the spread of destructive epidemics abroad, the system was variously modified; and in the changes it thus underwent, illustrating in a striking manner its history, cradled as it was in terror, nursed in ignorance, and for a while, still growing, tolerated in indifference, till at length its evils got to such a head as to be almost intolerable, when attracting attention and exciting inquiry, conducted chiefly by men of the medical profession, French and English, its reform was entered upon.

A few of the extreme acts which were perpetrated during the period referred to, are well fitted to show the spirit of the enactments at different times, and the abuses to which they were open in practice. In 1604, justices of the peace were empowered to shut up infected houses, with their inhabitants, for forty days, the well and the sick together, prohibiting egress or entrance. In 1721, two vessels from Cyprus, then infected, having cotton and other goods on board, were, by order of the Privy Council, burnt, with their cargoes, at an expense of 23,935*l.*, and this, although there had been no disease amongst the crew during the voyage, or after reaching port.

In 1752, all vessels coming from foreign ports without a clean bill of health, were compelled to go to a foreign Lazaretto, and there perform quarantine, and have the goods forming their cargo depurated before they could obtain leave to enter their destined home port.

In 1770, a quarantine of forty days was imposed on all vessels from certain ports, and whatever their lading, the articles were considered susceptible, even grain, and needing depuration.

In 1813, when yellow fever was prevalent at Gibraltar, a vessel of two hundred and twenty-six tons was chartered by a merchant there, who wished to avoid the fever, and who brought with him a few goods of his own. "The quarantine duty that had to be paid before she was released from quarantine at Stangate Creek, where she had been detained for a length of time, amounted to 124*l.*, and her freight was only 75*l.*" None on board, either during the voyage or on arrival, were sick. Even as late as 1845, the deplorable case of Her Majesty's ship *Eclair* occurred, which is so well known to the readers of our pages.

We cannot conclude without expressing our thanks to Dr. Gavin Milroy for his able exertions in this good cause, and our hopes that he will not relax, for, as we have before said, it is only by importunity and perseverance that any long-rooted evil can be got rid of.

Review VI.

1. *Lectures on the Diagnosis and Treatment of the Principal Forms of Paralysis of the Lower Extremities.* By C. E. Brown-Séquard, M.D., F.R.S., Physician to the National Hospital for the Paralysed and the Epileptic, &c. &c.—1861. pp. 118
2. *Course of Lectures on the Physiology and Pathology of the Nervous System.* Delivered at the Royal College of Surgeons of England, May, 1858. By C. E. Brown-Séquard, M.D., &c.—1860. pp. 276.

Our readers will remember that already* we have put them in possession of an exegetical statement as to the lectures comprehended in the work placed second in the heading of this review, which are well known as having deservedly obtained the public favour. We are not, therefore, about to abuse their patience by any recapitulation, or to conduct them a second time through the novel physiological positions established, or sought to be determined, therein. We are, however, about to invite their consideration of an appendix which, in publishing the lectures in a separate form, the author has subjoined; and to this appendix (containing, as it does, additional material of much value, and remarks which command attention, as leading to a fuller understanding of the various observations presented in the lectures) we give precedence of comment rather than to the later lectures on Paraplegia, because it is in its nature complementary to the course which, being delivered antecedently, anticipates by its more general and initiative character the special subject-matter of the volume on Paraplegia.

Passing, then, entirely over the lectures themselves on the Physiology and Pathology of the Nervous System, we find that the recently added appendix (above fifty pages in length) contains important matter both of a theoretical and a practical nature, much of which the author in the first instance omitted, apparently as it were for symmetry's sake, or fearing lest the demonstration of the subject should lose in transparency, or be in any way impeded by digression. In this postscript we find the writer addressing himself, in the *first* place, unshrinkingly to examine and controvert certain objections which have been, or might be, preferred with regard to some of the views previously urged by him; in the *second* place, to give the most important of the deductions which, referring to the treatment of several diseases, may be drawn from the principles inculcated in the lectures; and in the *third* place, to present some "additional facts" in proof of some of his propositions.

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* See the number of this Journal for July, 1859.

Of these several articles, we here purpose to give the salient points, and we shall offer no apology for dwelling to some extent upon the *first* intention which the author has had in mind—that, namely, of meeting "*objections*," for, being fully sensible of the deep responsibility attaching to the promulgation of theories so subversive of accepted physiological doctrines, as are many of Dr. Brown-Séquard's, and being at the same time mindful of the necessity of an unbiassed attitude in their reception by those to whom they are so strange, we have ventured to assume, without fear of contradiction, that this question of objections is one of the greatest gravity, and therefore demands the freest scope. This must be especially so when, as in the case of the views alluded to, conclusions of much practical moment regarding human life and the alleviation of its troubles, depend upon the acceptance or rejection of scientific teaching, the false dicta of which, like incautious language generally, deserve pre-eminently to be described as "the dry rot of the world."

The amount of space allotted to this review precludes, we regret to say, such an intimate examination into either objections or rejoinder as the subject well deserves; and consequently, whilst using all needful and possible carefulness, lest through over or under-statement, as respects broad principles, misconception should find room, we are compelled, as regards many highly important illustrative physiological and pathological facts, to refer our readers to the text itself.

The objections, real or supposed, which are adduced for notice, but summoned only to be refuted, are nine in number, and are contained in the following clauses:

I. The alleged existence of voluntary movements and of sensibility in children apparently deprived of the cerebro-spinal axis.

II. The alleged existence of voluntary movements and sensibility in parts of the body considered as deprived of their natural connexion with the encephalon.

III. The alleged existence of sensibility and voluntary movements in men and animals deprived of all the parts of the encephalon except the medulla oblongata and pons Varolii.

IV. The occurrence of cases proving that considerable alteration of the pons Varolii and medulla oblongata may exist without producing paralysis either of sensibility or of voluntary movements.

V. The existence of cases in which an alteration in the two sides of the pons Varolii appeared to have produced a paralysis only in one side of the body.

VI. The existence of cases in which an alteration existing in one side of the pons Varolii, or in the neighbouring parts, appears to have produced paralysis on both sides of the body.

VII. The occurrence of cases in which an alteration in one side of the pons Varolii, or of neighbouring parts, has produced paralysis in the same side of the body.

VIII. The occurrence of cases and experiments which appear to prove that there are in various parts of the encephalon and in the spinal cord, motor nerve-fibres which are *not* of a voluntary or motor nature.

IX. Anatomical and pathological dissections which appear to prove that there are nerve-fibres coming from the spinal cord which decussate in parts above the medulla oblongata.

Having above enumerated, in as condensed a manner as completeness of expression will permit, the statements or descriptions of facts which are, or may be, urged as objections, we shall now adduce, in an equally concise form, the answers which, according to our author, are to be given to these objections.

Objection I.—The simple answer to this is to be found in the categorical counter-statement that, the various movements supposed, in the animals in question, to be indicative of sensibility, and of an exercise of the will, are in fact nothing more than reflex or excited movements; but then comes the question (so very difficult of solution), How must reflex movements be explained in cases where no spinal cord exists? or, if it does exist, where it is not united with the nerves?

As explanatory of these excited movements in such instances, there are four hypotheses which may be urged. It may be assumed, with Geoffroy St. Hilaire, that the fluid sometimes found in place of the nervous centres, surrounded by membranes, may be looked upon as containing the elements of the brain and spinal cord in their primitive state of development, and that this fluid may act as those organs would do if complete. Seeing that in the human embryo no movements exist so long as the nervous centres are in a liquid state, the latter portion of the above supposition cannot be considered tenable, although it is possible that the liquid in the vertebral canal of monsters contains a sufficient number of nerve-fibres and cells to produce some reflex movements. Again, in certain cases wherein a rudiment of a spinal cord exists, but after death is found to have no union with the nerves, it may be assumed that before death such union (no doubt of the slightest possible kind, yet still sufficient to permit of reflex actions) did really exist; but that, owing to softening and manipulation, separation had been produced.

A third mode of explanation may hold good in certain anencephalic cases, wherein the nerve-fibres are found forming loops embedded in a filamentous tissue, and surrounded by numerous granules; viz., that this granular matter may be in reality grey nervous substance in an early stage of development, and that this incompletely-developed grey matter, if unable to produce reflex movements sufficiently strong to be observed, may, at any rate, help other parts in their production.

The remaining possible method of explanation is that the ganglia of the spinal and sympathetic nerves, which in monsters are deprived of nervous centres, and very exaggerated in size, become endowed with reflex powers. In order to render this supposition at all acceptable, we must assume that the nerves of the muscles are connected with these ganglia by many more nerve-tubes than usual; that the branches of communication between the spinal nerves and the sympathetic ganglia are larger than usual; and that the fibres pass from the periphery of the spinal nerves into these communicating branches.

Objection II.—This has already been partly anticipated in the original lectures, Nos. V. and VI. The objection loses all force owing to absence of any proof that nerve-tissue, even when much atrophied, indurated, or softened, loses its properties; or that, in cases even of complete liquefaction and apparent disorganization such a condition, as found *after* death, actually existed *before* death. In numerous cases, wherein division of the cord had been supposed, but in which paralysis of motion or sensation did not exist, either this division had occurred below the part of the cord whence the nerves were given off, or the division had been incomplete.

Objection III.—The statements involved in this objection rest primarily upon the determination of Longet. Dr. Brown-Séquard urges that the "agitation" and the "cries" in animals deprived of all the encephalon excepting the pons Varolii and medulla oblongata, are really not indications of any perception of sensation, or of volition, but merely the result of reflex action; the agitation of the limbs being identical with that produced by experimental separation of the spinal cord from the encephalon; and the cries being determined by tension of the vocal cords along with sudden reflex contraction of the expiratory muscles. In opposition to several experimenters, Brown-Séquard finds that cries, along with agitations, may be produced even after division of the pons Varolii in cats, rabbits, and guinea-pigs. When, however, the medulla oblongata is removed, although the agitations are still produced by pricking, the cries are not so produced. It may be supposed that if the motor nerves of the larynx were connected with the spinal cord, and not, as they are, with the medulla oblongata, such cries would still be formed (the contraction of the expiratory muscles being still producible along with the agitation of the limbs.) Thus, also, in anencephalic or acephalic cases, when the entire encephalon, except the medulla oblongata, alone or with the pons Varolii, is wanting, there is no proof that the movements produced were other than convulsive or reflex.

Objection IV.—The existence of such cases as are herein alluded to is of course freely admitted. The comparatively slight effects resulting in some of the cases adverted to is explicable on the assumption that in many instances—as, for example, in cases of slight pressure from tumours, we have only a separation and not a destruction of fibres;

and again, by the circumstance that the fibres conducting sensation or the mandates of the will, constitute, in their nervous centres, but a small proportion of the structure of these centres, so that they may more or less well escape damage, even when the centre in question is materially affected. Such cases as those last cited indicate incidentally, that the pons Varolii and medulla oblongata do not form the centre for volition or for perception of sensory impressions.

Before passing on to the next formal objection, Dr. Brown-Séquard alludes to the experiments of Drs. Vulpian and Philipeaux, in which they had divided a lateral half of the medulla oblongata transversely, about a line in front of the nib of the "calamus scriptorius," without, as they assert, destroying voluntary movement or sensibility in the two sides of the body. The so-considered volitional movements which occur, are explained by Brown-Séquard as being in fact convulsive, and the cries as being obviously of a reflex origin; and the experiments cannot in any way invalidate the proposition, that voluntary motor and sensory fibres are necessary as communications between the spinal cord and those parts of the encephalon anterior to the pons.

Objection V.—As respects those cases in which structural alterations of "*both*" sides of the pons Varolii appeared to produce paralysis on "*one*" side of the body only, very minute examination would no doubt have demonstrated that the alteration was in reality greater on one side than the other.

Objection VI.—As to cases included under this objection, experience points out that very frequently extensive disease of one side of the pons is accompanied by alterations extending to the opposite side of the same organ, although this oftentimes is not observable.

Objection VII.—As respects the existence of paralysis on the side of the body corresponding to the affection of the pons Varolii, &c., the existence of such a phenomenon has already met with an explanation in Lecture XII., and also in the author's 'Journal de la Physiologie,' 1858 (July and October).

Objection VIII.—Touching the point of this objection, the presence, along with motor fibres of a volitional nature, of non-voluntary motor fibres, originating from the same locality, in many parts of the encephalon (especially the medulla oblongata), is sought by Dr. Brown-Séquard to be established by the fact, that in certain injuries of one side of the brain in mammals we may have, along with paralysis of the opposite side of the body, convulsions or local spasms co-existing on the same or corresponding side.

Our author maintains, that if the nerves, by the alteration of which the convulsion or spasm is produced, were voluntary in function, they would be of necessity affected in like manner as the volitional ones allotted to the opposite side of the body, and then we should have paralysis on both sides of the body (the cerebral alteration being, nevertheless, only *one*-sided).

Objection IX.—The dissections here alluded to are those of Dr. Türck, whereby he found that in injury of part of the encephalon the nerves passing thence into and along the spinal cord become altered in structure, and that they cross over to the opposite side of the cord either in the pons Varolii or in parts anterior to it. Considering that there are fibres originating in various parts of the encephalon which are not sensory or motor, the fact has nothing in it which can invalidate the statements made in the Lectures, as to the place of decussation for sensitive and volitional nerve fibres.

Part II. of the Appendix, embracing the application to the practical treatment of diseases of some of the views set forth in the Lectures, is principally concerned with their surgical bearings, and especially has reference to the treatment of fractures of the vertebræ. Respecting this (to the surgeon) knotty point, the author, reviewing the literature of this branch of surgery, and quoting illustrative and apposite cases placed on record, endeavours to prove—

1stly. That in man and animals the laying bare of the spinal membranes, or even dividing them and so exposing the cord, is "*not*" a more dangerous operation than the exposure of the brain or its coverings;* indeed, the "meninges of the brain are much more liable to become inflamed than those of the spinal cord, as is proved by the relatively small number of cases of inflammation of the spinal meninges after they have been injured by a piece of bone, a sword, &c." This proposition is sought to be supported both by analogy from operations on the lower animals, and also by pathological cases in which portions of vertebræ in man had been removed, exposing the cord—i.e., we presume, the dura mater. It must be observed, however, that in these cases the spinal membranes were not divided.

2ndly. That death following fracture of the spine is commonly produced by pressure upon, or morbid excitation of, the cord by broken pieces of bone, and is not the result of partial or complete section of the cord—i.e., of a paralysis or loss of action; such an injury artificially produced not being followed by alterations in the kidney and bladder, &c., so common as causes of death in fractured spine. This declaration is supported by the consideration that when the fracture is *high up* in the cervical region and the cord crushed, death is rapid, owing to the cessation of respiration, and partly to the paralyzing influence on the heart; when the fracture is low down in the same region, or in the upper part of the dorsal region, life may be saved, if the cord is only partially cut and not crushed, and it is the same if there is pressure only, as (according to Dr. Brown-Séquard) usually is the case. To what extent observation serves to prove that this is common, we are not quite sure. When the fracture is in the middle of the dorsal region there is a chance for the patient's life being saved, even, it is stated, "if the cord is completely severed;" and of course the chance is still greater if only a part of the thickness of this organ is divided or crushed.

The supposition that sloughs, supposed to be from pressure, affections of the kidney, bladder, &c., so frequently causes of death in fractured spine, are due to *morbid excitation* of the cord (and not owing to *loss*

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* The author controverts the supposition that in cases of tapping for spina bifida, it is the entrance of the *air* which sets up meningitis, by adducing the fact that this inflammation attends the operation as frequently when it is a subcutaneous one as otherwise.

of action), owing to partial or complete section of the cord, is based on considerations to be alluded to hereafter (p. 384), regarding the action of the nervous system in producing alteration of nutrition.

3rdly. That re-union may occur after a wound of the spinal cord, so that its function may return—a fact proved as well from experiments on animals as from human pathological cases.*

4thly. That the removal of some parts of the vertebræ may be followed, although perhaps slowly, by the production of new bone; so much so, that the posterior half of the osseous ring of five or six vertebræ may be restored after removal.

5thly. That all the cases of fractured spine in which the trephine has been applied either show the great value of its use, or, at any rate, show that surgical interference, when rendered suitably, is not harmful or dangerous. On this subject Dr. Brown-Séquard thus expresses himself somewhat forcibly:

> "It is evident that operations which are not dangerous, and which may save the life of three or four patients out of a number of sixteen or seventeen—i.e., nearly 20 per cent., should not be neglected. The per-centage of cures after these operations, compared to the per-centage of cures (perhaps less than one per cent.) when neither of them is performed, shows clearly the importance of such kinds of surgical interference after fracture of the spine." (p. 259.)

Again:

> "Three distinct operations may be performed on the spine in cases of fracture: 1st. The extirpation of broken pieces of bone; 2ndly. The raising up, or lifting out, of the posterior arch of one or several vertebræ, when pressing upon the spinal cord; 3rdly. The application of the trephine. These various operations, or one or two of them, ought to be employed in almost all cases of fracture of the spine, especially in the cervical region and in the upper parts of the dorsal region where pressure upon the spinal cord is attended with so much danger. The operation should be performed as quickly as possible after the fracture, and before inflammation has set in. If, after having laid bare the spinal cord, it is found necessary to reduce a fracture of the body of one or several vertebræ, the reduction will be much easier, and attended with much less danger, than if the vertebral canal had not been opened in its back part.".

As regards this surgical operation of removing portions of the inpressing arch of the vertebra by the trephine, or by a Hey's saw, to be performed, of course, only in *certain cases* of fracture, no doubt much discrepancy of opinion is permissible, though, on canvassing most English surgeons of the present day on its expediency, we expect we should too frequently be met with the ominous shake of the head—*quieta non movere*—and perhaps as much objection as if Galen's operation of trephining the sternum for empyema, or that of nephrotomy for the removal of renal calculi, or that of excising a carcinomatous mass connected with the sigmoïd flexure of the colon were advised.

We think that an enumeration of the various cases placed on record, in which the operation has been performed, with a short *résumé* of some of them, will be here historically useful, and will at all events serve to show what former surgeons of repute thought of the matter, and so incline some to receive with more impartiality, if not with favour, the suggestion in question which has been made.

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* One of the cases to which reference will be made further on, in which depressed portions of six vertebræ were removed artificially by Dr. Potter in America, shows remarkably the power of enduring pressure possessed by the spinal cord. In that case bone which had been depressed as long as three months and ten days was removed, with the effect of allowing return of sensation which previously had been entirely wanting in the lower part of the body. Sensation became "nearly complete" in four or five hours. Unfortunately, nothing is said about any restoration of motor power.

It is well known, looking far back, that Paulus Ægineta urged, that in cases of fractured vertebræ, compression of the spinal marrow or membranes taking place, it was necessary, "if possible, to attempt to extract by an incision the compressing bone;" and that in the case of a broken piece of the vertebræ being felt, an incision of the skin must be made to extract it. Celsus, and later of the ancient writers, held the same opinion.

Trephining, in cases of fractured spine, was in modern times long ago advocated. It was spoken of by Dr. R. James, in his 'Medicinal Dictionary' (London, 1743, vol. ii.), who, under the heading of 'Fractures of the Vertebræ,' recommends the elevation and removal of fragments pressing on the spinal cord; at the same time, he coincides in the then general opinion, that if the cord be wounded "death follows inevitably;" and it was recommended in 1796 by Chopart and Desault. The operation was afterwards put into practice in this country. It had been successfully performed in France by the celebrated Louis, Perpetual Secretary of the French Academy of Surgery, in the year 1774; but in England, was first performed by the younger (Mr. Henry) Cline, in the year 1814, at St. Thomas's Hospital. Mr. South, in his translation of 'Chelius' Surgery,' vol. i. p. 538, describes the performance of the operation by Wickham, Attenbury, Tyrrell, Holscher, Smith, Rogers, and Astley Cooper, and it was also done by Mr. South himself. It was with partial success performed by Dr. J. R. Barton in America, in 1824, as is described in Godman's edition of Cooper's work, on 'Dislocation and Fractures' (p. 421), and Packard's Translation of Malgaigne (p. 343); and successfully by Mr. Edwards, of Caerphilly, in South Wales, as noticed in the number of this Review for July, 1838, p. 162; and by Dr. Potter, of New York, as recorded in the 'New York Journal of Medicine,' edited by Dr. Lee, vol. iv. p. 176, 1845. Dr. A. Mayer, of Würzburgh, performed the operation with benefit, as described in Walther and Ammon's 'Journal d. Chirurg.,' 1848, vol. xxxvii., the patient dying a fortnight after with pulmonary disease; so did Dr. Laugier, in a case in which the spinal cord was ruptured, as detailed in the 'Bulletin Chirurgical,' 1839, tome i., p. 401. A case by Mr. Jones, of Guernsey, in which the operation was in other respects not injurious, and was attended by a return of the functions of the cord, may be seen related in the 'Medical Times and Gazette,' for July, 1856, p. 86. In one of two cases of Mr. Tyrrell's above alluded to, for he performed the operation twice (that which was performed in the year 1822), it appears that Mr. Astley Cooper and also Mr. Greene and Mr. Travers were present, and gave their support to the measure. Respecting the operation we may quote the great Abernethy's words: "I think it is a proposition," he said, "too hardy to be acquiesced in; but I see nothing in it but what is rational; it is the only mode that occurs to one's mind." And Astley Cooper, who, as we before said, performed the operation, stated, "If it saves only a life in one hundred, it is more than I have yet seen accomplished by surgery ('Treatise on Dislocations,' &c., 1842, p. 559). It seems that Jäger is favourable to the operation.*

* With regard to these operations, we should have been glad (in order to have made such complete inferences as they might have led to) if it had been in our power to have given all the circumstances connected with the cases which have happened. But this, in spite of our researches in all directions, we are unable to do. It will, nevertheless, not be uninteresting if we state the particulars we have gleaned of most of them. The operation first placed on record as being M. Louis's, and related in a posthumous communication read before the French Royal Academy of Medicine, April 18th, 1774 (see Archiv. Général., 1836, tom. ii. p. 417), was performed in 1762, at Marbourg Hospital, on a soldier who was shot in the back. He fell, and at once became paralysed in the lower limbs. Four days after the accident, the patient was first seen by Louis; he then had much fever, with entire continence of urine, and the ball had been extracted. At the bottom of the wound several moveable portions of bone, some of large size, could be felt. These were removed, and an abundant and healthy suppuration set in. Day by day, the patient improved; the extremities recovered their sensibility, and finally their power of motion to such a degree that he could walk with the use of a stick, the legs remaining, however, weak, and without their ordinary "embonpoint." "Quoiqu'il en sort," says Louis, "c'est une victime que l'art a soustrait à une mort certaine." After relating this case, Louis details two or three others in which, as it appeared after death, the removal of loose bone embarrassing the spinal cord would have been most useful. He alludes to the probability of his reading a future paper, in which the important subject of the diagnosis of such cases would be considered. Of this, which would have been a valuable document, we regret to say we have found no traces. Respecting the operation by Mr. Cline at St. Thomas's Hospital in 1814, we find that it was performed upon a man who had fallen from a window, and evidently suffered displacement of the twelfth dorsal vertebra. Two broken and loose spinous processes, and also the arch of a vertebra, were removed. Convulsions subsequently came on, and the patient died on the fifth day after admission into the hospital, and the third after the operation. After death, the spinal cord at the injured part was found three-fourths torn through, and the sheath much lacerated; and there was oblique fracture of the body of the twelfth dorsal vertebra. The spinal cord had been compressed between the back of the body of the twelfth vertebra and the arch of the eleventh dorsal vertebra, owing to the moving *forwards* and downwards of the latter vertebra.

One of Mr. Tyrrell's cases may be seen related at length in the Lancet for 1826–7, vol. ii. p. 685. The operation took place at St. Thomas's Hospital, Feb. 5th, 1827. The patient was a man aged thirty, on whose back a huge piece of timber had fallen, producing complete paralysis of the lower limbs. The spinous process "of the last dorsal vertebra was found to recede, whilst the spine of the first lumbar appeared to project far beyond its natural limit." No crepitus was distinguished. On the following day, Mr. Tyrrell, with the sanction of Mr. Green, removed the spine of the last dorsal vertebra, and by a Hey's saw and forceps afterwards removed the arch, which "was felt to be materially pressing on the spinal marrow." Three or four hours after the operation, "feeling" had returned imperfectly in the upper two-thirds of the thigh. The urine which was retained, became ammoniacal, and on the sixth day after the operation, the wound going on favourably, symptoms of pleurisy set in, and he died on the eighth day after the accident. The body was not examined. The other case in which Mr. Tyrrell removed portions of an in-pressing vertebra, is alluded to at the end of the report of the above case, but it is described at length in Ollivier d'Anger's work on the Diseases of the Spinal Marrow, 1837, vol. i. p. 381, from the pen of M. Georgi of Bologna, who was present at the operation. The operation was performed October 17th, 1822, upon a man, aged thirty, at St. Thomas's Hospital, who fell on his back whilst carrying a load, fracturing the laminæ of the ninth and tenth dorsal vertebræ. The lower parts of the body were completely paralysed. Some hours after the operation, the patient could feel when he was pinched, but this return of sensibility was only momentary, and he never regained power of moving the legs, although he passed the evacuations voluntarily. He died of peritonitis and enteritis fifteen days after the operation. The spinal dura mater at the part corresponding to the operation was very dark in colour, but the spinal cord was not examined, as the parts were kept as an anatomical preparation.

The case in which Dr. David L. Rogers, of New York, operated, is described in the American Journal of the Medical Sciences, vol. xvi. 1835, p. 91, and was that of a man aged thirty-one, who, by falling upon a box, Feb. 3rd, 1834, fractured the spinous process of the first lumbar

Speaking of the operation, South says (loc. cit.), that it "would of course never be thought of unless symptoms of compression existed;" and again, "the only reasonable objection to the operation of trephining the spine is, that we cannot, previous to the operation, ascertain whether the spinal cord be simply compressed, or whether it be partially or entirely torn through, or whether the symptoms of com-

vertebra, and depressed it upon the spinal cord. Paralysis remaining, the operation for the removal of depressed bones in separate portions was performed. In their removal, the irregular edges came in contact with the spinal cord, "causing excruciating pain, accompanied with convulsive contractions of the muscles of the back;" but afterwards the spinal cord, of which two inches were exposed, did not appear to be at all injured. In about a quarter of an hour after the operation, the patient expressed himself as being much relieved; "sensibility returned to the lower extremities, respiration became easy, and with the assistance of an anodyne, he slept for several hours." The patient died on the eighth day after the operation, with gangrene in one foot, after being for three days delirious. The wound in the back had gone on well, and there had been no recurrence of any symptom indicating pressure on the spinal cord. After death, there was found also fracture through the body of the first lumbar vertebra, "but no displacement." "The spinal cord seemed in a healthy condition."

The operation by Dr. Alban W. Smith, of Kentucky (as related in the American Med. and Surg. Journ., July, 1829, p. 94) was performed two years after a fall and fracture, with displacement, of the spinous process of the third and fourth dorsal vertebræ, along with compression of the cord. There was complete paralysis of all the limbs, except the muscles above the elbow on each side. After the operation, sensibility was regained in the thighs and in the hands; but no further particulars of the case were given.

The operation by Dr. Barton was performed in the Pennsylvanian Hospital, August 18th, 1824, upon a man who, having fallen from the mast-head, had sustained fracture of the spinous process and arch of the seventh dorsal vertebra; also separation of the seventh and eighth dorsal vertebræ from each other, and fracture of the body of the ninth vertebra, with great compression of the cord, so that perfect paralysis was produced. The spinous process and arch having been removed, sensibility began to return forty-eight hours after the operation, and increased until the third day, when collapse set in, preceding death. On post-mortem examination, about two quarts of blood were found in the posterior mediastinum, and also much blood effused in the spinal canal.

The operation by Mr. Edwards, of Caerphilly, in South Wales, described in the British and Foreign Medical Review, is related in connexion with a notice of Sir C. Bell's Institutes of Surgery, and was performed upon a patient in whom "were present the usual symptoms of compression, paralysis of the organs of locomotion, the rectum, and the bladder. The situation, as far as the operation was concerned, was unfavourable: the lumbar region. The posterior arch of the bone was raised, the symptoms of compression relieved, and the patient did well. As far as other circumstances were concerned," continues the reviewer, "the situation was indeed favourable, for it may have been at a point where the medulla spinalis no longer retains its cord-like form, and where pressure is less suddenly fatal in its effects."

The case related by Dr. Potter occurred in Michigan, and was that of a man aged twenty-one, on whose neck a thick tree-branch fell from a great height, February 23rd, 1844. He became at once senseless, and very shortly stertorous and cold; the pulse became slow and almost imperceptible, and continued so throughout the night. On the day following, stertor continued, and vomiting came on. Much improvement had taken place on the third day, but large quantities of pus began to be passed by the bladder. Large abscesses, which were opened, formed all over the back, and in the glutæi muscles, and sloughs over the sacrum. There was complete absence of sensation or power of movement below the upper part of the thorax. The patient fluctuated very much, being at one time much better, at another much worse, until near the end of May, when he began to expectorate pus. On the 4th of June, after all the preliminary acts of the operation were completed, the operator "proceeded to remove the spinous processes and portions of the vertebræ, piece by piece, till he came to the spinal cord, when, as soon as there was room, he introduced the handle of a small scalpel under the compressing vertebræ, so as not to injure the cord, while he continued to use the forceps. Proceeding in this manner, he removed parts of the four inferior cervical and the two upper dorsal vertebræ. The vertebræ were so much ossified as to render it extremely difficult to ascertain the precise point of compression. There appeared, however, to be but four that were fractured so as to produce compression, although the spinous processes of the two inferior ones removed were more or less fractured. On the whole, ossification pression result from the effusion of blood in the different situations above mentioned."

Amongst the above-quoted opinions of various authorities, we find a very respectable amount of sanction accorded to the operation; several writers of eminence and much practical experience have, however, withheld their approval of it. For example, Sir C. Bell was highly averse to the operation, and Sir B. Brodie by no means a favourer of it. Malgaigne, in his 'Traité des Fractures et des Luxations,' 1847, tome i. p. 415, strongly disapproves of it, except when there is a wound of the surface and the bone is splintered; one of his points of objection arises from the fear of air finding an entrance into the arachnoïd cavity, but it is pretty clear that these fears were not so well founded as he thought. Mr. Alexander Shaw, in his paper upon Injuries of the Back in Holmes's 'System of Surgery,' vol. ii. p. 235, strongly protests against the recourse to the operation in such cases; the vivid sketch which Mr. Shaw depicts of the wound, and its healing, is painful enough, but yet we think careful nursing and constant cleansing of, and attention to the wound, would go far to prevent the evils that might arise from it. In the case before alluded to, in which Louis removed fragments of bone which were creating

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had taken place." The pulsations of the cord were seen and felt clearly, and before the operation the patient felt as if he were being pricked all over. "Sensation appeared to return almost instantaneously," and four or five hours after the operation, "he could readily tell which foot or toe was touched. Sensation was nearly perfect, except in the limb in which was the abscess: in that it was less natural." The discharge from the abscesses, which had been diminishing, now quite ceased, and from this time pus was expectorated in large quantities. "The wound gave but little trouble. Healthy granulations formed and filled up the cavity, and cicatrization commenced around its margin. But the expectoration and difficulty of breathing increased, and the patient died June 22nd, eighteen days after the operation, apparently from suppuration of the lungs," a morbid process which of course existed long before the operation.

The case of Dr. Holscher (described in the Hannoverschen Annalen, vol. iv. p. 330) was one of complete paraplegia from fracture of the eleventh and twelfth dorsal vertebræ. On the thirteenth day after the injury, when sloughing over the sacrum had begun, Holscher removed the posterior arches of these vertebræ, and also some coagulated blood. In six weeks the wound from the operation had quite healed, and eight weeks after the operation sensibility re-appeared in the dorsum of the foot, and afterwards higher up; a few weeks later, the legs could be moved a little, and after twelve weeks the patient could raise himself in bed, and move slightly his legs. He died of hydrothorax, ascites, &c., and after death the spinal cord was found quite healthy, the spinal membranes being dense and vascular at the place of operation, ligamentous tissue uniting the bony parts.

The case of Mr. Jones, of Jersey, related in the Medical Times and Gazette (July 26th, 1856), was that of a man, aged thirty-four, who, whilst drunk, fell into the hold of a ship, February 28th, 1856, his head coming in contact with a block. He was rendered insensible for some time, and afterwards had symptoms of pressure upon the spinal cord. Fracture of the arches of the fifth and sixth cervical vertebræ was detected by cutting down and removing the structures casing several vertebræ, and these were removed. The operation was performed eight days after the accident, lethargy, intermittence of the pulse, and other bad symptoms having arisen; and the effect was, according to Mr. Jones, material improvement, both as regards the pulse, sensation, and general symptoms. The patient, however, died, March 9th, from coma, which suddenly came on. On post-mortem examination, which was unsatisfactory owing to the impatience of friends, the cord was found "entire and un-lacerated in its posterior aspect, but had the appearance of injury on the left side corresponding to the seat of fracture of the bodies of the fifth and sixth cervical vertebræ," which "were preserved in their natural position almost entirely by the integrity of the anterior common ligament, so complete was the injury and displacement." Mr. Jones looked upon the length of time which elapsed between the injury and the operation, and also the want of apparatus for fixing the head and spinal column, as elements of the non-success of the operation.

pressure upon the spinal cord, much suppuration occurred; as Louis says, '*il s'établit une suppuration abondante et louable.*" We do not see, indeed, why suppuration connected with a compound fracture of the spine, either accidentally or intentionally produced, should be attended by worse effects than that which often follows a similar fracture of the cranium.

Again, we think Mr. Shaw does not sufficiently consider the possibility of fracture of the posterior parts of vertebræ without fracture, or at least without displacement and consequent in-driving of bone by direct "blows" concentrated upon the spinal column, *à tergo;* or, again, of the effects of gun-shot wounds. In connexion with this latter point, we find in the same volume as that containing Mr. Shaw's paper, a communication by Mr. Longmore, Professor of Military Surgery, &c., at Fort Pitt, on gun-shot wounds, in which (see page 53) he mentions a case wherein "a ball, which had lodged in the eleventh dorsal vertebra and was causing compression, with complete paraplegia," was removed with the elevator by M. Baudens. In this case "the paralysis disappeared immediately after the extraction of the bullet, but tetanus came on four days afterwards, and proved speedily fatal." Mr. Longmore thinks the operation of trephining the spine can only be resorted to in very rare cases, by reason of the mischief in such cases being usually complicated and extensive; and in the most recent work with which we are acquainted on military surgery,* it is stated, in the section on gun-shot wounds of the spine,† that whilst resection may be useful in cases where projectiles are only superficially lodged in the spine, yet when deeply lodged, or when splinters of bone are in-pressed, resection and trepannation are to be forborne, because we have no surety as to the amount of pressure and as to the true condition of the spinal marrow.

Nevertheless, we cannot ourselves help thinking that as yet we are not in possession of sufficient facts—that is, the operation of cutting down and removing fragments of the vertebræ which are exercising pressure upon the spinal cord has not hitherto been performed with sufficient frequency—to enable us to consider the question as set at rest either in one way or another. Without in any way professing to consider all the bearings of the matter (one which might well occupy a paper of some length), we would observe that, as it appears to us, the question of the performance of this operation involves two considerations—1stly, as respects the chances of recovery from such an operation in any given case; and 2ndly, as respects the general advisability of performing the operation under any circumstances. *The main difficulty, no doubt, is one of diagnosis.* If there be any considerable displacement of large or small fragments of the fractured BODIES of vertebræ without other fracture, then, of course, the operation would be useless, as it would appear to be quite impossible to replace such; but if the pressure be only due to fracture and displacement of the posterior arches, the laminæ, the spinous processes, &c., then, considering the power which the spinal cord has of recovering from injuries, we can see no rational objection to the operation being resorted to under certain circumstances. Of course, even in such a contingency, the risk from any other extensive injury, *or* from utter crushing of the cord, if the accident has recently occurred, or of disorganization from inflammatory changes, if not so recent, must be taken into consideration. Again, the difficulties would be increased if, along with fractured posterior arches, the bodies of vertebræ were also fractured; but if it should be that with this last complication the displacement were very slight, then, considering the free movement which by the operation must naturally be permitted to the spinal cord, according to its anatomical arrangements, there seems reason to hope that a certain degree (and how little might suffice ![*]) of room in a backward direction might, by removing parts of the posterior arch, be allowed to the cord thus encroached upon and pushed backwards from the front; in this manner, at any rate for a time, relief might be obtained; and even to gain a brief period wherein painful spasm may be prevented, a return of sensory and motor power permitted, and disturbance of nutrition and secretion by "*excitement*" of the spinal cord recovered, much ought to be undertaken in such cases.

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* Militär-Chirurgische Studien in den Italianischen Lazarethan von 1859; von Dr. Hermann Demme in Bern.

† Zweite Abtheilung, p. 199.

Possibly more careful observation, keeping in mind our recent advances in knowledge as to the functions of the various parts of the spinal cord, may in some cases actually enable us to determine whether the anterior parts of the spinal cord are affected or not by pressure. We are well aware that, as Ollivier says,[†] in most cases of fracture of the vertebræ, at least from falls, there is more or less fracture of the "*bodies*" of the vertebræ, with such displacement of fragments as to produce encroachment upon and actual trapping of the spinal cord, yet we cannot but suppose (from the anatomical consideration of the parts concerned) that if all fractures of the spine were duly and accurately examined and recorded, including cases from blows and gunshot wounds, as well as those from falls, instances in which the chief or only injury of the cord is from displaced parts of the *arches* or laminæ would be found to be less rare than is imagined.[‡] And Ollivier himself, whilst disclaiming[§] any expectation of good by operative interference when the pressure of the cord arises from displaced fragments of the bodies of vertebræ, observes, "nevertheless, if the fracture has been caused by a direct cause, if it is remotest to the vertebral laminæ, it is conceivable that trephining the spine may offer some chance of success." Careful observation of cases of fractured spine, with special reference to the question whether there be merely fracture, with depression, of the posterior arch, or whether some other fracture co-exist, will alone help to clear up this piece of diagnosis.

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[*] We could point to cases of which we have notes, in which the spinous processes and the bodies also of the vertebræ were considerably fractured, *without the broken fragments of these bodies being at all displaced.*

[†] Loc. cit., vol. i. p. 380.

[‡] No doubt the more careful registration and criticism of cases of fractured spines, which in our hospitals and elsewhere has been made during the last few years, have shown that recovery, without any artificial removal of bone, may be, in such cases, looked for more frequently than was thought possible by former surgeons; for, not to go so far back as Hippocrates and Paulus Ægineta, who thought that recovery was impossible in every case in which there was compression of the spinal cord by depressed bone, it was the opinion of most of our early European surgeons, that any injury producing lesion of the cord, of whatever kind, was inevitably fatal.

[§] Loc. cit., p. 381.

With regard to fears and hesitation in resorting to the operation itself of removing portions of fractured vertebræ—the other element for consideration in this question—we certainly can see no reason for their entertainment. It may be gathered from the cases on record, as well as from operations on the lower animals, and from what happens in the parallel cases of trephining the cranium, that neither is inflammation of the spinal membrane necessarily attendant on the operation, nor is the suppuration connected with the healing process subsequent to the operation to be of necessity dreaded. Again, there is no justification for objection on the score of any contemplated want of power in the re-forming of removed bone: this is borne out as much from experiment on animals as from the results of cases placed on record in which portions of diseased vertebræ in man have had to be removed; and the recent researches of Ollier on the reproduction of bone go far to neutralize any doubts which may exist as to the possibility of such a restoration. We may here also allude to one or two cases which we have known in which death after fracture of the spine arose from pyæmia, apparently owing to the detention of pus about the vertebræ and within the spinal canal, which would most likely have found an exit had trepanning been resorted to.

Dr. Brown-Séquard then, quitting the subject of surgical interference in cases of fractured spine, passes on to observe upon the possibility of preventing sloughs over the sacrum, nates, &c., in cases of injury or disease of the contents of the spinal canal. These secondary affections are considered by him to be owing to disturbance of nutrition (as before said), due to irritation of the cord or of nerves destined for the bloodvessels; and prompted by the results of experiments on lower animals, he suggests that the occurrence of these sloughs, when threatened, might be prevented by the alternate application of powdered ice, and warm poultices. In like manner the "reflex" disturbance of nutrition in organs situated within the several cavities of the body, so frequently produced by burns, and also the attendant pain, are said to be remarkably obviated by the local application of ice to the burnt part, by which the vital properties of the nerves are reduced. Hence, also, the excellence of belladonna as a narcotic in cases of burns in *preference to opium.*

The latter portion of the third part of the appendix is occupied in suggesting and giving reasons for recommending in the case of tetanus, hydrophobia, and other such convulsive diseases (when apparently arising from peripheric irritation), the division of the nerve through which the excitation is conveyed to the central part of the nervous system.

In Part III. (the concluding section) of the appendix, additional cases are adduced illustrating the production of paralysis caused by lesion of the encephalon on the *corresponding* side of the body, a species of paralysis which appears, at any rate in the cases noted, to have been owing to a reflex influence upon some portion of the nervous system, due to an irritation of some part of a crus cerebelli. Cases by Dr. Lente and Dr. Chew are quoted, and one with an accompanying engraving showing the relations of the parts, by Dr. Ogle, cited from the forty-second volume of the 'Transactions of the Royal Medical and Chirurgical Society.'

Having now concluded, to the extent we originally proposed, our analytical notice of one of the works placed at the head of this article, we will at once pass on to the main object which we have in view—viz., the succinct consideration of Dr. Brown-Séquard's new volume on the 'Diagnosis and Treatment of the Principal Forms of Paralysis of the Lower Extremities,' than which disease, so frequent in occurrence, so diversified in characteristics, so multifarious, and in too many cases so obscure in causation as it is, there is hardly any other that could be selected upon which the practical physician would desire sound and reliable information. We are, of course, not about to indulge or provoke our readers, as the case might be, with anything like an exhaustive and systematic dissertation on this subject; we merely set before ourselves the duty of becoming the exponent of the views entertained by the author on the matter, only making one or two comments *en passant*, as opportunity may offer, on the chief and salient details dwelt upon by him, feeling (as the author himself no doubt does) that the final adjudication of several points connected with the matter, owing to the want of material as data, must be postponed of necessity for the present.

The subject is treated of in four lectures, which formed part of a course delivered in the year 1859 at Edinburgh, Glasgow, and Dublin; their principal object being "to point out the extreme importance of a clear diagnosis of the various forms of paralysis of the lower limbs, and especially of the two most frequent and distinct forms—the reflex paraplegia,* and the paralysis due to myelitis." (p. 5.) This diagnosis between two specific affections, in one of which there is supposed to be no organic alteration of the cord, but only a diminished supply of blood; whilst in the other the cord is, according to the author's statement, over-supplied or surcharged with blood, is rendered remarkable by the circumstance that lecture the first is devoted to the elaborate proof of the "very existence" of one of these forms of paralysis—viz., the reflex paraplegia. Truly did that notorious and philosophical character, Mrs. Glass, of immortal memory, preface her instructions for jugging the hare by directing that it should first "be caught." The fact is, that, although discoursed upon and admitted into medical nosology by systematic writers, and implicitly acknowledged by men of high repute, both in past years, as by Stanley,[*] Graves,[†] Stokes,[‡] Rayer,[§] Cruveilhier,[||] &c., and more recently, as by Romberg,[¶] Leroy d'Etiolles,[**] *cum multis aliis;* and although a vast number of isolated cases, demonstrating the possible existence, and illustrating the history of paraplegia as a reflex phenomenon (i.e., as the result of some morbid impression transmitted to the spinal cord from some centripetal nerve, and, *as it were*, thence reflected peripherad), are to be found richly scattered through English and foreign medical literature, the actuality of this form of disease has of late been impugned by physicians of ability and research. Thus, it has been questioned by Dr. Gull,[*] in England, and on the Continent by no less experienced observers

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* It may be well, *in limine*, to observe that this expression "reflex," as used here and elsewhere, is not designed to carry with it reference to any supposed method, or any imagined state of spinal cord by which the paraplegia is brought about—as, for example, by any peculiar or specific state of the capillaries. It should imply only, as in the case of reflex ACTION, that the loss of voluntary power in the affected limbs is the result of some peripheral morbid impression acting upon the muscles of the limbs in a direction backwards *through* the spinal cord, in which structure it may be said to be "reflected" upon them. To speak in this sense is to avoid insinuation of our belief in a mechanism the existence of which we can only guess at.

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[*] The reader will perhaps remember, that as early as the year 1818 Mr. Stanley's attention was directed to the class of cases alluded to, which subsequently, in 1833, he brought before the notice of the profession, in a paper read before the Royal Medical and Chirurgical Society. In that paper are also contained cases illustrating the same pathological point observed by Mr. Hunt, of Dartmouth, and Mr. Horwood, of Gosport. It will be seen that Mr. Stanley fully recognised the influence exercised by the spinal cord upon the kidneys and other viscera by means of the sympathetic nerve, and inversely also the influence which the viscera through the same agency may have on the spinal cord. Thus at page 278 (loc. cit.), after speaking of the influence of the spinal cord on various organs, he says, "But the same phenomena may occur in an opposite order, as in the case of a compound fracture or other severe injury of the lower extremity followed by retention of urine, from irritation arising in the anterior crural and ischiatic nerves, and communicated through the lumbar and sacral plexuses of spinal nerves to the nerves of the bladder."

[†] Dr. Graves' observations, it seems, appeared in print in the London Medical and Surgical Journal, about the same time as Mr. Stanley's paper above mentioned, having been given in the form of lectures at the Meath Hospital, in November, 1832.

[‡] Dr. Stokes' remarks on the form of paraplegia in question may be seen in Renshaw's London Medical and Surgical Journal.

[§] Traité des Maladies des Reins, &c. Paris, 1839. In vol. iii. p. 167, Rayer has a distinct section devoted to 'Rapports de la Pyélite avec les maladies de la Moelle épiniere.' In this he speaks of the lower limbs being frequently the seat of a numbness in disease of the urinary passages. He draws attention to the paper by Stanley just alluded to above, and himself relates cases in which paraplegic symptoms were apparently due to disease of the kidney, bladder, urethra, &c. So it appeared to Rayer himself, but the unequivocal cases in which the connexion between such diseases and paraplegia, or disease of the spinal cord, were too few to evoke the author's certain determination as to their relationship. Rayer does not pass over the observations of Lallemand, who had noticed in his work, 'Des Pertes Seminales Involuntaires,' feebleness and loss of power in the lower extremities in cases of seminal losses; cases in which lesions of the urethra and prostate are often associated. Rayer also quotes a case related by Ammon (Preuss. Medicinisch. Zeitung, 1832, p. 6), of inflammatory disease of the bowels, followed by a bloody state of the urine, owing it was found to morbid deposits in the kidney, in which, towards the close of life, the lower limbs became paralysed.

[||] In the 'Anatomie Pathologique du Corps Humain,' Cruveilhier details a case of disease of the kidneys, in which complete anæsthesia of the lower limbs came on along with other phenomena, no lesion of the spinal cord being found.

[¶] Diseases of the Nervous System. Sydenham Society's Translations, vol. ii. pp. 376–394.

[**] The observations of Dr. Raoul Leroy d'Etiolles are contained in his work, 'Des Paralysies des Membres Inférieures ou Paraplegies; Recherches sur leur Nature, leur Forme, et leur Traitement,' Paris, 1856 and 1857, which originally, in 1853, was successfully written as a prize essay at the Academy of Medicine, and of which, as being not well known in this country, anticipating a fuller analysis to be given at a future time, we will now say a few words. Of the first part of this work, consisting of four chapters, Chapter I. is engaged in considering the question, reviewing the general subject of myelitis, and in enumerating the varieties of paraplegia, independent of myelitis; Chapter II. is concerned with paraplegia produced by disorders of the genito-urinary organs; Chapter III. in considering paraplegia consecutive to hysteria and chloro-hysteria; and Chapter IV. treats of paraplegia consecutive to losses of blood or to anæmia of the lower part of the body. In this part of the book, no less than seventy-five cases are brought together from diverse sources, pointing out the varied regions from which, in reflex paraplegia, the irritation starts; among which, in addition to the numerous affections of the urethra, prostate, bladder, kidney, uterus, &c., we find pregnancy, catamenial suppression, uterine hæmorrhage, artificial parturition, hæmorrhoids, self-abuse, &c., alleged as causes. Experiments undertaken, though unsuccessfully, on the lower animals, with the view of artificially producing reflex paraplegia, are related at p. 114; and mention is made of many instances in which paraplegia is produced in the brute creation, as from

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the arsenical preparations in the case of the dog (as mentioned by Orfila); in parturition and pregnancy in the cow, and in cases of clots in the aorta in the horse, as described by Goubault at Alfort. The causes, symptoms, diagnosis, pathological anatomy, treatment, &c., are given of the various affections treated of. The chapter on hysterical paraplegia is a long and important one; the author evidently looks upon this affection as a reflex form of paraplegia. Although not now concerned with the remainder of this work, of which, as before said, we purpose giving some notice to English readers, when it is completed, we may refer to the first fasciculus of the second part as containing information on paraplegia from metallic agents, from fever, from gastric irritation: a form which he terms "idiopathic," inasmuch as it appears to be independent of any ascertainable cause whatever, is described as rapid and progressive in its approach, unattended by pain on pressure upon the back at any spot, or by any affection of the sphincters, and is for the most part rapidly cured.

[*] See Transactions of the Royal Medical and Chirurgical Society, vol. xxxix. 1856, page 195, "Cases of Paraplegia associated with Gonorrhœa and Stricture of the Urethra." In this communication Dr. Gull adduces cases of paraplegia from disease of the urethra, in which, after death, organic changes were found in the spinal cord. In some of these the cord appeared outwardly to be quite sound, and was only found to be diseased by the use of the microscope. But Dr. Brown-Séquard seeks to overthrow the dictum that actual organic change is *necessary* in such cases of paraplegia, and illustrates his statement by citing a case related by Dr. Gull himself, in the Guy's Hospital Reports, Third Series, vol. iv. p. 174 (1858), in which the microscope quite failed in detecting morbid changes of any kind in any part of the spinal cord, although completely-marked paraplegia had existed, following immoderate venereal indulgence and difficulty in micturition. This case of Dr. Gull's, if not allowed to have been one of urinary paraplegia, may possibly have been (we would suggest) one of cerebral paraplegia, but inasmuch as the head was not examined after death, this is of course mere conjecture. (This omission is to be regretted, as also the non-examination of the various nerves given off from the spinal cord: a point to which Dr. Hinds, of Birmingham, drew attention; see British Medical Journal, April 9th, 1859.) It was not, apparently, the result of any urethral or vesical disease; but again, owing to the acknowledged habits of the "veneris intemperantia," it may have been, after all, one of those cases in which this kind of indulgence has the disease in question for its chastisement. In such cases, though we do not remember to have seen the opinion advanced, it may be that the paraplegia is the direct result of an irritation of the glans penis, as the habits alluded to, if freely indulged, are doubtless a source of such irritation; if so, the case in question may be looked upon as one of what may appropriately be termed *genital paraplegia*. Dr. Handfield Jones (loc. cit., see page 396), also states that in a case of paralysis from cold and wet, where death occurred, he examined parts of the spinal cord without finding any trace of "exudation." Dr. Gull, again, in the volume last mentioned of the Guy's Hospital Reports (p. 176), repeats his observations with reference to the supposition that paraplegia might be produced by morbid impressions conveyed thereto by incident nerves independent of structural lesion. He suggests, respecting the case of paraplegia above alluded to, in which no microscopic change was found, that possibly it would have been found if the patient had lived longer; or perhaps "it might have recovered itself by the slow process of nutrition." Still later, in the sixth volume just issued (which we had not seen when this article was written), we find that Dr. Gull (page 313), in a paper on Paralysis of the Lower Extremities consequent upon Disease of the Bladder and Kidneys, appears to us to have somewhat altered his views upon this subject, for whilst we gathered from the passages formerly quoted that he looked upon the fact of the existence of paraplegia of this kind (however brought about) as undetermined, he now speaks of its occasional existence as an "established doctrine." He, however, concludes that when paraplegia does arise from urinary disease, it is not from irritation, but from inflammation, and that of a chronic kind, of these organs—paraplegia does not, in fact, set in until the suppurative stage has set in (excepting in the case of acute gonorrhœa); and is owing to an extension of disease through continuous structures to the cord itself. We have not the opportunity at present of analyzing this communication, but hope to do on some other occasion. Suffice it to say that among other points of objection, he takes strong exception to the cases of supposed

than Hasse,* Valentiner, and also (as it were on second thoughts, and under the influence, as he appears to admit, of Hasse and Valentiner) by Romberg, in the later (the third) edition of his work (1857);† in the earlier edition of which he had fully described the various degrees of paraplegia arising from diseases of the intestinal canal, dysentery, enteritis, &c., and quoted instances related by numerous writers in support of this view.

Independent of the conviction resulting from the actual observation of cases, such as the frequent forms of paralysis of certain sets of muscles in children, often as transient as unexpected (whether from intestinal worms, disordered digestion, teething); as also from the motley group of what, failing a more appropriate designation, we call hysterical affections, in which muscles very suddenly lose power, and as quickly regain it (and here we distinguish cases of so termed hysteria wherein actual motor power is lost, from those in which merely the *will* is wanting); we ourselves have no more hesitation, and find no greater difficulty in accepting *à priori* the general dogma of reflex PARALYSIS, than in recognising that of reflex ACTION or SPASM. We know nothing, absolutely, of the mechanism (so to say) of such so-termed reflex movements as take place in the healthy human body every moment of our lives. Thus, likewise, is it in disease. We really know no more of the subtle process or delicate changes (molecular or otherwise) under which what we are pleased to call an "*irritation*," or "*excitement*," or "morbid *impression*," at a given

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paralysis mentioned by Leroy d'Etiolles, on the ground that they were only "muscular weakness," as also to one often quoted from Dr. Graves, in which he thinks the paraplegia was owing to vesical distension. He also remarks upon the insufficiency with which the history of urinary paraplegic cases corresponds with that depicted by Dr. Brown-Séquard in his Table of Diagnosis, and finds it impossible to recognise the support which that observer experimentally obtained to his views upon a contracted state of the spinal capillaries, inasmuch as when he performed similar experiments, the expected phenomena of contraction of bloodvessels did not appear to follow the irritation of renal nerves. We cannot say that this paper of Dr. Gull's has materially altered our mind respecting the possible production of paraplegia from affections of the urinary organs, without the production of any organic lesion, and certainly several of his objections appear to us wholly ungrounded. His general observations, however, regarding the value and relation of symptoms, and the due recognition of the *order* of morbid processes, are of the highest value.

* See pp. 116, 117 of the Krankheiten des Nervenapparates, by K. E. Hasse, in the Handbuch der Speciellen Pathologie und Therapie, Band iv. abtheil. 1, edited by Virchow. It will be found that Hasse's main difficulty in recognising diseases of the kidneys, uterus, &c., as causes of paraplegia, arises from the "*in*frequency" with which such a disease is so produced. Paralytic symptoms, he considers, ought not to fail (according to the view which he is slow to accept) in the somewhat common cases of almost complete atrophy of one or both kidneys. Nevertheless, in conclusion, Hasse is compelled to admit that these remarkable and rare cases are not at present capable of explanation.

† Romberg, after allowing the existence of reflex immobility of muscles, and alluding to Dr. Stick's use of such phenomena as a means of diagnosis in disease of the spinal cord (Annal. d. Charité-Krankenhaus, 1856, B. vii. S. 158–171), here says (p. 913): "Ein übergang dieser reflectorischen Immobilität in Lähmung ist bisher weder durch experimentelle noch klinische Beobachtung erwiesen, und meine frühere annahme von Reflexparalysen, gegen welche sich von verschiedenen Seiten (Hasse, Valentiner, &c.) Zweifel und Widerspruch erhoben haben entbehrt eine sichere Grundlage."

‡ Sir Charles Bell had asked the question (see 'The Nervous System of the Human Body,' 1844, p. 356), "are we to admit or deny this influence of deranged bowels, of visceral irritation, in producing external pains, local paralysis, or partial spasms? No man who attends to disease can deny the existence of this influence."

peripheric locality induces the more familiar "spasm" of muscles or capillary vessels, through the intervention of a central nervous structure, than we do of the conjectured intermediation by which a paralysis or "want of action" at certain times and in certain cases results from some distant and unwonted irritative influence or impression propagated centripetally.

Connected with this subject is another perplexing point of investigation, and that is, the determination of the exact circumstances under which it is rendered possible that an ex-centric irritation or spasm should, in a given case, so act upon the central spinal cord as to give rise either to paralysis or spasm of muscle or vessel. The frequency with which such ex-centric morbid impressions must of course occur in the system, and yet be unattended by any phenomena of the kind mentioned; and the apparent disproportion which mostly exists between the intensity of the irritation and the resulting evil effects, render it perfectly clear that some special but at present unknown condition or attitude of the nervous system generally, or of particular parts of the nerve-structure, must of necessity be coïncident in cases of reflex paralysis or spasm, without the existence or co-operation of which the given irritation would be wholly powerless in producing these specific effects on the central nervous parts, whatever their nature may be, upon which these states depend. This presumed exceptional condition or disposition of the nervous system (whether of spinal cord, or nerves, or the sympathetic system of nerves), we believe to be all-worthy of closer investigation than usually is accorded to it by neuro-pathologists, surrounded, as from the nature of the case it must be, by many difficulties; and we recommend it, as a fitting object of exploration, to the attention of those who are now on every side so fruitfully developing our knowledge of the history and progress of diseases of the nervous system.

On the subject of reflex paraplegia, Dr. Brown-Séquard establishes two propositions. Firstly, that a paralysis of the lower limbs may be caused by an alteration in the periphery or the trunk of the various sensational nerves; and, secondly, that this kind of paralysis differs extremely from the other kinds of paraplegia in many of its symptoms, and by the frequency and rapidity of its cure. (p. 7.) Both these enunciations are arrived at by inference from numerous cases related, showing that rapid or immediate cure of paraplegia followed "the removal or cure of the alteration of a nerve which was considered as the cause of the paralytic affection," it not being possible that such a rapid cure could occur if the paraplegia were determined by organic disease of the spinal cord or its membranes.

Dr. Brown-Séquard details a number of facts, accumulated from various sources and by many well-known authors (which we cannot now specify), *demonstrating that all the other parts of the body may be affected with paralysis of a reflex kind:* from which he very justly (as we think) argues the possibility of the production of this affection in the lower limbs. Even the paralysis depending on muscular atrophy he considers in many cases (p. 10) to be of a reflex origin, the atrophy being due to a peripheric excitation, such as neuralgia. He then proceeds to relate several cases, placed on record by competent and trustworthy observers, of reflex paraplegia depending upon disease of the uterus, urethra, kidney, bowels, thoracic viscera, joints, skin, &c.* At this stage of the inquiry two objections which have been offered have to be met and disposed of—viz., in the first place, that in the cases cited there is no need to admit the principle of

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* On reference to Dr. Graves' 'System of Clinical Medicine,' 1843, p. 396, wherein he alludes to his previously-published Lectures on the subject, it will be seen how clearly that observer had recognised the existence of cases of reflex paraplegia, though he had not ventured or was able, indeed, to indicate the rationale of such cases, further than to speak of "impressions made on the extremities of the nerves, generating a morbid action in them; that this morbid action will be conveyed along their branches or trunks to the spinal cord or brain; and that, continuing its propagation, it may, by a retrograde course, be carried thence along the nerves to distant organs, and in this way give rise to disease in parts originally intact and healthy." Referring to injuries done to the "EXTREME BRANCHES" of the nervous tree, he proceeds to say that "the lesion is not confined merely to the part injured, but in many instances is propagated back towards the nervous centres; and that in this way not only the nervous filaments of the injured part may be affected, but also the main trunk of the nerve and other branches, or that the lesion may reach the brain or spinal cord, and thus produce still more extensive effects on the system." And again, "What I endeavoured to impress upon the class at that time was, that pain, numbness, spasm, and loss of the power of muscular motion, may be produced by causes acting on the extremities of the nerves, and that such affections commencing in the extremities of the nerves may be propagated towards their centres, so as to be finally confounded with diseases originating in the centres themselves." (p. 397.) Dr. Graves then recapitulates some of the data on which his views were founded. Numerous cases are detailed and quoted, showing the dependence of paraplegia, as well as of various other forms of paralysis, upon disease and irritation of peripheric parts, as of the bowels, kidneys, urethra, lower extremities, &c.

Our more ancient medical records clearly show that the existence of various forms of paralysis without any organic lesion of the spinal cord being thought of or before discovered after death, had been long familiar; and without professing to go very far into the literature of the matter, we think the following instances of what we say, cited from medical literature of various periods, may be adduced as interesting to the reader. Thus, a case is recorded by Lælius a Fonte Eugubinus, a Venetian physician (whose work, 'Consultationes Medicæ,' republished at Frankfort in 1609, will be found curious and worthy of examination), and cited again by Lieutaud in his work, 'Historia Anatomico-Medica;' Parisiis, 1768, liv. i. observ. 68, in which disease of the kidneys was supposed to have given rise to paralytic symptoms. Of this case, in which fever and suppression of urine preceded entire loss of sensation and motion in the legs, Lælius a Fonte remarks, that by maltreatment, the blood which had been driven to the inferior parts "malignam in renibus inflammationem produxit, quæ *spinali medullæ communicata*, potuit spiritum ac virtutis animalis influxionem arcere, quæ per propagines nervorum sexti paris, vesicæ impertitur, &c." After death it is said by Lælius, "renem sinistrum compertum esse nigrum, et ab *eodem latere* etiam spinalem medullam affectam." (loc. cit. p. 364.) Again, Theophilus Bonetus, in his recondite "Sepulchretum," lib. i. sectio xv. De Paralysi, shows clearly, that in the year 1700, the connexion between diseases of the viscera and various degrees and forms of paralysis was recognised. In his remarks upon Observation 2, alluding to Willis 'Cap de Paralysi,' he observes, curiously enough too, referring to us Englishmen: "Affectioni colicæ crebro et gravius obnoxii, demum paralytici evadunt: casus adeo frequens *apud Anglos* ut morbi hujus successio inter istius prognostica habeatur: nam qui torminum in ventre, aut paroxysmos atrociores per intervalla redeuntes pati solent aut doloribus circa abdominis viscera paulo atrocioribus fere continue molestantur, tandem in habitu corporis ac memoris dolores vagos, dein postea stupores, et denique non raro ἀκινησίας sive resolutiones subeunt; Hujus modi effectus causa, tum a morbi sede, tum a materia morbifica immutatis procedit: Hæc nimirum quæ per-exigua, sed acris et irritativa, nervos tantum splanchnicos incurrebat, perindeque ob viscerum fibras vellicatas in iis spasmos et dolores ciebat, postea copiosior, simulque hebetior et narcotica evadens, *per spinalem medullam depluit, atque nervos his aut illis membris, aut musculis destinatos subiens, in partibus respectivis resolutiones parit.*" Bonetus also, in his remarks upon Observation 6 of this section, adduces the authority of several in support of the alleged connexion between affections of the colon and paralysis. Among others, he gives the supposition of Spigelius, that the morbid humour passed from the intestines into the lumbar vessels, which became causation enunciated; and, in the second place, that it is impossible at all to explain the production of a paralysis by a reflex action. (p. 19.) Of these objections, the former might be grounded on the supposition that, as regards the cases of paralysis from diseased uterus, the cause

swollen, and so pressed upon the nerves and induced paralysis of the legs. At page 375 he records a case 'imbecillitas totius corporis et *δυσκινησια*,' from disease of the spleen and liver.

Willis also gives proof of the supposed dependence of paralysis of a certain kind upon visceral disease, as, for example, when speaking of colic (see cap. xv. of the Pars Pathologica of his Treatise 'de Anima Brutorum,' in the Opera Omnia, published at Amsterdam, 1682), he says, "Postquam dolores colici aliquandiu in *ventre* desævierunt sæpenumero *lumbos*, et dein, morbo ingravescente, membra, et musculos fere omnes in toto corpore incessunt, tandemque non raro in paralysin terminantur." He then goes on to say that the "materies morbifica" is not transmitted by *arteries* but by *nerves*, and that it "in spinalem *medullam* illabitur, quam subiens, ac in imum ejus subsidens primo in *lumbis*, ac postea nervis plerisque aliis, qui à spina procedunt, affectis, in membris, et musculis aliis *dolores* exoriri facit; denique nervorum ductibus, à Materia morbifica in ipsis ad plenitudinem aggesta, infarctis, paralysis infertur." In the above quotation we have a little of the nerve-pathology of that day.

Morgagui also met with instances supposed to illustrate the same subject; and this author mentions a case in the chapter on wounds of the thorax in his work 'De Causis et Sed. Morb.,' &c., in which numbness or paralysis from the middle of the body downwards, was caused by a wound of the thorax, owing, as he thought probable, to "injury of the INTERCOSTAL NERVE and lesion of the GREAT SYMPATHETIC."

Camper was of opinion that it was in virtue of the communication between the radial nerve and the sympathetic that painters' colic "*and other affections of internal parts*," produced paralysis of the arm. (See his Anatomico-Pathologicæ Demonstrationes, lib. i. p. 12.) He attributes paralysis of the lower limbs in colic to the union of nerves of various parts with those of the intestines, especially those parts whose nerves are united "cum intercostali aut octavo pari" (loc. cit. 8.) We may incidentally mention that he also describes a case of immobility of the wrist from writing, cured by removing derangement of the stomach and bowels. (p. 12.)

Portal, in his Cours d'Anatomie, vol. iv. pp. 275, 341, and 319, suggests that it is by connexion between the sacral and visceral nerves and the sympathetic that diseases of the intestines and pelvic contents produce paralysis and atrophy of the limbs.

Broussais alludes to cases of what are termed "SYMPATHETIC PARALYSIS," owing to affection of the liver, spleen, kidney, &c.; and Van Swieten, in his Commentaries, vol. i. p. 343, mentions as a reason rather against the opinion (which thus evidently prevailed) that complete paralysis may arise in the leg and thigh of the same side from an alteration in the kidney, the fact that the large nerves within the abdomen distributed to the leg are so placed as scarcely to be compressed by any so large a swelling of the kidney.

Cullen formerly, and later Chomel, Trousseau, with many other writers, have spoken of paralysis independent of myelitis or other affection of the spinal cord, and many have engaged in the consideration of what they term "idiopathic" paralysis — i.e., paralysis without discoverable lesion It will be found also that Ollivier d'Angers, in his work 'Traité de la Moelle Epinière,' vol. ii., relates cases in which, owing to suppression and retardation of the catamenial discharge, more or less paraplegia, along with other symptoms, was originated.

In the systematic Nosographie Organique of Boisseau (Paris, 1830, vol. iv. p. 774), we find the following statement: "Il est possible que la lesion des nerfs ganglionnaires peut sympathiquement déterminer des paralysies, presque toujours alors de peu de durée;" and at page 668 he enumerates, among other causes of injury to the spinal cord, "les contusions et les compressions de l'abdomen;" "les lesions du placenta de la matrice;" "l'abus des organes sexuels;" "les irritations gastriques." Again, in the Dict. des Sciences Médicales, 1819, vol. xxxix. p. 277, we find that, among other causes, paraplegia is attributed to "les alterations sympathiques qu'elle éprouve à l'occasion de certaines affections gastriques et intestinales," &c. Lastly, it may be remembered that in 1855 the Montpellier Academy of Sciences and Letters proposed the following thesis for a prize: "Des paralysies qui paraissent indépendantes de toute lésion appreciable des centres nerveux."

The above, selected from a great number of instances and facts related by older authors, will amply suffice to show how experience had led them to associate loss of muscular power with morbid irritations—to recognise, in fact, a reflex paraplegia.

Not to multiply indefinitely proofs of the recognition of what we now call reflex paralysis, we may mention such titles of published works as the following:—Dissertatio de colica passione, ejusque symptomate illustriori paresi, 1618; Dissert. de parawas pressure upon the obturator nerve and sacral plexus;[*] and that in cases of disease of the genito-urinary organs or the viscera, the *causa vera* was to be sought in uræmic or other poisoning of the blood; whilst in those cases of paraplegia from external cold or wet, we have instances of rheumatic paralysis; and in cases supposed to arise from affections of the skin, joints, &c., the paraplegia had really no real relation to these local affections, being merely coincidentally produced, along with them, by some common affection of the cord. Our author admits that in some cases the paraplegia is produced in the varied ways here alleged; but affirms that in others, without doubt, we have neither sufficient enlargement of the womb to produce the suggested pressure,[†] nor any relation whatever between the paraplegia and any retention of urine, &c. Again, when we do have paralysis from obvious uræmic poisoning, other symptoms are, in addition, produced; and Dr. Brown-Séquard thinks it is difficult to conceive how a complete paraplegia can be produced by a peculiar poisoned condition of the blood which "circulates elsewhere," without some other part than the lower extremities being also affected.

We agree with our author, that this supposition is difficult of reception, yet we must not forget that there are cases, and some are even quoted by himself, showing that a certain degree of paralysis of muscles may arise from a general state of the blood, and if any degree of paralysis may so arise, why not, it may be asked, complete paralysis? For example, at p. 42 of this work, where, speaking of the various kinds of paraplegia which have to be contra-distinguished, he makes mention of "those resulting from poisoning by carbonic acid, lead, arsenic, mercury, opium, belladonna, tobacco, camphor, mushrooms, fish, and those resulting from loss of blood," &c. Certainly among these various causes are some which operate locally only by virtue of a general state of the blood. Again, we have ourselves records of cases in which *hemiplegia* appeared to be connected with uræmia without any actual brain lesion discoverable to the eye having been found after death.

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lysi ex colicâ, 1623; Dissertatio de paralysi dysentericis familiari, 1750; Dissertatio de methodo, paresin ex colicâ rationi convenienter curandi, 1762; Dissertatio de paralysi brachii unius et pedis alter-utrius lateris, dysentericis familiari, 1750; &c. &c.

[*] The consideration of this objection recalls to our mind a case related by Morgagni, showing, as he conjectures, that difficulty in moving the thighs is apt to remain after parturition, owing to bruising or injuring of the obturator and iliacus internus and psoas muscles, which are contiguous to the gravid uterus on the inner surface of the pelvis. See the 'De Causis Morborum,' &c., vol. ii. chap. 4; and also the mention made by Portal (loc. cit. pp. 275 and 279), of numbness in the thighs in pregnant women, from pressure upon the crural nerve, and of paralysis and atrophy of the lower extremities by pressure on nerves within the pelvis from congestion or obstruction of the rectum or from enlarged hæmorrhoïdal veins. The numbness, or as he expresses it, the "stupor femoris," of pregnant women, is ascribed by Camper (loc. cit. lib. ii. p. 7) to pressure on the obturator or posterior crural nerve.

[†] It is of interest to remark that, according to Mr. Stanley's before-quoted communication (Transactions of the Royal Medical and Chirurgical Society, vol. xviii. p. 270), Mr. Hunt had noticed the occurrence of "cases of disordered uterus combined with loss of power in the lower limbs, in such a degree, that the patients were wholly confined to their beds; adding that, by the subsequent and perfect recovery of some of these patients, it was clearly proved there had been no change of structure in the parts to which the symptoms were referred as the source of irritation."

We have on record many instances among the lower animals of paralysis of the lower limbs following the use of certain metals; thus the hinder parts of the dog are known at times to have become paralysed when antimony is given for the mange. In connexion with this subject, we may quote the supposed affection of the vagi nerves in hooping cough by the specific poison; and refer to the opinion of those who hold (as alluded to by the late Dr. Todd in Part iii. p. 115, of Todd and Bowman's 'Physiology,') that neuralgia, as of the branches of the fifth cranial nerve, is owing to their forming "a focus of attraction" for a morbific matter generated in the blood in persons exposed to the paludal poison — a poison which of course must affect the entire mass of circulating blood. See also in Dr. Todd's 'Collected Clinical Lectures,' 1861, p. 51, a statement referring to periodical neuralgic affections being "due to the determination of some poison to a particular nerve."

In certain of the cases of reflex paraplegia in which the relation between paraplegia and some affection of the joint or skin was thought by objectors to be only that of having a common cause, it seemed clear that some closer connexion than this existed, as there was not merely "referred" pain due to a centric cause, but real inflammation of the joint, skin, &c. To this difficulty in diagnosing the various forms of paraplegia, we shall allude later on.

In reference to the second objection, that concerning the positive method in which a reflex action can produce paraplegia, the mode of action involved in the causation is believed by our author to be twofold; being, firstly, a "*contraction of the bloodvessels,*" either of the spinal cord, or of the motor nerves, or of the muscles, owing to irritation starting from various parts and reflected upon the structures mentioned; and, secondly, want of nutrition,* but this is probably the case only when progressive and rapid atrophy of muscles occurs. (p. 25.) The statement promulgated as to the dependence of this form of paraplegia on a contracted state of the bloodvessels of the cord through reflex transmission of peripheral irritation, is made to rest on the absence of any visible changes in the cord as to colour, consistence, vascularity, &c. (post mortem), in such cases, and upon actual experience as to the contraction of bloodvessels of the spinal pia mater produced experimentally by irritation of the spinal nerves.

As regards this last pillar of substructure upon which the statement or opinion just named is built, the matter will be better understood if we quote our author verbatim. At p. 24 of his work on 'Paralysis of the Lower Extremities,' he observes—

> "A contraction of bloodvessels in the spinal cord I HAVE SEEN (in the vessels of the pia mater) taking place under my eyes, when a tightened ligature was applied on the hilus of the kidney, irritating the renal nerves, or when a

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* The very interesting subject of the reflex influence on nutrition (and secretion) will be found well considered in the tenth of the Lectures on the Physiology and Pathology of the Nervous System placed at our heading, page 151, where reasons are given for supposing that the reflected irritation acts directly on the tissues, producing alterations of the interchanges naturally existing between them and the blood. See also Landry's 'Traité Complet de Paralysies,' tome i. part 1, pp. 43–47.

similar operation was performed on the bloodvessels and nerves of the supra-renal capsules. Generally in these cases the contraction is much more evident on the side of the cord corresponding with the side of the irritated nerves, which fact is in harmony with another and not rare one, observed first by Comhaire (as regards the kidney), and often seen by me after the extirpation of one kidney, or one supra-renal capsule—i.e., a paralysis of the corresponding lower limb."

This, then, in short, is the grand central idea upon which the doctrine of reflex paralysis is based, the regulating point by which it is directed: "*The production of a contracted or spasmodic condition of the bloodvessels of the spinal cord, owing to an irritation reflected upon their walls, and originating from without, unaccompanied by actual structural alteration.*" This view, indeed a novel one, not put forth dogmatically by our author, as far as we can discover, as absolutely and unconditionally true, has certainly sufficient basis for its foundation to raise it from the level of a mere fanciful hypothesis or impotent conclusion. It would have been, we conceive, within the limits of scientific propriety to recognise it as something more than arbitrary, if only arrived at by a method of exclusion, all other supposed causes of the paralysis being determined as insufficient; but, as we have seen, the author has a certain amount of auxiliary analogical support on his side, and as the Arabs say, "a small date stone may prop up the water jar." For granting, as we must, according to our author's statement, the production of spasm of the capillaries of the intra-spinal pia-mater, as a consequence of experimental irritation of spinal nerves, it is but a single and a short step further to conceive the possibility of spasm of intra-medullary bloodvessels: and we know, from pathological observation and from actual experiment, that arrest of blood-circulation in nervous structures will diminish, and ultimately arrest, the function of the delicate structures whose blood-supply and consequent nutrition depend on those vessels.

The author does not, however, suppose, apparently, that in this reflex paraplegia the vessels of the spinal cord alone are contracted, for he proceeds to say (loc. cit.)—

> "It is probable that irritation, starting from the urinary and other organs, produces a paraplegia by a contraction rather of the bloodvessels of the spinal cord than of those of the motor nerves and muscles. However, in this form of paraplegia it is not rare that a notable diminution of temperature of the paralyzed limbs shows that the bloodvessels of these parts are also contracted."

As we before remarked, this view is at least novel, for we have been in the habit of looking upon temporary and transient paralyses or convulsions of parts of the body, as connected with temporary and transient "congestions" of the nervous centres—i.e., with the presence of an unusually large quantity of blood in their vessels. Thus, in reference to certain cases in which paraplegia, along with other paralytic symptoms, was occasioned by peripheric irritation, related by Ollivier d'Angers (as mentioned by us at p. 391), we find that author observing that, "*he does not for a moment doubt that these phenomena* *were produced by a momentary spinal congestion, of which the lower part of the cervical region and the superior dorsal part of the spinal cord and its membranes were the seat; it was evident that this temporary afflux of blood in the spinal vascular system had taken place when the uterus ceased to be a centre of derivation for this fluid.*"

Such speculations as the above one quoted from Ollivier have hitherto for the most part prevailed, and we can easily understand that objectors will be found to Dr. Brown-Séquard's conclusions regarding the connexion between this variety of paralysis, and a contracted state of the capillaries of the spinal cord, and will gravely pronounce the Caledonian verdict of "non-proven." Others, it may be, allowing the possibility or even probability of a contracted condition of the capillaries of the spinal cord or spinal nerves, or of the muscles, as a result of outside irritation, may be indisposed to admit that it is by reason of its arresting nutrition that this condition is connected with the paralysis in question. At any rate it may be retorted by its advocates that such objectors are not in a position by actual observation to "disprove" this conjectured association, and we are certainly unaware of any sufficiently weighty antecedent or collateral reasons for repudiating it. Moreover, it may be, and has been objected, that such a prolongation of spasm of the bloodvessels as Dr. Brown-Séquard's view supposes, is of very unlikely occurrence. We do not see any inherent difficulty in this matter. Observation furnishes us with instances in which capillary vessels are maintained in a dilated state for weeks and months, under certain disturbances of the nervous system, as for example, in the case of the vessels of the conjunctiva in some forms of neuralgia. Why may such vessels not remain long in the opposite state? Again, permanent contraction of various muscles by nervous irregularity is well known, and tends to support the possibility of the conjecture. Setting aside the assumption that the paralysis from irritation of a peripheral portion of the nervous system is conditioned by a contracted state of the capillaries either of the spinal cord or of the motor nerves, or of the muscles themselves, by reflex action, through the intervention of the spinal centre; there are yet other conjectures which may be entertained on the subject, and which are equally tenable with the previous one, being alike incapable of disproval. For example, acknowledging as a first principle that the walls of capillary vessels may be acted upon by reflex nervous action, it may be held that the supposed outside irritation or morbid impression may produce paralytic results by virtue of a reflex depressing or paralytic effect upon the vaso-motor nerves of the paralysed muscles, the "sympathetic system of nerves alone" being internuncial; for any one inspecting the relation of the various portions of the sympathetic system to each other along the back of the abdomen, and thorax, and their connexion with the large bloodvessels, may easily conceive the existence of such an intercommunication as would allow an impression to be conducted from any one part of the sympathetic tract to another, without the spinal centre being at all concerned in the transit.

Again, another conjecture: even allowing the necessity of the spinal cord being directly concerned, and, as it were, interposed in the course taken by the supposed morbid impression, why may not this reflex paralysis be a pathological illustration of that depressing or inhibitory effect upon the nerve-structure itself of the spinal centre (apart from its contained capillaries), which we know that certain impressions upon an afferent nerve may produce? As physiologists, we are now in a position fully to recognise the possibility of such an influence being exercised; and there are not wanting a variety of pathological facts which may fairly be considered as applications of this principle.[*] What this influence, so exercised on the central nervous material may be, or HOW it necessitates a paralysis of motor nerves connected with that particular part of the nervous centre, is another and further speculation. This entire question is obviously closely allied to that general one before alluded to, concerning the influence possessed by the sympathetic or ganglionic system of nerves upon the bloodvessels by means of vaso-motor nerves, and to the subject of nutrition and secretion as phenomena dependent on nervous influence, in connexion with which the Prize Essay by Dr. H. F. Campbell, of America, on the "Excito-secretory System of Nerves," may be consulted.[†]

Leroy d'Etiolles, at page 124 of his work before noticed, under the heading of "Hypotheses sur la manière dont se produit cette Paraplegie," asks whether it would be reasonable to consider that in cases of paraplegia from disease of the kidneys this affection was owing to the absorption of pus, which reäcted upon the spinal cord, and for a time arrested the nervous influx? This suggestion, not by any means chimerical in some cases, accords with that made lately by Dr. Gull, namely, that "instead of regarding the nerves as the channels through which the cord is secondarily affected in disease of the urinary organs, we ought rather to look to the *veins* or *the blood itself* as the means by which the lesion is propagated, and instead of attributing the paraplegia to functional depression of the nervous energies, to refer it to inflammatory changes." The paraplegia from seminal losses Leroy d'Etiolles considers to be owing to perturbation (*ébranlement*) of the brain and the large expenditure of the nervous fluid, as well as to the feebleness resulting from an exaggerated secretion.

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[*] This question of inhibitory or depressing influence exercised by afferent nerves on their nervous centres, is well and fully commented upon by Dr. Handfield Jones in the British Medical Journal, February 5th, 1859, where apposite allusions to various modern observers and writers on the subject, such as Pflüger, Lister, Bernard, Weber, &c., are made, with numerous relations of pathological cases bearing upon and illustrated by the subjects.

[†] In reference to this subject, we may quote a passage or two from the antiquated but by far from uninteresting or uninstructive works of Dr. Anthony Nuck, quondam Professor at Leyden. In his 'Sialographia et ductuum aquosorum Anatome Nova,' Lugduni Batavorum, 1695, at page 24, "De Saliva," after speaking of the various theories advanced in explanation of the secretion of their proper fluid by the salivary glands, and referring to the very numerous nerves distributed to the glands, much more than is sufficient for their movement and sensation, he proceeds to observe: "Si ligatura injiciatur in nervum ad glandulam tendentem, aut transversim dissecetur nervus, non tamen ideo secretionem cessare observimus, sed tardiorem solum modo et magis lentam procedere." Again, at p. 16 of his 'Adenographia Curiosa et Uteri Fœminei Anatome Nova,' Lugduni Batavorum, 1696: under the second chapter, "De Glandulis Conglomeratis vulgò dictis," he has the following passage: "Ductus autem hi Lactiferi soli non sufficiunt ad lac secernendum, sed simul necessarium fuit, per nervos numerosissimos, ex nervis thoracicis oriundos, copiam spirituum affluere, lactis secretionem promoventium. Constat enim, obstructis compressione nervis, humorum secretionem, aliis in partibus, aut lentò procedere, aut planè cessare." Such expressions are certainly to be considered remarkable, as indicating a very early foreshadowing, by means of experimental research, of much that has in quite recent times received fresh and more explicit attention concerning the secretory system, and of our present knowledge of the dependence of the secretions upon the bloodvessels and nerves distributed to the glands and their excretory ducts.

Lecture II. is devoted to the Diagnosis and Treatment of Reflex Paraplegia, with special notice of "reflex urinary paraplegia"—i.e., paraplegia arising from some impression or irritation in the urinary organs, *without* or independent of structural alteration in the spinal cord or its membranes. The before-mentioned objections advanced by Dr. Gull[*] to the supposed existence of such a form of disease are combated (p. 31), and it is concluded that the facts adduced by that physician do not prove, as he inferred, that when irritation of the urinary organs causes paraplegia, it is of necessity by virtue of myelitis being produced; inasmuch as, by Dr. Gull's own showing, the spinal cord may, even in cases of paraplegia where the cord was expected to be diseased, remain, *even microscopically*, healthy. Of course such urinary irritation may in some cases be connected with myelitis by the propagation of some inflammation of a vein from the urinary organs to a vein of the spinal cord;[†] "OR," again, the urinary disease and the paraplegia may be merely associated, and only related as having some cause in the diseased spinal cord, common to them both.

In diagnosing the form of paraplegia termed "urinary" from that produced by myelitis, Dr. B.-Séquard introduces the following interesting table (p. 33), giving a synoptical view of the various (we suppose *generally*) observed differential symptoms:

Urinary Paraplegia.	*Paraplegia from Myelitis.*
1. *Preceded* by an affection of the bladder, kidneys, or prostate.[‡]	1. Usually no disease of the urinary organs, except as a *consequence* of the *paralysis*.[‡]
2. Usually lower limbs alone paralysed.	2. Usually other parts paralysed besides the lower limbs.
3. No gradual extension of the paralysis upwards.	3. Most frequently a gradual extension of the paralysis upwards.
4. Usually incomplete paralysis.	4. Very frequently paralysis complete.
5. Some muscles more paralysed than others.[§]	5. Degree of paralysis same in the various muscles of lower limbs.

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[*] See p. 387.

[†] We know of only one case of what appeared to be urinary reflex paraplegia, in which disease of an intervening vein appeared to propagate disease to the cord from pelvic viscera. It may be a point for consideration whether myelitis occurring in old-standing disease of the urinary organs, may not be of the nature of what are called "secondary deposits."

[‡] Dr. Graves (loc. cit.) mentions that, according to his observation, in reflex paraplegia, the urine is turbid, scanty, and voided oftener than usual, but that he had "*never seen it in any case decidedly ammoniacal, even in the advanced stages of the disease, and when the patient was completely bed-ridden.*"

[§] Graves (loc. cit. p. 418) says that this reflex paraplegia in all cases assumes a creeping form, and "generally appears at first in one limb, and afterwards in the other."

Urinary Paraplegia.	*Paraplegia from Myelitis.*
6. Reflex power neither much increased nor completely lost.	6. Reflex power often lost, or sometimes much increased.
7. Bladder and rectum rarely paralysed, or at least only slightly so.	7. Bladder and rectum usually paralysed completely or nearly so.
8. Spasms in paralysed muscles extremely rare.	8. Always spasms, or at least twitchings.
9. Very rarely pains in the spine, either spontaneously or caused by pressure, cold, &c.	9. Always some degree of pain existing, spontaneously or caused by external excitation.
10. No feeling of pain or constriction round the abdomen or chest.[*]	10. Usually a feeling as if a cord was tied tightly around the body at the upper limit of the paralysis.
11. No formication, no pricking, no disagreeable sensation of cold or heat.	11. Always formications or pricking, or both, and very often sensations of heat and cold.
12. Anæsthesia rare.[†]	12. Anæsthesia very frequent, and always, at least, numbness.
13. Usually obstinate gastric derangement.[‡]	13. Gastric digestion good, unless the myelitis has extended high up the cord.
14. Great changes in the degree of the paralysis, corresponding to changes in the disease of the urinary organs.	14. Ameliorations very rare, and not following changes in the condition of the urinary organs.
15. Cure frequently and rapidly obtained, or taking place spontaneously after a notable amelioration or cure of the urinary affection.	15. Frequently a slow and gradual progress towards a fatal issue; very rarely a complete cure.

Such are the distinctive features which the author establishes as in the main observable between the two classes of disease in question. Not, we suppose, that he intends it to be understood that every one of the enumerated peculiarities are observable in each case, but that, by comparing a large number of cases belonging to one or other category, such distinctions are, on the whole, capable of being made; just as we may state that stone in the bladder produces pain at the end of the penis, or that pain at the knee is a symptom of diseased hip-joint, or that epileptic fits are attended by biting of the tongue or foaming at the mouth, without affirming that such attendants are invariably necessary for the identification of those various diseases.

As regards the state of the muscles in reflex paraplegia, they DO NOT, as a rule, become atrophied, although exceptions to this exist.

The author then, reviewing the characteristic symptoms of each form of paraplegia, goes on (page 35) to indicate how a reflex paraplegia is to be contra-distinguished from paraplegia resulting from (1) myelitis; (2) meningitis; (3) pressure by a tumour, diseased bone, &c.; (4) tumour in the grey matter of the spinal cord; (5) hysteria, which variety is considered, as it is by Leroy d'Etiolles, to be probably but a reflex paralysis; (6) seminal losses; (7) hæmorrhage within the spinal canal; (8) hæmorrhage in the grey matter of the spinal cord; (9) spinal congestion; (10) spinal effusion;[*] (11) non-inflammatory softening; (12) obstacles in the circulation of blood in the aorta or in its principal ramifications in the pelvis (generally an aneurysm); (13) pressure on nerves in the pelvis.[†]

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[*] In Mr. Stanley's paper before alluded to, Mr. Hunt speaks of "a peculiar feeling of tight wires or cords" as existing (in cases of reflex paraplegia) in different directions "*through the limbs.*" This symptom is not usually observed or alluded to.

[†] In most of the cases adduced by Mr. Stanley as illustrating reflex paraplegia, it is observable that sensation WAS greatly affected: as much so in the majority of cases as the muscular power.

[‡] We have been for a long period watching a case of reflex urinary paraplegia, most probably arising at the onset from stone in the kidney, in which the functions of the stomach have never been seriously disturbed. In this case there is considerable anæsthesia below the knees.

We confess to a disappointment in not finding any remarks offered with regard to a form of paraplegia which at various times and by numerous writers has been supposed to exist, but regarding which there appears but little recent evidence or experience of a strictly precise or satisfactory nature. We allude to paraplegia occasioned by disease "*within the cranium.*" That our older pathologists recognised this form of paralysis, may be gathered from Morgagni, who in his work, 'De Sedibus et Causis Morborum,' in Section VI. of his Chapter I. (on the Diseases of the Head), describes "the lower limbs, as far as related to muscular action," as being paralytic, owing to a scirrhous state of the cerebellum; and a case is mentioned by Bonetus, in his 'Sepulchretum,' vol. ii. p. 369, of paraplegia following spasm from effusion into the cerebral ventricle and "dryness" of the brain.

It will be found that in August, 1814, Mr. H. Earle suggested, in reference to a case which had fallen under his care in the year 1811, that the paraplegia depended possibly "on some morbid affection of the brain or its membranes, which rendered it incapable of transmitting its influence to the extreme parts of the body." This statement may be seen in a communication made by him at page 10 of the eleventh volume of the 'Edinburgh Medical and Surgical Journal,' 1815, *à propos* of a review which had appeared in the same periodical upon Mr. Baynton's 'Essay on the Cure of Crooked Spine.' Later on, Dr. Baillie recognised this variety of paraplegia.[‡] This gentleman read a paper at the Royal College of Physicians, November, 1817, in which he observes:[*] "In adults, however, when there has been no accident affecting the spine by outward violence, paraplegia, I believe, depends most commonly in a great measure upon a disease affecting the brain itself. This opinion I have entertained for several years, and some other medical men have likewise held the same opinion" (alluding to Sir H. Halford, Mr. Copeland, and Sir J. Earle). He does not deny the occasional occurrence of paraplegia from disease within the spinal cavity, but looks upon it as being very rare. He also thinks that the spinal marrow may be pressed upon by fluid thrown out "between the membranes of the brain," which may "fall into the cavity of the theca vertebralis." It is true that, at page 22, he remarks that he had not had much opportunity of becoming acquainted with the morbid appearances in paraplegia. At page 24, he says, "I think it not improbable that in those cases of paraplegia in which the brain is found to be diseased, the morbid affection extends to both hemispheres of the brain; but this point can only be ascertained by a great number of dissections." These opinions of Dr. Baillie were evidently opposed to those of many of his contemporaries; and it is not a little remarkable that one whose opportunities and whose powers of observation were so good, was led to adopt such views. Can it be accounted for by supposing that, in those days, the spinal cavity was even less opened after death than at the present time?

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[*] It is possible we may suppose that under certain circumstances the spinal cord may be actually *compressed* (so as to have its functions paralysed) by serous fluid effused within the spinal cavity, but we cannot help thinking such a contingency to be rare; the mere presence of fluid outside the cord (unless the entire spinal arachnoïd and the cerebral arachnoïd cavities be so full as to prevent the yielding which would be experienced by such fluid, to the collective movements of the cord and that produced by the pulsation of its vessels) would appear but little able to produce an effect sufficient to paralyse mobility and sensation. We do not find the prediction of Dr. Maty verified with regard to paraplegia from the pressure of effused fluid in the theca vertebralis, who thought it might eventually be possible so clearly to diagnose the symptoms arising from such pressure, that surgeons would trepan the spine for its removal.

[†] Concerning this difference between the symptoms of paraplegia of a reflex kind and that from other sources, it is of interest to refer as far back as Mr. Stanley's paper before adverted to, where, foreseeing the difficulty of the subject, he observes at the close (p. 279): "But in the present paper I have simply intended to illustrate by a variety of facts the reciprocal connexion between the kidneys and the spinal cord, of the importance of which there can be no doubt, when we consider that *until by more exact observation our diagnosis is improved, cases of paralysis in the lower limbs may be submitted to us in which we may feel uncertain whether it is a disease of the spinal cord or of the kidney to which our treatment should be directed.*

[‡] See vol. i. of Wardrop's edition of his works, 1825, page 2, and also the Medical Transactions published by the College of Physicians, vol. vi. 1820.

Mr. Copeland, alluded to by Dr. Baillie, had referred in his 'Observations on the Symptoms and Treatment of the Diseased Spine,' 1815 (p. 10), to the "other cases of palsy of the lower limbs, the cause of which is connected with some derangement of the functions of the brain." Moreover, the reader curious on this subject, will find a lengthy comment on Paraplegia, "as dependent on some morbid alteration of structure in the encephalon," by Mr. H. Earle, in the thirteenth volume of the 'Transactions of the Royal Medical and Chirurgical Society' (1827), p. 516. In this paper Mr. Earle refers to the cases which he had noticed in the year 1815, as above cited. Dr. Abercrombie, however, as is seen from his work on 'Pathological and Practical Researches on Diseases of the Brain,' had evidently not assured himself with regard to this question of paraplegia, as a result of disease of the cerebrum. At p. 320 of his third edition, when speaking of the various classes of symptoms connected with organic cerebral disease, he observes, with regard to the paralytic ones, that one form may be that of paraplegia, in which case the cerebellum or tuber annulare is affected. Subsequently, however, he says, "It must be confessed, that the cases of this class, with paraplegia, are rather unsatisfactory from want of attention to the condition of the spinal cord;" and, furthermore, "perhaps it may be considered as a point not yet ascertained, whether paraplegia ever arises from disease confined to the brain."

Professor Alison, we find, associated paraplegia with cerebral disease in some cases;[†] and quite recently, Dr. Watson (1857), at p. 502 of the first volume of his 'Lectures on the Principles and Practice of Physic,' observes:

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[*] See Transactions, vol. vi. 1820, p. 17.

[†] See his Outlines of Pathology, 1844, p. 693.

"Paraplegia, that condition in which all the parts below a transverse line are paralysed, though it sometimes results from cerebral disease, is much more commonly the consequence of mischief in the spine."

Again, at p. 702, Dr. Watson says:

"In the other form, the paraplegia or the diminished sensibility seems to have an encephalic origin, and its apparent causes are such as operate upon the nervous system generally—mental and moral causes—influences which tend to lower the nervous energy. No definite or appreciable change presents itself in the spinal cord; but subarachnoïd effusions, collections of fluid in the cerebral ventricles, with general wasting perhaps, or general softness, or general induration of the nervous substance, are frequent attendants on this condition."

No less an authority than Romberg, recognises the very occasional existence of the paraplectic form of cerebral paralysis, especially in diseases of the medulla oblongata, cerebellum, pons Varolii, and in chronic hydrocephalus; and quotes from Gölis' work on the 'Diseases of Children,' in support of his statements.* Lastly, Dr. Bright, when speaking of symptoms produced by effusion of blood within the cranium, after remarking that he had never met with a decided instance of paraplegia from this cause, proceeds to say—"Cases occur where paralysis of the two lower extremities has appeared to depend on other diseases or injury in the brain; but of these we should be somewhat sceptical, from the obvious sources of error to which they are liable, amongst which the unobserved or the unsought diseases of the spinal cord and its membranes are the most to be suspected."

Most likely in many of the cases observed, from which the existence of cerebral paraplegia was inferred, organic changes in the cord would have been recognised, had the appliances which we now possess for minuter scrutiny of organic tissues been at the disposal of the observers. Possibly also this pathological view may have received force from the belief untenably entertained by several anatomists of repute, as by Saucerotte, Foville, Bouillaud, Pinel, Grandchamps, Serres, &c., that the corpus striatum, presided, as it were, over the movements of the lower extremities, the upper ones being dominated by the optic thalami.

It would be foreign to our present purpose to consider in this place the various arguments regarding paraplegia from cerebral causes which might with propriety be urged as well *for* as *against*.† We have only introduced the subject for the purpose of pointing out that it was not without reason that we expected Dr. Brown-Séquard to have taken notice of it, and to show that it is a question which has received such attention from others as to entitle it to a full and particular consideration.

Emphatic warnings as to the limits of space conceded to this article forbid our enlarging, as we might profitably do, upon what Dr. Brown-Séquard has written on the various subjects comprehended under the separate headings which we have named above; we are consequently obliged, in respect of them, to abridge what we should have had pleasure in describing *in extensô*, and for the remainder to refer the reader to the original. Among other points of interest he will perceive (p. 41) that our author looks upon the so-called cases of "idiopathic" paraplegia as essentially reflex in character, or dependent upon some real affection (congestion, serous effusion, &c.) of the spinal cord; although even after death it may be not possible to find out the cause of the paraplegia.

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* Loc. cit. pp. 931 and 947.

† Attention may be directed to a case recently recorded by Laborde, in which incomplete paraplegia, in addition to amaurosis and paralysis of the vagus nerve, was produced by cysts in the brain. See Sydenham Society's Year Book, 1860, p. 152, as cited from L'Union Médicale, 137, 1859.

He will also perceive that the forms of paraplegia sometimes found connected with rheumatism, gout, fevers, cholera, &c., are, according to our author, most frequently to be attributed to serous effusions within the spinal canal, or to venous congestion, or sometimes to reflex action.

As regards "prognosis" of reflex paraplegia, the author observes:

> "Of the two forms of reflex paraplegia, one of which depends upon an insufficiency of nutrition in the spinal cord, and the other upon an alteration of nutrition in the muscles of the lower limbs, the first is almost always curable when the external irritation which has caused it has ceased; while the second is very frequently incurable, whether its external cause has ceased or not. It is, therefore, most important for the prognosis, to ascertain if there has been or not a rapid atrophy of muscles in cases of reflex paraplegia."

The question of "treatment" of reflex paraplegia takes up thirteen pages, and, in addition to the obvious consideration of the urinary irritation, is itself specially brought forward under two headings, in reference to the paralysis. *Firstly*—with a view to the prevention or diminution of the transmission of any nervous influence from the diseased part to the spinal cord, such as paralysing the sensitive nerves by injecting narcotics (belladonna and opium) into the bladder, vagina, rectum, and by their internal use also. *Secondly*—with a view of increasing the quantity of blood in the spinal cord itself. This is to be sought in the use of revulsives (as recommended, it will be remembered, by Graves and others), cold, and galvanism, agents which seem to have this effect owing to their inducing excessive contraction of the blood-vessels of the cord in the first instance, and subsequently and, in consequence, their dilatation, in accordance with the ordinary rules of muscular action—relaxation being proportioned to the intensity of previous contraction. We also must enjoin frequent recumbency on the back (the limbs being held up so as to favour gravitation), and resort to remedies calculated to increase the amount of blood in the cord, and so to intensify its vital properties, as sulphur and strychnia, the latter of which appears to have this faculty as well by its power of increasing the quantity of blood in the spinal cord, as by its power of acting in a special and direct manner upon the structure of the cord, independently of the amount of blood therein contained.* The deficient nutrition of the cord must also be increased by suitable food; and the muscular atrophy and consequent diminution of irritability (effects of over-rest) must be counteracted by such remedies as galvanism, shampooing, &c. The application of heat and cold, sometimes alternately, to the skin, and of revulsives to the lower limbs, must be made, though their application to the region of the spine (as Graves suggested) is of much less utility.

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* Dr. Graves in the strongest terms recommends the use of strychnia continued "*until some sensible effect on the system is produced,*" when sulphur is to be resorted to, as well internally as in the form of baths. Graves, however, counsels recourse to stimulant liniments and blisters to the legs and thighs in the first instance and before the above remedies are tried; but he denounces their application to the back or loins as being highly annoying and quite inefficacious. The use of mercury in reflex paraplegia is spoken of by the same author as "decidedly injurious." We may here notice that Dr. Watson, in the earliest

Lecture III. is appropriated to the diagnosis and treatment of paraplegia arising from myelitis, meningitis, and simple congestion of the spinal cord and its membranes—a form of paralysis in all respects quite distinct, as has been pointed out, from the reflex variety. In this chapter, also, an interesting comparison (p. 61) is drawn between the symptoms caused by myelitis limited to a small extent of the dorsal region at its middle part, and those from myelitis of the lower parts of the cord; violent spasms in the paralysed limbs existing in the former variety, as in cases of fractured spine, tumours, &c.; and but little or none in the latter, except under certain circumstances. The salient points of difference between myelitis, meningitis, and spinal congestion are well shown, being classified (pp. 71–73) according to the variety of phenomena respectively presented by the three kinds of nervous conductors—motor (better called volitional), sensory, and vaso-motor—existing in the spinal cord. Thus, as regards the *motor or volitional conductors*, we find that, as paralysis and cramps result from their excitation, these symptoms are in myelitis more frequent and complete than in meningitis or spinal congestion, inasmuch as in this disease (myelitis) the number of motor conductors submitted to alteration, as well as the degree of excitation, is greater; the chief cause of paralysis in meningitis and congestion being pressure (which is of course very variable) of the spinal nerves in the narrow canals through which they pass out of the spinal cavity. The rigid spasm of the muscles of the "back" in meningitis seems to be due to reflex action, as in tetanus.

As respects conductors of *sensitive impressions*, it seems to be the rule, that it is in cases of myelitis that we have the frequent "reference" of various sensations so well known (formication, tingling, heat, &c.) to the skin and other parts of paralysed parts; whilst it is in (uncomplicated) meningitis and congestion that this referring of sensations is "almost null." This appears to be owing to the fact that in the latter case the conducters of sensation where they pass out of the spine are not at all, or very slightly, excitable from pressure. Then, as regards alteration of "*vaso-motor nerves*;" in myelitis the effects of excitation of these nerves are very striking, as shown by morbid changes in the urine, the formation of sloughs, rapid atrophy of muscles, serous infiltrations, &c.; and these effects, specially alkalinity of the urine, are more remarkable than in cases of meningitis or spinal congestion. Oftentimes in all three affections, in the neigh-

edition of his Lectures (vol. i. p. 529), had alluded, in speaking of the use of strychnia in paraplegia, to "some striking instances of recovery from paraplegia under the exhibition of this drug." He goes on to say, "No good can reasonably be expected from it, but much harm, unless the cord be free from organic disease."

bourhood of some part where the effects of EXCITATION of the vaso-motor nerves are observed, the results of PARALYSIS of these nerves are noticeable, such as dilatation of bloodvessels, increased heat, abundant perspiration, &c.

A few words only suffice for the "prognosis" of subacute or chronic myelitis in the dorsal or lumbar region. This variety, different from that of acute myelitis, is described (p. 74) as almost always capable of being arrested and sometimes cured, although perhaps in all cases some results of the paraplegia may remain, such as diminished capacity in directing the movements of the lower limbs.

The subject of "Treatment" of myelitis, meningitis, and spinal congestion occupies the remainder of this Lecture.

As regards the former disease, treatment is directed to consist of means tending to diminish the quantity of blood in the spinal cord. Recumbency should be prohibited, the limbs kept as hot as possible, the hot douche frequently applied to the spine, or the cold shower bath, followed instantaneously by friction, so as to produce dilatation of the vessels of the skin; dry cupping to the spine. If the myelitis arise from caries or other organic affection of the spine, then the cautery, moxas, &c., to the diseased spot will be in requisition. Touching internal remedies, on the same principle, those endowed with the property of diminishing congestion of the spinal cord are to be exhibited, especially belladonna and ergot of rye—remedies which (dilating the pupils) strongly excite unstriped muscular fibres, and diminish the reflex power of the spinal marrow, by lessening the contained amount of blood.* If, after six or eight weeks of this treatment no good follows, then the iodide of potassium is to be added; and if the coëxistence of meningitis also be suspected from the outset of the disease, this drug must be given from the first. For the purpose of alleviating pain and general soothing, those remedies which, like opium, induce spinal congestion, are to be avoided; conium, Indian hemp, hyoscyamus, especially the latter, being, on the contrary, freely used. Of course the morbid alterations in the kidney, bladder, &c., demand individual and particular consideration; and constipation, with its too frequently attendant congestion of the vessels of the spinal cord, must be sedulously combated; nutritious diet and moderate exercise not being neglected.

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* Respecting the use of ergot of rye, which of late years has obtained some celebrity in certain cases of disease or derangement of the functions of the spinal cord, it is of interest to notice that the earliest systematic notice of its use in such cases with which we are conversant, is the reference to it in a communication by Dr. Payan, of Aix (Bouches du Rhone), to the Revue Médicale, 1839, tome i. p. 357, in which he suggests that the value of its therapeutical use as a quickener of uterine action, as also in amenorrhœa, passive uterine hæmorrhage, paralysis of the bladder, &c., is due to its primary action upon the spinal cord, from which nerves of sensibility and motion addressed to these organs affected by the ergot, are derived; and quotes four cases of paraplegia, in which very remarkable, and in some cases very rapid cures or benefit was produced by its administration in the form of infusion. We cannot here go into the consideration of these cases, which are titled as follows:—1. Chronic Paraplegia without Organic Lesion of the Spinal Cord: use of ergot of rye; cure. 2. Paraplegia consecutive to a Commotion of the Spinal Cord, uselessly treated in different ways; cured by ergot of rye. 3. Enfeeblement of the Lower Extremities, following Potts' Spinal Disease: ergot of rye; perceptible amelioration. In these cases the remedy was administered, however, under the supposition that no inflammatory action about the spinal cord existed, its disturbance of function being attributed to a "want of action," "manque d'action sans lesions organiques," an "inertie de la moelle," &c., this remedy being supposed only useful in cases requiring stimulation of the organs enervated by the spinal cord, cases in which the cord requires awakening from its stupor. Dr. Payan's explanation of the utility of this remedy will be seen to differ essentially from that given by Brown-Séquard.

Of the prognosis and treatment of meningitis and spinal congestion much the same may be said as of myelitis. Meningitis is, however, more open to cure than the latter disease, and blisters and iodide of potassium are particularly serviceable in its treatment. If much effusion into the spinal canal exist, diuretics are necessary.* Spinal congestion, if of very long standing, is very difficult of cure.

The treatment of the sloughs, ulcerations, &c., so frequently accompanying paraplegia from myelitis, and the result of altered nutrition from irritation of vaso-motor nerves, has been already alluded to. (See p. 384.)

Lecture the "Fourth" and last (p. 84) is concerned with the symptoms and treatment of various other forms of paraplegia—such as those arising from white non-inflammatory softening of the spinal cord, hæmorrhage, tumours, &c.; with a review of the symptoms of paraplegia in general, and with "conclusions."

In paraplegia from *white softening*, the result commonly of interrupted supply of blood from alterations in the bloodvessels, &c., we have marked absence of pain in the spine or limbs, or of cramps or spasms in the legs. At first there is great weakness, especially at the knee and ankle, tottering, &c.; then the paralysis becomes much more marked, and sensibility throughout the legs and pain over the sphincters much diminished; the urine being but rarely changed. *The temperature of the legs is generally much increased.*

The paraplegia from *hæmorrhage* may be distinguished by its suddenness, by pain at the affected part of the spine, both as well without as with pressure upon the spot, and by pain in those parts of the body supplied by nerves from the region of the cord which is implicated. The sphincters are almost always quite paralysed from the very first. Not rarely myelitis occurring around the hæmorrhage implicates the symptoms; and, of course, if the hæmorrhage be confined to the lateral half of the grey matter, we have loss of motion in the side of the body corresponding to the hæmorrhage, and loss of sensibility on the *opposite* side. Hæmorrhage into the *grey matter* is from the first attended by diminution of the sensibility. If the hæmorrhage is in the vertebral canal, *outside* the spinal cord, we have more extended pain, along with more anæsthesia, and frequently also tetanic convulsions, which do not exist when the tissue of the cord itself is only affected. Of course hæmorrhage into the cord often coëxists with softening.

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* With regard to the value of the cantharides in paraplegia, the words of one of our most practical English physicians may here be aptly and beneficially quoted. In his work on the Nature and Treatment of Dropsy, &c., 1837 (p. 44), Dr. Seymour says: "I have succeeded beyond my hopes in restoring the use of the limbs in early life in this dreadful disease. It has been long known (the cantharides) to be useful in such cases, and in pressing it upon the consideration of the profession, I am advocating no very novel practice, though the successful cases are scattered through different works. In all the cases in which I have seen its administration effectual, it has acted as a powerful diuretic, and if Dr. Baillie's opinion be correct as to one of the causes of these diseases ('if there be any effusion of serum between the membranes of the brain, which is a very common occurrence, a portion of the serum may fall into the cavity of the theca vertebralis, and press upon the lower part of the spinal marrow'), this diuresis will sufficiently explain its utility in the palsy of the lower extremities."

As respects the prognosis of paraplegia from hæmorrhage and from softening, that from the former cause is much more grave than that from softening, as there is a great probability of its recurrence, and also of great alterations caused by effused blood. Cure of paraplegia from either cause is not known by the author; in some cases of white softening amelioration, however, takes place, and *arrest* in many cases may be anticipated.

In paraplegia due to tumours of various kinds, we have varied spinal symptoms, according to the locality of the tumour, and also symptoms of disease of other organs, in consequence of irritation of nerves originating at the part where the tumour exists. If no myelitis or meningitis be set up, then the symptoms are very much those of non-inflammatory softening; excepting that we have pain in the spine produced in addition to the effects of irritation or destruction of individual nerves or sets of nerves; and the ascertained functions of the grey part or the white posterior or lateral parts of the cord will, of course, explain numerous symptoms, according as these respectively are the seat of the tumour. The well-known symptoms of diminished power of guiding the movements of the legs, *unless the patient can see them*, appear to be dependent upon an alteration of some of the posterior roots of nerves and of the posterior white and grey portions of the spinal cord; and the attendant partial anæsthesia of the skin and muscles of the feet and legs is specially spoken of as a symptom of some tumour pressing upon the posterior surface of the lower part of the cord. The persistence of the reflex power of that part of the spinal cord which is situated *below* many tumours is indicated as a means of diagnosis between paraplegia from this cause (a tumour), and that due to the effects of pressure dependent on effused material in localized meningitis, in which cases the reflex faculty of the dorso-lumbar part of the cord is diminished.

The frequency of spasm of the LIMBS in a case of tumour of the *lumbar* part of the cord, whilst spasm of the muscles of the BACK, along with fever, is most frequent in lumbar meningitis, is to be noted; as also the difficulty of diagnosing between tumours of the lumbar part of the spinal cord, and cases of meningitis limited to the lumbar part of the meninges, and again between paraplegia from myelitis in the upper part of the dorsal region, and that from a tumour which by irritation has produced inflammation.

In this lecture, also, we have a consideration of the significancy of various symptoms belonging to or co-existing with paraplegia, and a kind of summary offered of the various rules of treatment in separate cases, having special reference to the use of belladonna, ergot, strychnia, sulphur, phosphorus, mercury, iodide of potassium, cantharides, the various sedatives, tonics, and revulsives. We find our author speaking from his own experience of phosphorus as being of questionable service, and very guarded against the resort to mercury, which has been so much abused in the treatment of paraplegia; but of the iodide of potassium he speaks in eulogistic terms, as it appears to excel even mercury in power of absorption, and is free from the depressing influence of that remedy. Of this agent he observes, "it is the only known remedy that may be employed without danger in the various forms of paraplegia." (p. 114.) The cantharides appears useful only in cases of chronic myelitis.

This lecture and the volume are brought to a termination with the following axiomatic conclusions:

> "1st. That there is a form of paralysis of the lower limbs entirely distinct from all others, as proved by its mode of production, by morbid anatomy, by its symptoms, and by the influence of a certain mode of treatment, and that this form of paralysis fully deserves the name of reflex paraplegia.
>
> "2nd. That the reflex paraplegia may be caused by the most varied irritations of the skin, the mucous and serous membranes, the abdominal or thoracic viscera, as well as of the genital organs or the trunks of the spinal nerves.
>
> "3rd. That most cases of paraplegia can be placed in two groups, entirely different one from the other, according to the presence or absence of symptoms of irritation of the motor, sensitive, and vaso-motor nerve-fibres.
>
> "4th. That most of the therapeutical means to be employed in paraplegia are also to be grouped in categories, one of which is fitted to those cases in which there are symptoms of irritation, and the other to these cases in which these symptoms do not exist." (p. 117.)

On reviewing the labours contained in the disquisition of which we have above endeavoured to give a concise *résumé* (in which, however, we have abstained from giving any general history of paraplegia, or presenting our readers with anything like an essay on the subject), we cannot fail to notice especially that the subject of paraplegia has been handled in a most comprehensive manner, its varied circumstances being considered in their fullest and widest bearings, and many attendant difficulties as to diagnosis and modes of treatment cleared up. Previous observations had practically established, as we have seen, the possibility of a reflex form of paraplegia; and Mr. Stanley's anatomical explanation, as before alluded to (p. 386), resting upon the known connexion between the spinal marrow and the sympathetic nerves distributed to various viscera, was, as far as it went, good and true. The further inquiry presents itself, to what extent is the explanation thrown out by Dr. Brown-Séquard regarding the supposed condition of the capillaries of the spinal cord, to be ratified as a just and admissible one? The subject must be, even in Dr. Brown-Séquard's mind, far from being absolutely a settled one, more cases and observations being required to render any view irresistible; also further experiments and further microscopical investigations of the spinal cord, as Romberg has suggested.* Moreover, this latter writer has very properly counselled the use of electricity applied to the muscles in determining the character of supposed instances of reflex paraplegia; and respecting assumed cases of this affection placed on record, alluding specially to those described by French writers, he seems to think that many which are termed paraplegic are not such in reality, and suspects the, what he terms vague, description of a "*weakness*" of the limbs given in the relation of many cases.* We are ourselves willing to concede that the description of some of these writers hardly amounts to what would be designated paralysis in certain instances, and that some of them are described without being sufficiently criticised (*ohne entsprechende Kritik*); but these doubtful cases do not suffice to destroy the evidence afforded by others of a more positive nature. Again, we would ask how the various degrees of weakness, when extreme, are, as mere symptoms, and especially when aggravated, to be distinguished from various grades of slight paralysis? In either case all that is meant is that the patient finds himself unable, by an act of volition (and this not occasionally only), to move the muscle or sets of muscles in question at all, or with more or less imperfection. Weakness of the muscle passes insensibly into complete or incomplete inability. As we before said, Dr. Brown-Séquard's is but a conjectural explanation; but there appear to be no reasonable grounds for antagonism or exception to it. Hitherto, even by those who recognised its existence, no interpretation of this so-called reflex paraplegia has appeared beyond the bare acknowledgment of the anatomical connexion existing between certain organs primarily diseased or deranged, and the central spinal cord whose functions are in some way or other perverted. The one suggested by Dr. Brown-Séquard is at any rate not an unlikely one; it is perfectly intelligible, and to a certain degree consorts well with our present views of nerve-pathology, having analogy for its support. We do not see how we can consider ourselves entitled to anticipate any actual demonstration carrying us to the very truth of the matter, and perhaps we shall have to be content to say, as on so many occasions, "Sufficit si quid fiat intelligamus, etiamsi quomodo quodque fiat ignoramus." More practical points for inquiry, we venture to think, are the following—viz., to what extent has the diagnosis between paraplegia from some primary affection of the cord and that from disease, derangement, or irritation, &c., in distant parts—i.e., of "excentric" origin—been substantiated? and to what degree can we, in a rational manner, depend upon one or other form of treatment being alone applicable to either form of paraplegia? Our own experience quite coincides with that of our author in leading us to acknowledge a reflex variety of paraplegia, produced in some or other way; and, for the most part, we think that the discriminative line of treatment recommended by him, provided that the form of paraplegia was very manifest, is the correct one. We are of opinion, however, that he has coloured his picture a little too vividly, that his outlines are, on the whole (perhaps inevitably) too lucid and distinct, and that he has not brought forward with sufficient prominence and force the difficulties which very often practically, and at the bedside, exist in defining the exact cause of the paraplegia; or, in other words, in establishing to which category a given case pertains. Being aware of the extent to which the proper adaptation of remedies in the various forms of paralysis depends upon a just diagnosis, and how pernicious treatment may prove in the absence of such diagnosis, one is not a little tantalized by the contrast between the clearness and precision, on the one hand, with which the true line of treatment is mapped out, and on the other, the consciousness that cases in actual practice of reflex paralysis are very numerous in which, (as in reflex epilepsy) the "fons et origo mali," the exact and vulnerable source of the outside irritation, fails to declare itself openly. Certainly, with regard to the majority of cases of epilepsy, we know no point in the whole range of pathology more difficult than the determination of the source of the morbid irritation. As Dr. Brown-Séquard observes (p. 8), speaking of the characteristics of reflex paralysis, "An outside irritation, starting from some sensitive nerve, exists before the reflex paralysis appears." The enigma, both as regards epilepsy and reflex paralysis, is, in too many instances, to discover the spot from which it does start, as the expectation of cure depends on this discovery and on the removal of the irritation, although in very many cases, even when the cause is removed, the effect does not entirely or quickly cease. In this respect we experience a want in the author's work of more guidance, owing to the absence of specific individual cases, the relation of whose circumstances should illustrate, enforce, and render more intelligible the general propositions adduced. We naturally have our misgivings as to the direct connexion supposed to subsist between certain forms of paralysis on the one hand, and the many lesions of viscera, pleurisy, pneumonia, &c., on the other, which are described as occurring in the paraplegic patient, so difficult is it to demonstrate the "*punctum*" or source from which real or supposed morbid impressions (especially in inward parts) are conveyed along the nervous channels centripetally.

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* Loc. cit. p. 913–14, edition 1857.

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* Romberg further alludes to the microscopical investigations of Türck on the transformations into granular and fat corpuscles, which are found in the spinal cord. He also takes exception later on to those cases of paralysis supposed to be due to diseases of the intestines.

Touching the question of treatment, the author attempts, as we have described, to show why it is that one set of remedies best suits the reflex form, and why others alone are useful in other forms of paraplegia, basing their various uses, which, indeed, are empirical (in the proper sense of the word), upon their supposed action on the capillaries of the spinal cord, dilating or contracting them, as the case may be, and thus increasing or diminishing the supply of blood to the cord. This explanation, of course, has exactly the same cogency and weight as that given of the causation of the paraplegia itself, and it would appear that our author was led to make the inferences which he did respecting the connexion between reflex paraplegia and a contracted state of the spinal capillaries, by noticing the action upon these vessels of those remedies which proved successful in given cases. This proposition suggests a question which we should like to have answered; for, remembering that reflex paraplegia is thought essentially to depend upon a "*contraction*" of spinal capillaries, and that the useful action of certain remedies assumed to produce contraction of spinal capillaries is attributed to the supposed fact that the excessive contraction of these vessels is followed, *ex necessitatê*, by their dilatation, one is unavoidably driven to inquire, how it is that the original contraction of these vessels, the conjectural *sine quâ non* of this disease, does not, by the same law, lead spontaneously to their reactionary dilatation, and thus subsequently to a spontaneous cure of the disease?

There is one reflection which naturally arises in the mind, on considering the value of diagnostic differences between various kinds of paraplegia, and it is one which is not, we believe, adverted to by our author. It is this—viz., to what extent may certain organic changes found in the spinal cord after death, in cases of paraplegia, be the result of abnormal molecular or other conditions of the cord, ORIGINALLY BROUGHT ABOUT IN THE REFLEX VARIETY OF PARAPLEGIA, and therefore the result of morbid excentric impressions conveyed to the nervous centre by afferent nerves? This is manifestly a problem of no little significance, and one which, as far as we know, yet awaits its solution.

We have ample data, it would appear, for supposing that frequent aberrations from the ordinary and healthy condition of the cerebral capillaries, such as not uncommonly exist in epilepsy of ex-centric origin, may in the course of time and in particular instances lead to organic cerebral changes. Why, then, may not such sequences occur with regard to the spinal cord, however the changes in the state of the capillaries may have been, in the first place, conditioned? Indeed, with regard to the spinal cord, a case related by Portal* is much to the purpose. It is one in which spasmodic action of the muscles of the "*left*" leg was for a length of time wont to take place shortly before the catamenial periods, abating when the attendant flux had ceased. Subsequently, the left leg became paralysed, and then spasmodic action of the left arm came on; the patient died of coma, and after death the spinal cord was found to be red and softened, but this structural alteration was, curiously enough, on the "*right*" side. It is an interesting speculation, whether or not in this case the spasm was of a reflex character originally, and the subsequent paralysis due to interposing histological alterations in the cord, which in the first instance were the result of functional changes, which functional ones had followed the conduction of morbid impressions along afferent nerve-fibres. We think we are justified at this juncture, especially in calling attention to the contingency which we have mentioned, because the point under consideration is that of a "*contracted*" state of spinal capillary vessels; and it is a contracted state of bloodvessels (diminished calibre—i.e., and therefore diminished blood-supply) which, when permanent, leads to softening and other intimate alterations of delicate nervous tissue.

If we are able to recognise the possibility of such an occurrence, it is very easy to conceive that post-mortem examination may, in individual cases, *be far from helping us to the diagnosis between the reflex and the other forms of paraplegia.*

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* Cours d'Anatomie Médicale, 1803, tome iv. p. 116.

Before bringing our observations to a close, we cannot help referring to a lecture by Mr. Wells, on Paraplegia, which appeared in the 'Medical Times and Gazette' for November 14th, 1857. In it we find, besides a good recount of the then existing state of our knowledge on the matter, several interesting practical remarks, enforcing attention in a pointed way to the reflex form of the disease. This variety, as it occurs in connexion with disease of the urinary organs, is described "*ad naturam*," both as regards its ætiology, mode of approach, and regular course; it appears to us, however, that in certain respects some confusion exists in this description touching the diagnostic symptoms of the reflex form of paraplegia and that from myelitis. In the matter of treatment Mr. Wells, in one respect, differs very decidedly both from Graves and Brown-Séquard, and that is as regards the exhibition of strychnia as a remedy.

After enlarging on the mode of treatment, general and local, which is desirable in reflex paraplegia from urinary disease, he remarks :—

> "As to strychnia, I have seen it tried in small doses, and I have seen it carried on till its characteristic effects have been produced; I have seen it do harm, but I have never seen it do any good whatever. Galvanism or electro-galvanism, on the contrary, I feel quite certain, is of very great utility; but it must be used perseveringly."

From the results of our own practice we can by no means endorse this anathema as respects the nux vomica, provided that the proper cases for its administration have been selected; and we have no doubt that in paraplegia, as in many other diseases, owing to deficient care, or impossibility by reason of complications, in duly diagnosing particular cases, good and efficient remedies have acquired, like the once naughty dog of renown, that "bad name" which undeservedly, but infallibly, sticks to it for ever after.*

In considering the literary character and qualifications of the works placed at the head of this Article—and we speak chiefly of that devoted to the subject of paraplegia—we observed undoubted indication of hastiness in composition and, in consequence, some occasional obscurity in meaning — as, for example, at p. 59, where a certain amount of intricacy attends the delineation of the diagnostic difference between the forms of local myelitis. Again, in places, we experienced difficulties owing to the absence of strict separation between different subjects treated of, sections wanting here and there their appropriate headings—as at p. 109, where that of treatment is not separated from that of symptoms; or at p. 42, where that of diagnosis runs into that of prognosis. We are also of opinion that more detailed and didactic guidance as to the doses of remedies required would for the purposes of many readers much enhance the value of the work.

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* Here we must also allude to the communication read at Birmingham by Dr. W. Hinds (mentioned before at page 387), in which two cases of paraplegia, supposedly from disease of the kidney, are related. In Case I. this affection was supposed to be owing to a scrofulous state of the kidney, but unfortunately the spinal cord was apparently not examined. In Case II., the patient was suffering from hæmaturia and pain in the left loin, the left leg being more affected than the other one, and was still under treatment. Dr. Hinds has recently informed us that regarding this second case, though the health has shown a slight improvement, the paralysis has slightly increased, and both lower extremities pretty equally implicated. Towards the end of the year 1860, the patient passed blood from the bladder, a quarter of an hour before making water. The lumbar pain has not the severity it formerly had. He complains much of numbness of both hips, legs, and thighs.

These imperfections, easily to be removed in a future edition, do not, however, in any manner, detract from the abstract value of the works, and we think that the profession is much indebted to Dr. Brown-Séquard for the able and complete manner in which, by the juxtaposition and comparison of pathological cases, and of the results of experimental research, he has illustrated the general subject of paraplegia, and especially the more limited one of reflex paraplegia.

We look forward with interest to the completion of those Lectures on the Diseases of the Nervous System which are now in the course of publication by the author, and with which we hope to make our readers familiar at a future time.

REVIEW VII.

Mémoires de l'Académie Impériale de Médecine. Tome xxiv., pp. cix. et 654. 4to.—*Paris*, 1860.

I. PASSING over M. Dubois' eulogium on Geoffroy St. Hilaire and M. Guérard's 'Report on the Medical Employment of the Mineral Waters in France during 1857,' we come to M. Trousseau's 'Report on the Epidemics observed in France during 1858.' The complaints of the neglect or inefficiency of the official reporters, which we noticed in our last review of the 'Transactions of the Academy,' are reiterated again by M. Trousseau. Fortunately, there seems to have also been less to report upon during 1858; even typhoïd fever, that standing scourge of the French provinces, having proved far less prevalent than usual. Still, one affection took on a great extension, and alone caused more deaths than all the other epidemic diseases of the year combined. *Diphtheria*, which had begun to manifest itself in 1857, spread rapidly and extensively in 1858, so that few departments were exempted. Unfortunately, the statistics which have been obtained cannot be regarded as exactly representing the mortality, and still less the numbers, of those attacked. Such as they are, those received from thirty-one departments represent 1568 adults and 7474 children (i.e., under sixteen years of age) as having been attacked by the disease, 165 of the former and 3384 of the latter dying. M. Trousseau thus describes some of the peculiarities in the mode of invasion of this epidemic:

> "The invasion of diphtheria has not taken place simultaneously throughout France. Already in 1857 several departments were invaded, the disease appearing in a certain number of localities along the sea coast, from Boulogne to Havre, and beyond. At the same time it appeared in England, where the extent of its propagation has excited extreme solicitude. Frequent communications between the places affected would seem to explain the almost regular progress of the disease, taking Boulogne as a starting point; but an examination of the facts is far from confirming this supposition. Not only other

centres existed simultaneously, but these were situated in the most distant provinces of the Pyrenean chain; and even with respect to the littoral departments, it will be found, by inspection of the dates, that the invasion did not take place through places in proximity. The same was observed in 1858. Diphtheria did not traverse France more or less rapidly from one point to the other; but it prevailed at the same time in countries situated at great distances from each other, not having followed the progressive course analogous to that of which the first invasion of cholera, for example, furnished so striking an example. When epidemics explode, in the absence of a single impulsion a greater or less number of partial centres may become established, whence the disease radiates to the environs. This is the case with some of the epidemics of typhoïd fever. The disease is found, spontaneously developed or imported from a distance, fixing itself in a centre of population, whence, after having been developed for some time, it extends to the neighbouring communes, sparing some, and implanting itself in others, but always the product of an importation which may be traced. Diphtheria does not obey this law. One, two, or three centres of population in a country are attacked with various intensity, without its being possible to seize the origin of the contagion or to trace any relation between the affected localities.

"If in place of studying its propagation at great distances, we content ourselves with following the mode of communication of the disease in each local centre, we are met with no less difficulties. Sometimes the family in which the first case is observed, or, so to speak, the 'parent-house' of the epidemic, is almost entirely subjected to various degrees of the diphtheritic intoxication; while, at others, a single child alone is affected, the rest of the inhabitants of the house remaining entirely exempt. Sometimes we may follow the succession of the early cases in neighbouring houses, but in general even small villages do not furnish such a series. Isolated farms, having no communication with the affected hamlets, are attacked, while the cases occur in the village at hazard, or at all events without any ascertainable affiliation. Must we, then, renounce the investigation of the mode of propagation of this disease, and confine ourselves to the registration of contradictory facts? Not so. Even presumptions, providing they be advanced with extreme reserve, may serve as a starting-point for ulterior researches.

"The first question is this: Is diphtheria contagious? We do not hesitate to reply, yes. It seems to us incontestable, and the epidemic of 1858 would furnish numerous examples, that the contact of a healthy individual with a diphtheritic patient is one of the causes of the development of the disease. For transmission to take place, cohabitation is often necessary. The more carefully cases have been observed, the more they have confirmed the existence of centres of contagion. If it be true that the disease once produced has an indisputable tendency to extend in the house in which it has appeared, its mode of importation is far from offering an analogy. In some cases it is known for certain by whom the disease has been imported; but, in general, ignorance prevails with respect to the antecedents, or it is known that the child first attacked has never left the village, and has been placed in no possible relation with any one suffering from it." (pp. xxxvi-xl.)

In other circumstances of its history, diphtheria is no less irregular. We are unable to fix its mean duration, nor does it exhibit during its invasion the periods of increase and decline like other epidemics. It is thus evident that its period of incubation cannot be determined. The disease is as insidious in its progress amidst a population, as it is in its course in individual cases. A first case may or may not be immediately followed by others, and then a fortnight or more may elapse before the next invasion—the epidemic proceeding on a jerking course, disappearing and returning at indeterminate intervals. After speaking of the numerous cases of eruptive affection of the pharynx observed during this epidemic, M. Trousseau goes on to describe the varieties of diphtheria itself:

"It is well known that of late years diphtheria, departing from the course which it had formerly followed, has frequently proved fatal without determining asphyxia properly so called. Hence its division, pretty generally admitted, into two forms: the one, *croupal*, properly so called, invading the larynx by preference, and intercepting the respiratory passages; the other, *toxical*, having no special laryngeal localization, and characterized (besides the enormous glandular tumefaction) by general symptoms of poisoning. It is a matter of interest to ascertain the extent to which the epidemic of 1858 corresponded to one or other of these types. Although the documents are defective with respect to comparative diagnosis, we may state that the diphtheria did not assume an uniform character, and that cases of laryngeal diphtheria and others of toxical diphtheria were met with in the same localities. It is possible that in some places one of these forms predominated, while it was more rare at other points; but nowhere was the one form observed to the exclusion of the others.

"In the analysis of the Reports we have found some interesting examples of the primary generalization of diphtheria. Thus, at the Rochefort Hospital, the employment of blisters had to be discontinued because the denuded surface of the skin became covered with false membranes, even in individuals exempt from diphtheritic affections (examples from other localities are also given by M. Trousseau). In the cases of some children, nearly all the mucous membranes have been invaded but secondarily. Even in the well-defined croupal form, diphtheria often becomes cutaneous, and extension to the skin is not of itself alone an indication of the generalization of the affection. Paralysis of the velum, with its consequences, appears to have been a very frequent occurrence during convalescence. Many practitioners report that most of the children cured, besides the *nasonnement*, and immense difficulty in deglutition, suffered later from generalized paralyses. . . . . With respect to treatment, no method was resorted to during this epidemic which was not already known, and for which and against which numerous facts have been recorded." (pp. xlv–xlvii.)

II. *On the Mental State in Chorea.* By M. Marcé.—M. Marcé's memoir, illustrated by 18 cases, is terminated with the following conclusions:

"1. Disturbance of the moral and intellectual faculties is a very common occurrence in chorea. In a given number of patients, at least two-thirds exhibit more or less well marked traces of this. The immunity of the remaining third is not explicable by the age or sex of the patients, by the acute or chronic condition of the disease, or by the intensity or extent of the convulsive movements. 2. Four morbid elements, sometimes isolated, but generally associated, require our notice in the mental condition of the subjects of chorea. (1.) The disturbance of the moral sensibility, manifested in some notable change in disposition, which becomes singular and irritable, and gives rise to an unaccustomed tendency to gaiety, or especially to sadness. (2.) The disturbance of the intellect, as characterized by diminution of the power of memory, a great mobility of ideas, and an impossibility of fixing the attention. (3.) Hallucinations which occur during the state between waking and sleep, more rarely on waking in the morning, and sometimes during dreams. Frequently limited to the sense of sight, they extend in rare cases to the general sensibility, and even to the sense of hearing. They may be met with in simple, uncomplicated chorea, but they are of much more frequent occurrence when the chorea is associated with hysteria. Although in the great majority of cases these hallucinations are not an alarming symptom, in certain exceptional ones they may lead to excitement and delirium. (4.) The maniacal delirium which sometimes complicates chorea from the commencement or during its course. This is a dangerous condition, which in more than half of its subjects leads to death, amidst formidable nervous accidents, and even in the more fortunate cases frequently leaves after it various forms of intellectual disturbance. Inhalation of chloroform, prolonged baths, and antispasmodics, are the means which have been found of most service in the treatment of this affection, which everything seems to prove is, in the great majority of cases, a purely nervous delirium." (p. 19.)

III. *Clinical Researches in Auscultation of the Head.* By Dr. Roger.—The declaration of the value of auscultation as a means of detecting disease of the head in children, made some years since by two American physicians (Drs. Fisher and Whitney, in the 'American Journal of Medical Science' for 1838 and 1843), has not made much impression in Europe; and Dr. Roger, not altogether satisfied with the amount of examination the subject has received, resolved to institute an investigation upon a sufficiently large scale to confer weight upon any conclusions that might be drawn from it. To this end the heads of about three hundred children were examined, those subjects being first selected who were supposed to be labouring under cerebral disease or rachitis, and then others as they offered themselves indiscriminately (having first established the existence of a cephalic *souffle* in children apparently healthy) at the Children's Hospital or in private practice. It so happened that Dr. Hennig was pursuing a similar inquiry, upon a smaller scale, at Leipzig, at about the same time, and Dr. Roger compares the results of these two investigations with the prior researches of Drs. Fisher and Whitney.

Auscultation of the head in health.—For auscultation of the head, Dr. Roger agrees with Dr. Hennig that the flexible stethoscope is the preferable instrument, and in order to hear the cephalic *souffle* with as much distinctness as possible, this must be applied over the region of the anterior fontanelle. Other sounds are also to be heard and distinguished. Thus, the circulation of air in the nasal fossæ gives rise to a cephalic respiratory sound, which in some cases of dyspnœa becomes so rapid as to be liable to be confounded with the proper *souffle*. When the child speaks or cries, the voice gives rise to a remarkable and piercing cephalic resonance. Deglutition and suction give rise to other sounds, and a well exercised ear may sometimes listen to the double pulsation of the heart, especially when palpitation exists. As to the cephalic *souffle* itself, while the American writers maintain that it is always an abnormal sound, Dr. Hennig declares that it is purely a normal sound, which even disappears upon the supervention of disease. Both these views M. Roger's investigations lead him to regard as too exclusive; for he found that a normal *souffle* may exist in exceptional cases, but that such normal *souffle* is by no means the rule. It is neither an exclusive phenomenon of disease nor a habitual phenomenon of health. He regards it as rather an unfavourable sign than otherwise, and one which, when met with in children apparently well, leads to the fear of some latent or imminent disease.

Auscultation of the head in disease.—According to Drs. Fisher and Whitney, cerebral auscultation may be employed at any period of life, from infancy to old age: but Dr. Hennig declares that the *souffle* is not audible after the third or fourth year; and M. Roger states—able stethoscopist as he is well known to be—that he has never been able to hear this souffle except in early infancy, prior to the closure of the fontanelle. The other sounds induced by respiration or the propagation of the sounds of the heart, are even more distinctly heard at a more advanced than at an early period of ossification; but the sole abnormal one, the *souffle*, is imperceptible after the closure of the fontanelle. All that can be really said of cerebral auscultation is, then, that it may reveal a bellows-sound in certain pathological conditions of young infants. M. Roger details at considerable extent the various researches he has made in the different diseases of early childhood, with this result:

> "Is there, then, any cerebral affection which may be recognised with tolerable certainty by means of the *souffle?* None. Absent in the vast majority of cases, absent in meningitis, in convulsions, &c., this abnormal sound has only been heard in some infants the subjects of chronic hydrocephalus, but not with sufficient constancy to be considered as a sign of effusion. Neither from its presence or its absence can we positively conclude as to the existence of any cerebral disease whatever.
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> "On the other hand, cerebral auscultation renders unexpected service in the diagnosis of diseases dependent upon alterations of the blood; since, as we have shown by numerous examples, we are enabled by the presence of this *souffle* to conclude as to the existence of an anæmic and ricketty condition. Auscultation of the head has indeed in such patients a great advantage over auscultation of the vessels of the neck, by reason of the greater facility of its application in these young subjects. . . . . .
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> "I should much regret the trouble I have taken in collecting these numerous cases, and in examining into and invalidating the results announced by preceding observers, if I did not remember that it is often more difficult, and sometimes as useful, to correct an error as to demonstrate a truth. I may add, that if my inquiries on this point of stethoscopic science have terminated in an almost complete negation, I have during this pursuit of truth met with some facts on my way which I believe to be new and of practical interest. They are as follow:—1. The existence of a cephalic *souffle* in the chloro-anæmia of very young subjects. In this it is of very frequent occurrence, while it is quite exceptional in affections of the brain. 2. The nature of this *souffle*, which is always a sound connected with an alteration of the blood, an inorganic, not an organic sound. 3. The frequency of chloro-anæmia during the first year of life, and at the epoch of dentition. 4. The frequency of anæmia in pertussis. 5. The possibility of the early recognition of the changed condition of the blood by means of auscultation of the cranium. This enables it to be promptly met, which is of importance in very early life, when every cause of enfeeblement of the economy may lead to (especially when there is predisposition) general tuberculosis. 6. The frequency, if not constancy, of the cephalic *souffle*, as well as carotidean sounds, in rickets. 7. The demon-

stration by this *souffle* and its characters of the nature of rickets, which should not be regarded as a disease localized in the osseous system, but as a change in the condition of the blood, as a disease implicating the entire economy. 8. The demonstration by means of exact figures of the epoch when the fontanelles commence closing (at ten months in a fourth of the subjects), and of that at which such closure should be complete (between two and three years in almost all the cases). This fact is not without its pathological and juridical importance. On the one hand, a delay in the closure indicates a retardation of the general ossification, and announces the imminence of rickets, or the commencement of hydrocephalus; and, on the other hand, a precocious closure of the sutures and fontanelles indicates the possibility of a microcephalon with consecutive idiocy. The determination of the condition of the fontanelles at a given period of early infancy may aid the medical legist in approximatively determining the age of a young subject, or in resolving a question of identity." (pp. 03-96.)

IV. *The Pathological Anatomy of Internal Strangulations, and the practical Consequences thence deducible.* By M. DUCHAUSSOY.—This Prize Memoir, occupying between two and three hundred pages of the volume, is far too elaborate and exhaustive to admit of analysis in the space we have at command. Being, indeed, a repertory of almost all the known facts upon the subject on record, it must be referred to by all future inquirers, and to their notice we commend it. The memoir is founded upon more than 700 recorded cases, in 518 of which the results of autopsies are stated; and these are classified with all the elaboration characteristic of the French writers. So large a proportion of the cases referred to are due to British authorities that the author is led to believe that internal strangulation is oftener met with amongst ourselves than amongst the inhabitants of the Continent, and this he attributes to the differences which prevail in the alimentary regimen.

V. *On the Retraction, Cicatrization, and Inflammation of the Umbilical Vessels, and on the Ligamentous System which succeeds.* By CHARLES ROBIN.—This is a minute anatomical account of the changes which take place in the conditions and relations of the constituent parts of the umbilicus, illustrated by several plates. Without the aid of these latter it would not be possible to give an intelligible account of the paper.

VI. *On the Value of Sulphate of Cinchonine in the Treatment of Intermittent Fever.* By M. MOUTARD-MARTIN.—This is one of the answers returned to a circular addressed by the Director-General of Public Assistance to the physicians of the various Paris Hospitals, requesting them to test the efficacy of sulphate of cinchonine in the treatment of intermittent fever, and to report to him the results of their observation. M. Moutard-Martin first makes some observations upon the causes which have led to various succedanea of the sulphate of quinine being vaunted as perfect substitutes for this substance, which have eventually proved to be well nigh worthless. This has arisen from the mode in which the trials of these substances have been conducted, indispensable precautions having been neglected. Thus, if a subject of ague quit the locality in which he has acquired the infection—the air, mode of life, &c., thus undergoing a complete change—the paroxysms will, in the great majority of cases, at once cease to appear. It is just under these conditions that patients in hospitals, upon whom most of the trials are made, are found. Thus, while the author was *interne* at the Hôtel-Dieu, Chomel was desirous of testing the efficacy of *cail-cedrin* in ague; but of 18 cases admitted within the space of four months, there were only 2 in which the paroyxsms had not disappeared or become so modified as to render the trial of a new remedy inexpedient. It is essential, then, before administering such that the patient should be watched for some days: and it is obvious that a few cases taken under such precautions are of more value than a greater number unaccompanied by such a guarantee. These precepts were indeed laid down by Chomel himself, about forty years since, during the early trials made of the sulphate of quinine, but they have of late been too much lost sight of. M. Laveran has shown their truth, even as applied to the fevers of Algeria, in which the mere passage of the patient from the place where he had contracted his disease into the nearest hospital, generally leads to a rapid cure, without the aid of medicinal agents.

Owing to his observing these and other precautions, the author found that among 51 cases of intermittent which presented themselves, he was only enabled to test the efficacy of the sulphate of cinchonine in 23 patients, and in 4 of these the disease was spontaneously declining, the medicine being given only to hasten its disappearance. In the 28 other patients, the disease either spontaneously disappeared, or was so ameliorated as not to justify any conclusions being drawn. Of the 19 cases in which the cinchonine was fairly tried, 13 were cured, in 2 its action was incomplete, and in 4 it entirely failed. The following are the author's general conclusions:

"1. Sulphate of cinchonine administered in intermittent fever exerts an incontestable, but variable, action. 2. Sometimes this action is rapid, and it cuts the paroxysms short, like the sulphate of quinine. In other cases it operates slowly, whatever be the dose given, the paroxysms only gradually disappearing. 3. The dose always requires to be larger by at least a third than that of sulphate of quinine employed under the same circumstances. 4. To secure its curative action a dose is required varying in different individuals from ten to fifteen grains. 5. In this dose it frequently gives rise to certain physiological effects (as headache, sense of weakness or faintness, colic, &c.) which it would not be prudent to aggravate. 6. The therapeutical action of the cinchonine is not proportionate to its physiological action: for sometimes it has cured without the patients having been aware of its action, while on other occasions it has failed when its physiological action has been energetic. 7. It cannot replace the sulphate of quinine in the treatment of severe intermittent fever. 8. But it may become a precious adjuvant to this, by completing a cure which has been commenced by one or two doses of sulphate of quinine. In this way we may unite certainty of treatment and economy of administration." (p. 466.)

The 23 cases are given in detail.

VII. *On the complete Obliteration of the Neck of the Uterus in Pregnancy.* By M. Depaul.—M. Depaul observes that great doubts have been entertained by most authors as to the reality of this pathological condition, uterine deviations having in their opinion been mistaken for it. At all events it is a very rare occurrence; and three cases having by a remarkable coincidence come under his own notice, he has deemed it desirable to call the attention of the profession to the subject. We will first furnish brief abstracts of the three cases, full details of which are given by the author. 1. He was called to a woman at full term, who had been in labour during two days, and whom he recognised as a former patient in whom he had applied the cephalotribe on account of a narrow pelvis. She had continued very well since that period. On the present occasion, the most careful examination, both with the finger and the speculum, made repeatedly by himself and M. Paul Dubois, failed to discover the slightest appearance of orifice in the cervix uteri; and after temporizing as long as it seemed safe to do so, a free incision was made opposite the portion of the cervix against which the head was felt to be presenting. Owing to the state of the pelvis, the cephalotribe had to be employed. By its aid delivery was effected without difficulty, and the woman did very well. 2. This woman, who had formerly borne a child, entered the Hôtel-Dieu when about at her seventh month, on account of vomiting, which resisted every means that had been had recourse to. This continued more or less after her admission, and was followed by eclampsia; and as owing to her excessive emaciation and debility, life was considered in imminent danger, it was resolved to induce labour by means of the uterine douche. On examining the condition of the cervix for this purpose, M. Depaul was much surprised to find a complete obliteration of its *internal* orifice, the external orifice being gaping and capable of admitting two fingers. The finger passed into this cavity was arrested at the depth of a centimetre by a thick, resisting, transverse partition, which so effectually cut off all communication with the interior of the uterus, that not even a small probe could be passed into this. An examination by the speculum fully corroborated the conclusion that had been drawn. A free aperture was made into the obstructing part, the liquor amnii becoming then discharged, and the os uteri somewhat dilating. The dilatation continuing, the woman was delivered by the forceps some hours afterwards, without difficulty. The convulsions returned soon after the delivery, and two days afterwards she died. At the autopsy, a cancer of the pylorus, which had not been suspected during life (the true cause of the obstinate vomiting), was discovered. Although the traces of the incisions which had been practised in the cervix were discernible, had the details noted above not been known, the obstacle which had existed could never have been suspected. The case is probably the first example of *obliteration of the internal orifice* on record. 3. The subject of this case was a primipara. It was found that no orifice whatever existed in the cervix uteri; and after the labour-pains had existed for a considerable time, incisions were freely made through the projecting part until a considerable aperture had been formed. These caused no pain and but little bleeding, and after waiting awhile, the pains not proving sufficient to complete the labour, a living child was delivered by means of the forceps. This patient became again pregnant, and was delivered of a large child without any difficulty. 4. Besides the above cases, M. Vosseur has communicated to the author another, which has never been published. This also presented itself in a primipara at full time. It differed from the others, inasmuch as the obliteration consisted only in a false membrane occluding the os, which was ruptured by means of the finger-nail during the presence of strong pains. To these four cases M. Depaul adds abridged accounts of six others which have already been published.

In most of these cases the origin of the obliteration is traceable to inflammation of the cervix, in consequence of prior laborious parturition; and indeed the lacerations and contusions of this part often produced in an ordinary labour render it a matter of surprise that it is usually so soon restored after that process to its natural state. There are other cases, however, in which this morbid condition of the cervix arises during the progress of pregnancy, and that in primiparæ. It must not be confounded with mere agglutination of the orifice, although both conditions may be only different degrees of the result of a similar inflammatory process, the greater thickness and older date of the adhesions constituting the peculiarity of the mode of obliteration now under consideration. It certainly, however, does not suffice that inflammation of the cervix be developed either prior to or during pregnancy, to give rise to obliteration of its orifices. This very exceptional occurrence supposes the existence of special conditions, the nature of which is at present unknown.

After dwelling upon various points in relation to the accurate diagnosis of this condition, M. Depaul goes on to observe:

> "There are certain congenital or accidental anatomical dispositions of the uterus and vagina which throw real difficulties in the way of the diagnosis of the obliteration of the external orifice. It is of importance that these, and the means of distinguishing them, should be examined into, especially as some of them have been considered to have deceived most of those who have published cases of complete obliteration during pregnancy. To commence with that to which most of such errors have been attributed, *deviation of the uterine orifice:* It is generally believed that during pregnancy the development of the uterus takes place in a regular manner, each portion contributing in an equal proportion to the general increase. This is an error which, for my part, I have long endeavoured to destroy; for, in fact, nothing can be more unequal than the development of the various parts. Confining myself to the subject before me, I may mention that I have demonstrated a great number of times upon women who have died at various more or less advanced periods of gestation, that the anterior wall of the uterus acquires a much more considerable increase than does the posterior wall, so that the vertical axis of the tumour formed by the uterine globe does not pass through the centre of the cervix, as might be supposed, but is directed much more forwards, and that in a varying proportion in different individuals, and in an increased degree as the term of gestation draws to an end. From this disposition it results that the orifice of the cervix does not correspond with the centre of the pelvis, but is

carried more or less backwards upon a much higher plane than that of the summit of the tumour plunged in the pelvis. To attain this it is necessary to turn around the lower segment of the uterus which covers the presenting part, and carry the finger sometimes to a level with the sacrum. Through inexperience or careless examination, an obliteration might be supposed to exist; but a well-practised finger is not deceived, and passing along the tumour to the insertions of the vagina, it will perceive either the relief formed by the uneffaced cervix or a simple depression surrounded by an edge which, though sometimes excessively thin, can always be slightly raised. . . . . It seems to me to be completely unjust to endeavour, on the ground of errors thus arising, to throw a doubt upon most of the cases of obliteration which have been recorded. I do not deny that in the case of excessive deviation of the cervix the diagnosis may not be difficult; but in order that it should prove impossible, the superior limits of the vagina either must not be sought for or cannot be reached. . . . . . Deviation of the cervix is not the only cause of error which we have to guard against. The difficulty may arise from a peculiar conformation of the vaginal portion of the uterus, and from an extreme narrowness of its aperture. These conditions may be congenital or may be the results of lacerations ordinarily produced during prior deliveries. The superior extremity of the vagina is not always detached at the same height from the cervix, so that the vaginal portion of the organ is far from being of the same length in all women. But a much more singular congenital disposition is sometimes met with. The vagina is not inserted at a certain distance from the orifice, but on the very edge of this aperture, so that the vaginal cul-de-sac does not exist. When we make an examination in such cases, in place of the projection formed by the cervix we come upon the concave vault formed by the extremity of the vagina, in which is the opening forming the entrance to the womb. This vault is ordinarily smooth and without bridles, but the aperture may be excessively small; and I have recently met with a case in which a very small probe could not be passed in. The woman had been married seven years, and had never become pregnant. The finger easily distinguished a small depression, which corresponded to this almost microscopic orifice. It was easily recognisable by means of the speculum, and during the menstrual period blood was seen escaping from it. I have had occasion to observe the same disposition in two other women at an advanced period of their first pregnancy. It is far more common to see the vaginal portion disappear or undergo considerable change as the result of difficult delivery requiring instruments. The more or less contracted orifice may then, so to say, be lost amidst the cicatricial tissue; and nothing but an attentive examination, and considerable skill in conducting this, will discover it. But while admitting the difficulties which exist in the diagnosis of these cases, I am convinced that by attentive and repeated exploration, bearing in mind the antecedents and other circumstances, the orifice, however small it may be, will not long escape detection by the true practitioner. . . . . .

"Among the vicious conformations which the vagina may present, there is one which should not be forgotten, for in some cases it may give rise to the belief in the existence of an obliteration of the cervix, or at least of a narrowing of the orifice. I allude to the presence of a transverse partition situate at a variable height, and dividing the cavity of the canal into two. In other cases, this segmentation of the vagina is due, not to a partition, but to a kind of co-arctation or circular wrinkling, occupying its vertical direction only to a very limited extent. The narrowed portion consists in these cases of hard cicatricial tissue, the inequalities of which often render the detection of the orifice a matter of difficulty. The shortness of the vagina, and explorations with the speculum, finger, and probe, enable us to recognise the fact that it is the vagina and not the neck of the uterus which has undergone the change in form. The detection of the aperture is rendered less difficult by the fact that the upper extremity of the vagina is rather narrowed than dilated, and can be easily felt and seen throughout its entire extent." (pp. 524–530.)

VIII. *On Osteomyelitis and Secondary Amputation after Gun-shot Injuries.* By M. JULES ROUX.—This paper contains the observations made by its author, a Surgeon of Marine at Toulon, upon the wounded who, returning from the Italian war in 1859, were placed under his care in the hospital of St. Mandrier. His object is to indicate the frequency of the occurrence of osteomyelitis after gun-shot wounds, and the danger this imparts to secondary amputation; and to advocate the preferability of disarticulation. We have already noticed his memoir* when it was read at the Academy, and the discussion which it gave rise to.

REVIEW VIII.

Om Smaknervernas ändningssätt i grodtungan jemte anmärkningar öfver nervernas likartade ändningssätt i de öfriga högre Sinnesorganerna. Akademisk Afhandling af ERNST AXEL KEY, Medicine Licenciat o. Kir. Mag. Lund, tryckt uti Berlingska Boktryckeriet, 1861. With a Plate. 8vo, pp. 35.

On the Mode of Termination of the Gustatory Nerves in the Tongue of the Frog; with Observations on a similar Mode of Termination of the Nerves in the Organs of Higher Sense. An Academic Thesis. By ERNST AXEL KEY, Med. Lic. et Chir. Mag.

THE present essay is an amplification of a paper previously published by the author in the German language, 'Ueber die Endigungsweise der Geschmacksnerven in der Zunge des Frosches,' in Reichert's and du Bois-Reymond's 'Archiv' for 1861, No. 3, p. 329. The retina and the olfactory region are the portions of the subject into which especially the writer has, in the Swedish version, more fully entered.

What structures of the *retina* are to be considered as the proper terminations of the nerves, or what in general in this complicated apparatus are to be referred to the nervous system, and what to the connective tissue? have long been disputed questions, respecting which the most varying opinions have been propounded. The author is inclined, with Schultze, to consider Müller's filaments, together with the membranæ limitantes and the reticulated intervening substance, as a form of connective tissue, constituting a support in the retina for the nervous elements. As to the latter, it would appear that the "rods" in Jacob's membrane may with tolerable certainty be considered as nervous terminal formations. The nature of the "cones" is much more doubtful. They would seem in some parts (the more peripheric) to be in connexion with nerves, in others with connective tissue, and in a third to be continued as a purely epithelial formation.

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* British and Foreign Medico-Chirurgical Review, vol. xxvi. p. 269.

The author suggests that it is possible that where the cones are decidedly of a nervous nature, as in the macula lutea, fine nervous structure may ascend centrally in them, and that they are not in their integrity to be considered as nervous formations. Where the rods are again present, they are to be looked upon as the principal terminal nerve-formations, although it might also be possible that the cones, even where they present themselves as being in connexion only with connective tissue, may nevertheless contain central nerve-formations which have hitherto escaped observation. In support of this view the author points out some analogies in comparative histology.

After Eckhard and Ecker had made known their suspicion that the epithelial cells in the *olfactory region* were in connexion with the nerves, Max Schultze succeeded, as Kölliker expresses it, in as nearly as possible deciding this question.

Thus, M. Schultze invariably found in this region in all classes of vertebrate animals two dissimilar species of cells, which had already been in part seen by Eckhard and Ecker, but had not been by them quite correctly understood and described. One kind of these cells were modified non-ciliated epithelial cells, whose inner prolongations often united with one another by means of lateral filaments, and eventually ramified towards the subjacent layer of connective tissue, without entering into any connexion with the nerves. "Between these was a great quantity of other cells of entirely different form, and of a peculiar chemical nature." They consisted "of a rounded cellular body and two minute efferent filaments proceeding in opposite directions, one of which, the peripheric, terminated on a level with the free surface of the epithelium; while the other, on the contrary, passed into the basement layer of connective tissue in the mucous membrane." The latter filament was extremely fine, and agreed both in appearance and chemical properties, so far as these could be ascertained, with the minute varicose filaments in which the olfactory nerve was lost in the mucous membrane itself. The peripheric efferent filaments of the peculiar cells (varicose fibre cells, olfactory cells, Sch.) bore, in many animals, capillary prolongations on their extremities in the surface of the epithelium.

Clarke has quite recently denied the presence of the olfactory cells in the mammalia examined by him—the sheep, rabbit, and cat; a result attributed by the author (who found them in the sheep, rabbit, and most easily in the frog) to his adopting different methods of investigation.

That the efferent filaments of the epithelial cells form connexions with one another, and that they present enlargements, and here and there dilate to inclose nuclei, is quite true, and has been stated by Schultze. The presence of the olfactory cells in the frog and the pike has been confirmed also by Clarke. He is inclined to assume that they anastomose with efferent filaments from the epithelial cells, which is, however, adds the author, never the case.

Having quoted the observations of Balogh,* the author adds:

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* Das Jacobson'sche Organ des Schafes, in the Sitzungsb. d. kaiserl. Akademie der Wissenschaften, Band xlii. p. 449.

"The existence of the olfactory cells appears to me now to be, by reason of what I have stated, placed beyond all doubt. Their connexion with the nerve filaments has not, indeed, been directly observed; but considering their peculiarities, by which they are so manifestly distinguished from the other epithelial formations, and the complete correspondence between their central offshoots and the peripheric nerve filaments; and, finally, seeing that the last-named formations meet each other in the surface of the mucous membrane, it must be admitted that the probability which exists in favour of their direct connexion is so great as to border upon absolute certainty."

In the vestibule and ampullæ, Schultze* found the auditory nerve to terminate in a manner precisely analogous to that in which the olfactory nerves end in the olfactory region. Thus he found that the nerves in the ampullæ and sacs of the ossicles in various fishes, after they had lost their medullary sheath, emerged from the foundation of connective tissue as free axis-cylinders, and ramified as minute filaments among the epithelial elements. Among the somewhat modified epithelial cells were peculiar formations analogous to the olfactory cells, whose minute central offshoots corresponded exactly to the filaments into which the axis-cylinders passed through repeated subdivision or ramification. Neither here did he succeed in directly observing the connexion between the nerves and the proper cells; but it would appear that there can be no doubt that the latter are the terminal formations of the former. Various observations are in favour of a similar termination of the auditory nerve in the vestibule and ampullæ in other vertebrate animals.

Shortly after Schultze had discovered the mode of termination of the nerves in the olfactory region, Billroth undertook the examination of the tongue of the frog, in order, if possible, to ascertain the mode of termination of the gustatory nerves. The interesting results he obtained have been communicated in the 'Deutsche Klinik,' May, 1857, p. 191; and in Müller's 'Archiv,' 1858, p. 159.

Leydig† had already found that the cells investing the extremities of the fungiform papillæ of the tongue of the frog were of a different nature from that of the other cells on the tongue. Thus at the edge of the papillæ the cells lost their ciliæ and their clear appearance, and acquired finely granular contents, with some tendency to yellow.

Billroth found that the nerves ascended only in the broad or fungiform papillæ, and that the nerve-filaments were pointed directly under the epithelium. From these their apparent ends he could not see any prolongations, but on removing the epithelium he observed a few pale fibrillæ projecting over the surface of the papillæ, and he believed that these were the extremities of the nerves and muscles emerging from the connective tissue. He found that the cells on the extremities of the broad papillæ were decidedly different from the others, both in the greater darkness of their contents and in the difficulty of isolating them, in consequence of their firm adherence to one another and to the papilla. Billroth believed that the most central ones were in connexion with the nerves, and that they were, in fact, terminal ganglionic cells. In his later essays, however, he expresses himself with much doubt as to this mode of termination, which he then considered as probable, only if the mode of termination of the olfactory nerves discovered by Schultze should acquire confirmation.

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* On the Mode of Termination of the Auditory Nerve in the Labyrinth: Müller's Archiv, 1858, p. 343.

† Lehrbuch der Histologie, 1857, p. 307.

Fixen denies all connexion between the nerves and the epithelium.*

Hoyer† believes that he has so fully established the mode of termination of the nerves in the broad papillæ of the tongue of the frog, that we may thence deduce inferences as to the mode of termination of other nerves also. According to him, the nerve-branch, ascending in a papilla, terminates at a short distance from the epithelium, being separated from the latter by a layer of connective tissue, "with a somewhat arched, sharply-defined extremity, in which the cylindrical ends of the nerve-filaments themselves lie close to one another, and appear as a layer of round bodies."

Krause has also recently‡ endeavoured to ascertain the mode of termination of the nerves in the papillæ of the tongue of the frog. He has only occasionally succeeded in seeing a minute prolongation of a nerve-filament above its apparent blunt extremity, and he considers the frog's tongue to be an unsuitable object for the investigation.

Having thus reviewed the literature of his subject, the author now proceeds to lay before his readers the results of his own investigations, which were carried on under the able guidance of Professor Max Schultze.

The solutions used by the author in hardening the tongue were either an extremely weak solution of chromic acid (one-fourth to one-sixth of a grain to the ounce), or one of four grains of bichromate of potash in the ounce of water. The sections were made as fine as possible with scissors—

"If from such preparations we endeavour to brush away the epithelial investment, the rest of the epithelium separates with tolerable ease and completeness, while its several elements fall from one another, or remain attached to one another only in small groups, the proper epithelium remaining attached, forming, as Billroth strikingly expresses himself, as it were, small crowns on the ends of the papillæ. Every such epithelial crown stands on a central, somewhat elevated plateau at the end of the papilla. Around this plateau runs, in the edge of the papilla, a furrow or constriction in the connective tissue itself. In this furrow the ciliated marginal cells and the bobbin-shaped intervening cells have their attachment, and usually some of the latter remain after the removal of the others.

"If we cause a dilute solution of soda to act on a papilla so prepared, we shall find that the nerve-trunk ascending in the centre of the papilla, at some distance from the extremity expands, with its neurilemma, into a basin-shape. This expansion occupies superiorly exactly the same extent as the proper epithelium, and forms precisely the plateau on which the latter stands.

"The central epithelium on the ends of the papillæ has therefore the expanded neurilemma itself as its support, while the surrounding fringe of ordinary epithelial cells is attached to the above-mentioned furrow, with ordinary connective tissue as its substratum. This arrangement is in itself sufficient to justify us in distinguishing the former as the *nerve-epithelium.*"

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* De linguæ raninæ texturâ, p. 25. Dorpat, 1857.

† Mikroskopische Untersuchungen über die Zunge des Frosches: Müller's Archiv, 1859, p. 481. See also our Microscopical Report for April, 1860, p. 517.

‡ Anatomische Untersuchungen, p. 56. Hanover, 1861.

On further examination it appeared that—

"The cellular elements therein are, as in the olfactory region, of two kinds—namely, modified epithelial cells, which do not appear to be connected with the nerves, and peculiar cellular formations imbedded between the same, which are the nervous terminal formations." (p. 21.)

"The epithelial cells themselves stand in a single layer, and consist of cylindrical cells, which, on a level with each other, pass into tapering efferent filaments. These run into the foundation formed of the nervous expansion, and here, or rather somewhat sooner, divide into numerous branches, which unite with one another, here and there contain nuclei, and over the whole expansion form a coherent network.

"Among the different filaments of the epithelial cells above-described, lie at different levels a number of cellular formations, which, in form, appearance, and behaviour towards reagents, agree with the olfactory cells in the olfactory region, and with the 'auditory cells' in the ampullæ." (p. 22.)

"The peculiar cellular formations above-mentioned might, by reason of their form and their agreement with the rods, and the granular matter belonging to them in the retina, be suitably designated by the name of rod-cells.

"By the arrangement of the epithelial cells and rod-cells with respect to one another, we have two divisions in the epithelium—namely, a superficial one, containing the bodies of the epithelial cells, and the peripheric efferent filaments of the rod-cells; and an inner one, occupied by the efferent filaments from the former and the bodies of the latter. These two divisions together exhibit a number of striking agreements with the layer of rods and the outer granular layer in the retina, and I believe they may with good reason be placed side by side with them." (p. 26.)

The author concludes with some observations on the nerves. The fine varicose filaments into which the axis-cylinders for the most part divide in the nervous expansion itself, enter, like those axis-cylinders which have not previously so divided, into the inner division of the epithelium.

"That they there enter into connexion with the central efferent filaments from the rod-cells, there can be no doubt, though we have only once seen these formations, and convinced ourselves of the harmony between them. Under particularly favourable circumstances I was able, the first time I distinctly saw an axis-cylinder separate into varicose filaments, to perceive one of these passing directly into a rod-cell. The central efferent filaments from the rod-cells are therefore to be considered, if we will, as broken-off nerve-filaments.

"The observation just mentioned of the direct connexion between the proper epithelial elements and the nerves in the tongue, must be regarded as, to a certain extent, corroborative of the direct connexion between the above described corresponding formations in the other organs of higher sense." (p. 30.)

The author sums up the bearings of the facts and arguments detailed in his interesting and instructive essay in the following deduction—

"*That the nerves in the broad papillæ of the tongue of the frog finally pass into the most minute varicose filaments, which at their extremities bear peculiar cellular formations existing in the epithelium itself* (gustatory cells or rod-cells). The beautiful harmony which is thus shown to prevail in the mode of termination of the nerves of higher sense, may be considered as evidence in favour of the correctness of the results arrived at."

We may, however, remark, that a comparison of the sentence last quoted with the preceding paragraph, would, at first sight, make it appear as if the author had been led, so far, into arguing in a circle. This fallacious mode of argument is, nevertheless, in this instance rather apparent than real. The wish of the author is evidently to show that the two facts alluded to mutually support each other. The essay is illustrated with a well executed plate containing eleven figures.

REVIEW IX.

Clinique Médicale de l'Hôtel-Dieu de Paris. Par A. TROUSSEAU. Tome premier.

Clinical Lectures on Medicine delivered at the Hôtel-Dieu, Paris. By A. TROUSSEAU. Vol. I.

THE volume before us is the result of many years of sedulous professional labour, embodied in a series of clinical lectures. M. Trousseau has well deserved the gratitude, not merely of those who have listened to his teaching, but also of those who only know him by his published works.

The introduction forms an apt prologue to the connected whole. Daily attendance at the bedside, habitual familiarity with the sick, whether in a medical or in a surgical ward, are marked out as of primary importance. The result will be—

> "These confused materials, which one amasses without order and without method, are nevertheless excellent of their kind. Useless to-day, you will find them later on buried in the treasures of your memory. Now an old man, I yet recal the patients I saw forty years ago, when I made the first steps in my career; I remember the principal symptoms, the anatomical lesions, the numbers of the beds, sometimes even the names of the invalids, who at this so distant date have struck my memory. These remembrances are often of service to me: they still afford me instruction, and sometimes you hear me invoke them in our conferences by the bedside." (pp. ix—x.)

A thorough acquaintance with the accessory sciences (chemistry, for example) is declared to be unnecessary, and even prejudicial to success as physicians; careful observation and collation of results form no unimportant part of the student's curriculum, and with this end in view:

> "That he will especially learn to know the course of diseases, the most important, the most essential knowledge for the practitioner. . . . . To know the natural course of disease is more than the half of medicine." (p. xx.)

This very necessary acquirement is by no means easily attained. The student must watch the practice of different physicians in cases of the same malady, the remedies employed, and the results of the treatment, bringing at the same time his own mental powers to compare and analyse the whole, himself

"Nullius addictus in verba jurare magistri."

The knowledge thus made his own, may be looked upon as the "boussole"—the compass of therapeutics.

The facts of therapeutics are shown to have been in great measure primarily derived from the domain of empiricism; yet though such be the case, the employment of remedies is by the intervention of the physician reduced to certain rules, and arranged in accordance with a standard no longer empirical.

The varieties of nosological arrangements and their probable value are then noticed. A strong preference is expressed by M. Trousseau for those appellations which do not imply adhesion to any special medical doctrine. "They will be so much the better the less they have of nosological meaning." (p. xxxii.)

The advice to students may be well closed by this quotation:

> "But, gentlemen, when you know enough (*connaître*) to be able to recognise (*reconnaître*)—allow me this play on words—hasten to forget nosologies, remain by the bed of the invalid studying each malady, studying the same malady in the case of each patient; as the naturalist studies the plant in itself, in all its elements, in all its varieties, forgetful of classes, families, and species, until he will know enough to systematize—that is to say, to comprehend, to discover, and to establish analogies." (p. xxxiv.)

This work treats in order and at considerable length in some of the chapters, on the

Eruptive Fevers.
Exanthemata.
Affections of the Mouth and Larynx.
Pulmonary and Chest Affections.

The space allotted will not allow of our doing more than noticing some of the principal subjects—three more especially which from their intimate characters or frequent occurrence have the most of daily interest for the practitioner.

I. The stages of incubation and access of scarlatina are carefully described, with their contingent accidents; the extremes of incubation named by the Professor are:

One case of twenty-four hours only.

One case where the symptoms pointed to positive inflammation of the brain, relieved, however, on the eighth day by the appearance of the characteristic eruption.

The co-existence of abundant miliary eruption (p. 9) is stated as almost invariable when the scarlatinal rash is at all confluent. The error of those authors is then commented on, who declare the more abundant the skin-rash, the less the chance of serious intercurrent affections. Directly opposed to this is M. Trousseau's experience that, "The severity of the disease is in direct relation to the intensity of the eruption." (p. 10.)

The diagnosis of the throat affection of scarlatina from diphtheritic exudation is thus given:

> "The palate curtain is red from the first day of the malady in scarlatina, of a tint analogous to that of the skin; the tonsils, slightly swollen, are of a violet colour. The fever continues, and after two, three, or four days, there

often appear upon one of the tonsils—sometimes on both—little whitish concretions, usually of a milky white, unless after vomiting they are tinged by the passage of the stomach contents. Examining them more closely, and raising them with the handle of a spoon, you will recognise that these concretions differ from false diphtheritic membranes; these (the latter) are adherent, and when one seizes them with forceps, they are raised in layers. The scarlatinal concretions, pultaceous, less adherent to the tonsil, have not the character of false membranes, but resemble much more those secretions which show themselves, for example, on the surface of ulcers of unhealthy appearance." (pp. 14–15.)

The very serious form of throat affection coming on about the eighth or ninth day, when recovery appears morally certain, is referred by M. Trousseau to diphtheria supervening on the original disease. This he believes, while yet hardly daring positively to affirm it. (p. 16.)

Among the special affections likely to present themselves during the period of decline, are named anasarca and hæmaturia; the latter almost always preceding or coincident with the general effusion into the tissues. The immunity enjoyed by the thoracic organs during the acute stage of the malady is no longer preserved. Two cases are noted where pleurisy of specially unhealthy type occurred with early purulent effusion; in both, on the twelfth day, paracentesis was performed.

The existence of scarlatina, unaccompanied by eruption—*scarlatine fruste*—is noted at some length.

II. Measles, with the accompanying accidents, are referred to in this chapter.

III. Speaking of the *discrete* variety of small-pox, the Professor tells us, what has perhaps hardly been recognised, that the pain in the back is not muscular, but dependent on the spinal cord—the proof being that in a large number of cases it is accompanied by paraplegia. Coincident loss of power over the bladder has also come under his notice.

The early time of eruption is noted as an almost positive diagnostic of the greater severity of the resulting affection, appearing early in the disease. M. Beraud* has noticed, and been the first to work out, a variolous orchitis in man and variolous ovaritis in woman.

Small-pox in its confluent form is described at some length, and the relative value of the accompanying accidents is discussed. In common with the earlier authorities, much weight is attached to the occurrence of swelling of the hands and feet, and the non-appearance of this symptom is marked as especially unfavourable. Various forms of cutaneous disorder wait on the course of the modified form, varioloïd.

IV. This section opens with the history of inoculation as first introduced into England; the process not, however, finding favour in France for nearly a century afterwards. No legislative enactment has hitherto interfered with the practice of inoculation across the Channel, and even in the hospitals it is still resorted to in exceptional instances.

M. Trousseau gives the result of some experiments instituted by him, from the analogy of veterinary medicine, to ascertain whether the manifestations of repeatedly inoculated variolous matter might not in a series of cases be ultimately reduced to the one pustule of inoculation, *la pustule mère.* The hope, however, was found to be deceptive. The occasional existence of this pustule mère, earlier in its appearance than the other pustules, is noted in some instances of small-pox derived from contagion.

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* Arch. Gén. de Méd., Mars et Mai, 1859.

V. Vaccination, in its history, derivation of virus, and mode of employment, is next considered.

The literature of the subject, particularly the standard work of M. Steinbrenner,* is freely referred to; and justice is done to the labours of more recent inquirers.

With us the seventh or eighth day after the insertion of the virus is usually chosen as the proper time for obtaining a supply of vaccine. The Professor strongly recommends that we should have recourse to lymph of not a later date than the fifth or sixth day. These are his directions:

> "When one wishes to propagate a legitimate vaccine, able to transmit the immunity which it usually confers, he must seek the virus in conditions the most favourable to its activity, take it from children in good health, of sound constitution; choose the fine and large pustules, well-developed (*fleuries*), from the fifth to the seventh day of their development." (p. 112.)

A careful report on re-vaccinations, embodying the conclusions arrived at by M. Gintrac,† from his experience in an epidemic of variola; with some severe strictures on the opponents of vaccination, closes this chapter.

VI., VII., are respectively devoted to varicella and roseola; the latter especially separated from measles by its tendency to appear more than once in the same subject.

VIII. *Typhoïd fever, or dothiénentérie.*—The intestinal lesion peculiar to this disease is shown to be constant in its occurrence, whether other symptoms, usually aids to the diagnosis, are or are not present. It must not, however, be looked upon as, *per se*, constituting the affection; it is simply the local evidence of a constitutional malady.

Peritoneal inflammation, most frequently dependent on direct perforation of the intestinal wall, may, however, arise spontaneously in the later stages of the disease. Cases illustrative of both forms are put on record. Jenner, in his work on Fever, has noted two instances of the latter description.

Intestinal hæmorrhage, in M. Trousseau's opinion, is not of such grave import as it is usually supposed to be. Thus, he says:—"That intestinal hæmorrhages in typhoïd fever, far from having the gravity which is generally attributed to them, form most frequently an appearance of favourable augury." (p. 149.)

The symptoms of typhoïd may for the first few days simulate the onset of intermittent fever, more especially in those who have been recently exposed to malarious exhalations, and *vice versâ.*

We welcome with much pleasure the warm testimony of the author to the necessity of giving food in all stages of the disease in question:—"Not only . . . . at a more advanced date of the pyrexia, when the fever is moderated, and the tongue less loaded; . . . . . but also from the first days, and during the whole course of the affection." (p. 184.)

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* Traité de la Vaccine, 1846.

† Gazette des Hôpitaux, July 11th, 1857.

IX. M. Trousseau has never met with a case of typhus fever in his own experience. He refers to the question of the identity of the fever-poisons (typhus and typhoïd), and cites the standard books of reference.

XI. The specific character (*spécificité*) of diseases forms the subject of consideration. The erroneous doctrines of Brown and Broussais are considered, and instances of certain local diseases are called in aid, to afford examples of varying appearances with varying causes. "To each special morbific cause, the organism replies by effects having their specific character." (p. 231.)

XII. In treating of contagion, the author gives the preference to the definition of M. Anglada.* It is, then,

> "The transmission of a morbid affection from the sick individual to one or more, by the intervention of a material principle, being the product of a specific morbid elaboration—which principle, communicated to the healthy man, determines in him the same phenomena, the same symptomatic expressions, as those observed in the individual from whom it has taken its departure." (p. 241.)

Here are the differences between the infectious and the contagious germ:

> "The infectious germ, generated under influences which we do not know, determines certain effects in the individuals who receive it; but these effects stop there, the germ dies out in the organism which it has infected.
>
> "Engendered primarily, also under influences which equally escape us, the contagious germ developes itself, bears fruit in the organism which has received it; it is, if I may so speak, conceived by it as the child is conceived by its mother; much more, it assimilates to itself all the substance of the economy—*Totus homo morbus fit*—which becomes in its turn the centre of new morbific emanations." (p. 251.)

Very definite stress is laid on the presence and mutual influence of the two factors—the morbific germ from without, the economy which receives it.

The quantity of the morbid principle has little relation to the great question of propagation; the quality of the germ is all-important.

The exanthemata are now considered; unlike the eruptive fevers, no immunity is conferred by any single attack from a future outbreak.

The sudoral exanthemata, under which head is ranged a larger class of appearances than practitioners have usually accorded—urticaria, erythema, &c. &c.—occupy five shorter chapters.

XVIII. Erysipelas of the head and face, according to M. Trousseau, supervenes in the majority of cases on some lesion of the integument, although possibly very minute; spontaneous development occurring when the affection is epidemic, rarely when such is not the case.

When the disease is uncomplicated, medical treatment is declared to be unnecessary. The Professor declares himself willing "to remain spectator of a contest, from which I well know that nature will come off victorious if I do not interfere with her in her operations." (p. 304.)

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* Traité de la Contagion, &c. Paris, 1853.

When erysipelatous affections are found in newly-born children, their cause must be found in the existence of a form of puerperal poison. M. Lorain* has worked out the analogy between the anatomical conditions of the mother and the child, and hence, it would seem, results a liability in each to the same severe forms of disease. The mortality in children is attributed less to their defective vital resistance, than to the peculiar characters of the malady in question.

XIX. It is beyond our limits to give even the barest *résumé* of this exhaustive section on diphtheria. We are strongly disposed to believe that, like most other epidemic diseases, the characters and course of this affection have their cycle of variations; and that the notes and opinion of any one observer will be found valuable to others, in proportion only to the similarity of the general conditions.

M. Trousseau speaks in the first place of simple, or, so to speak, non-malignant diphtheria, having its seat of election at the upper and back part of the pharynx, and, if unchecked, tending most frequently to terminate in extension to the larynx—i.e., in membranous croup. More than this, he is decidedly of opinion that, even in cases where the disease is marked at first by severe laryngeal symptoms, there, too, close examination of the pharynx would almost always detect the *débris* of diphtheritic exudation.

Even at the moment of writing, a case in point has occurred to us. A delicate child, five years old, taken ill with laryngeal symptoms at 1 A.M. on the Wednesday morning, was seen within six hours. Distinct traces of diphtheritic exudation were visible on tonsils and back of pharynx. The treatment employed was of no avail; tracheotomy was obstinately refused, and within twenty-four hours from the first notice of illness the child was dead.

We are, however, persuaded, and in this opinion the majority of English physicians will probably agree, that many cases of pure laryngeal croup do occur, in the production of which the peculiar poison of diphtheria has had no share.

The graphic description of the symptoms of laryngeal obstruction leaves nothing to be added to the portraiture. It has not appeared to us that, in this simple form, the gland-swelling has ever, either in time of occurrence or in amount, added much to determine the diagnosis; the symptom is essentially variable.

The *malignant* form destroys life, not by local extension, but by its depressing influence on the system as a whole—as a typical blood-disease. Here the œdema of the submaxillary tissues and the engorgement of the glands hold a far different value. M. Trousseau says:

"But that which is never wanting in this malignant form, that which savours of its malignancy, to avail oneself of the phrase of Mercatus (*pestiferi morbi*

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* Sur la fièvre puerperale chez la femme, le fœtus, et le nouveau-né. Paris, 1855.

naturam indolens), is the *swelling of glands.* It is considerable, and *extends itself to the cellular tissue* surrounding the lymphatic glands." (p. 337.)

After a close description of the accidental complications that may arise, including a special reference to the hæmorrhages from various parts, the section closes by a very apposite allusion to

> "A series of symptoms with which we are powerless to deal. This is a loss of appetite which nothing can conquer, and which shows itself as well in adults as in children. I have often tried to fight against it; I have frequently employed every kind of means; threats, and even violence, with young subjects, have been resorted to to make them take nourishment, but all in vain; they resist everything, will take neither solids nor fluids, and they allow themselves to die from hunger." (p. 343.)

Almost every mucous membrane in the body has afforded a nidus for the appearance of diphtheric exudation; and the removal of the epidermis, the disease once existing in the system, seems to be all that is necessary to ensure its deposition on the external integument. The disease is declared to be thoroughly contagious. M. Bretonneau* also, in his latest published work on this subject, espouses this view very warmly.

The forms of paralysis consequent on diphtheric affections are treated of lucidly and carefully. Their existence was noted by French physicians in the last century, but the positive connexion between their occurrence and the precursive malady is only of recent establishment. To M. Maingault† we owe the best treatise on the subject. The real cause of these strange nervous affections is "to be found in the poisoning of the economy by the morbid principle which gives origin to the malady on which these accidents depend. They are due to the disturbance sustained by the nervous system, to the peculiar modification which it has undergone—modification which, so far, we do not know, and which we perhaps never shall know." (p. 394.)

Very much confidence is placed by M. Trousseau in the agency of topical applications: "The topical treatment is the mode of treatment *par excellence* of diphtheric affections. (p. 504.) Astringent remedies, such as alum and tannin, caustic and chemically acting forms, as hydrochloric acid, strong solutions of nitrate of silver, are spoken of as of high value. Dr. Budd‡ recommends the use of Beaufoy's solution of chlorinated soda. We have employed it in several instances with marked advantage.

The operation of tracheotomy is described at some length, and the indications for its performance in laryngeal diphtheria are given. Much stress is laid on the slow and deliberate performance of the operation.

Chapters XX.–XXV. are devoted to affections of the mouth, throat, and larynx. From Chapter XXVI. to the end of the work, several of the more important chest affections are noticed.

XXXIII. With regard to the treatment of pneumonia, M. Trousseau says:

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* Archives de Médecine, 1855. † De la paralysie Diphthérique, &c. Paris, 1860.

‡ British Medical Journal, June 1st, 1861.

"From the fact that in a certain number of cases pneumonia has got well of itself, must one conclude that medical treatment should be expectant? I do not believe it, and for my part I should not know how to remain inactive in the face of this malady. As soon as I am called to an invalid attacked with simple pneumonia, without any complication, I hasten to interfere with the aid of an antiphlogistic plan of treatment." (p. 602.)

XXXIV. The subject of this chapter has been made by M. Trousseau so thoroughly his own, that a brief notice at least must be given to the indications for paracentesis thoracis.

These axioms are put forward as reasons for the performance of the operation, and cases are cited to show their truth :

1. Pleurisy may terminate life merely by the excessive amount of effusion.
2. It may do so more indirectly by becoming the starting-point of accidents which are mortal sooner or later. The effused fluid may become purulent, and hectic fever supervene.
3. The continual irritation of the thoracic viscera may induce tubercular deposit in those persons who have constitutional predisposition.
4. The anatomical changes in the position of the lung, consequent on long duration of a pleurisy uninterfered with, prevent the organ resuming its functions.

Iodine injections have proved of essential service in instances of purulent accumulation.

The direct indications for the performance of the operation are to be drawn, not from the oppression of breathing, but from the conditions made out by auscultation and percussion. This one symptom—i.e., the oppression of breathing—is thoroughly variable; its absence must not give rise to feeling of security, nor must its presence be deemed absolute proof of the immediate necessity for practising the operation.

The operation is then described; a preference is expressed for the simpler form of instruments; the puncture into the chest after division of the integuments should be made boldly and cleanly, since then false membranes will be penetrated by, not pushed before the point of the instrument.

The various objections to the operation are considered at the close of the section.

Our limits forbid any more lengthened notice. Enough will, however, we trust, have been adduced to prove the thoroughly practical character of the work, and to show the extreme care with which disease has been observed, and the attention paid to the relative value and interdependence of the accidental phenomena. We have met throughout with very many of the tendencies and opinions of some of our own highest medical writers.

The very aim and object of all sound medical authorship must be the accurate painting of disease, and the recording with all fidelity of the opinions and results of experience. In both these points we would add our humble testimony to the value of these collected lectures.

When so eminent a member of our profession sets forth the paramount importance of acquaintance with the natural history of morbid affections, and avows distinctly that this knowledge is, so to speak, the master-key of medicine, it augurs well for the future of the medical art. Based on data such as these before us, dependent no longer on the uncertainty of empiricism, content to be the handmaid rather than the fancied controller of nature, medicine may well be proud of the position which it will reach as an art, even though, with M. Trousseau, we allow that it falls short of the dignity of an exact science.

REVIEW X.

Health and Disease as Influenced by the Daily, Seasonal, and other Cyclical Changes in the Human System. By EDWARD SMITH, M.D., LL.B., F.R.S., &c.—*London*, pp. 409.

AFTER reading the preface to this work, we turned to its pages with more than ordinary interest, inasmuch as the author states *in limine* that he has undertaken to supply a deficiency "which exists in medical literature, and to offer the results of a series of inquiries in aid of our knowledge of the two functions of the medical practitioner—the preservation of health and the treatment of disease." And we are glad to be able to say that in the perusal, from the beginning to the end, we have not been disappointed, the performance, we think, as a whole, answering the promise.

From the very nature of the work, both its method and matter, it is difficult to review, its method being aphoristical—a series of propositions—its matter cycle within cycle of human existence, passing in order from the daily to the weekly and seasonal, and from thence to the changes in the ages of man, first giving the results of scientific researches, next their application to health and disease, including in the great argument the progress of the individual and of society, and not omitting those epidemic visitations which from time to time, as far back as history reaches, have been the scourges of mankind.

We are not disposed to find fault with this arrangement, for it has its advantages, especially with a view to development and enlargement, to which we look forward, little doubting that the book will be well received, and that Dr. Smith in successive editions will have it in his power to make emendations and corrections where these are needed, and to introduce without change of plan new facts as these are brought to light.

One, and the chief peculiarity of the work is its scaffolding or frame, this being the original researches of the author on certain physiological points of the first importance in their bearing on the vital economy—such as the rate of pulsation and respiration at different ages and under different circumstances, the quantity of atmospheric air inspired, the quantity of carbonic acid expired, the quantity of urea evolved and of urinary water excreted; comprising researches carried on for several years, and in part daily, with little interruption, and not neglecting those of other inquirers, chiefly contemporary and most deserving of credit.

Whilst such is the framework, the larger portion of the whole may be considered as deductive: inferences from the scientific premises used in explanation of the registered results and the accredited cyclical changes and events. It is this portion of the work which, if we mistake not, is most open to criticism and most in need of it. Whilst we can agree with the author in a large amount of his deductions, there are some respecting which we cannot but entertain doubt, and some from which we must dissent. However, even when compelled to dissent, we give credit to Dr. Smith for ingenious and plausible, if not always for logical, reasoning. One, and not the least, excellence of the writing is its suggestive character, its very thoughtful mood, prompting to further research and to methods of research such as modern science requires, that science which is founded on weight and measure and is essentially exact.

Instead of attempting a regular analysis, which of such a work could hardly fail of being futile unless unduly lengthened, we shall bring under the notice of our readers certain portions as specimens of the whole, not doubting that those who take an interest in the science and literature of medicine will read the original.

First, of the pulse and respiration: Much new and valuable information is given respecting these, as influenced by age and sex, by position, rest, and exercise, food, fasting, &c., the results of the author's own observations, and conveyed in the form of tables—the conditions in health contrasted with those occurring in disease. This account of these, two of the most important functions—the pulse as the index of the heart's action, the respiration as the index of the state of the lungs variously modified—is deserving of the utmost attention of the medical practitioner. And not less important are the observations made by the author, the results of his own observations, together with those collected from the best original authorities, on the quantity of air respired, the quantity of carbonic acid formed, and the quantity of urea and urinary water excreted. In all these a certain correlation is noticeable; a correlation between the air inspired and carbonic acid formed, the urea and urine voided, the temperature of the body, the power of bodily exertion and of mental activity. We must refer to the work for details; we shall notice only a few of the more striking and least expected results. Speaking of the effects of meals on the pulse and respiration Dr. Smith observed, that not only was there an increase of these witnessed during the act of eating, but also a decrease in the interval of the courses; and that in the instance of children, in whom it was most apparent, the temporary increase was oftentimes twelve and fifteen pulsations per minute. The following passages are an epitome of the general course in the daily cycle of pulsation and respiration, as deduced from actual trials. Whilst they convey curious and valuable information, they also afford an example of the author's method and style:

"21. In the evening, from seven to nine P.M., there is an evident tendency in the rate to decline, and with some slight variations this is continued progressively through the following hours, until from one to three A.M., when the rate is at its minimum. During the next two hours there is a slight tendency to increase, but it is very gradual until the usual hour of rising, when it will have attained an increase of several pulsations per minute. Immediately after the breakfast has been taken there is a rapid and great increase, which attains its maximum in the second hour afterwards, after which it declines greatly in an hour, and loses from ten to fifteen pulsations immediately before the dinner. After the dinner has been taken there is another increase, but the rate is seldom raised so high as that which follows the breakfast, and the highest point is attained in the second or third hour. This again is followed by a decrease which precedes, and a subsequent increase which follows, the tea, when a point as high as that which follows the breakfast is usually found; and lastly, there is the final decrease, which is usually progressive, notwithstanding that supper may be taken at a later hour. When dinner had been taken at a later hour than that above indicated, the rate of the functions followed the same course as that now given, except that there was not any important increase after midday until the dinner hour. The rate remained low, but not uniform, from twelve to one P.M., until the dinner hour.

"22. The extreme difference was sometimes thirty pulsations per minute, and was the greatest in children.

"23. The ratio of the two functions varied with each hour of the day, but was highest during the day, and the lowest during the night.

"24. The ratio is dependent rather upon respiration than pulsation, so that the high ratio of the day is due to the fact, that whilst the rate of both functions is then increased, that of respiration is increased disproportionately. The extremes were as 1 to 2·9 and as 1 to 5·7, or the larger was double of the smaller ratio; but there was no ratio nor any progression of ratios which was absolutely uniform on consecutive days.

"25. The effect of posture is very different in different persons and at various times, both of the same and different days, so that averages give but a very imperfect view of the result." (pp. 11, 12.)

The quantity of air inspired and of carbonic acid formed appear, from some very accurate experiments recorded, to have varied in the same ratio. In three trials on different individuals, the proportion of carbonic acid produced to the quantity of air inspired at rest was 1 grain to 58 cubic inches in one, to 58·5 in another, and to 54·7 in the third:

"There were commonly (Dr. Smith states) 4 minima and 3 maxima in the daily quantities of carbonic evolved, the former found immediately before each meal (except supper) and during the night, and the latter following each meal. The largest increase commonly followed breakfast and tea, and then the total quantities evolved were nearly identical, whilst there was also a great similarity in the minimum quantities recorded between the intervals of meals. This variation was due to food, but there was a low point below which the quantity did not fall. The highest amount of this variation was from one-third to one-fourth of the whole quantity evolved. There was not any hour of the day in which the evolution of carbonic acid was stationary, except in the hour immediately preceding breakfast." (p. 32.)

The quantity of urea discharged in the daily cycle was ascertained to be as variable as that of any other element of the daily changes; and what is very worthy of note, it was found to be greatly increased with the increase of urine under the influence of the water drunk, the latter acting, as it were, as a depurator of the blood—if we suppose, as is now very generally received, that the urea is formed in the blood—and is a criterion of organic waste—the débris of the nitrogenous compounds.

The second chapter, on the application of the scientific results to health and disease, is that portion of the work which is likely to be most studied, as laying down rules for the preservation of the one and the correction of the other, enforced by explanatory remarks founded on the results of experiments, using them as principles. The propositions expressing these rules will, for most part, be willingly assented to, more readily, we fear, than adopted and acted on, many of them being in opposition to the ordinary habits of society, such, as in the instance of the easy class, making the early meal the most substantial, when nourishment is most needed, and the vital power is at its maximum, and the late meal the lightest—a meal at which tea and coffee are most appropriate, these beverages promoting respiration and digestion, and thus conducing to the depuration of the blood and the excretion of urea; and in the instance of the working class, the propriety of not beginning labour before breakfast, and limiting it to the interval between the early and late meal—that is, from about six or seven A.M. to five or six P.M., when the system, on physiological data, seems best fitted for muscular exertion.

Whether in these rules the author has made sufficient allowance for the force of habit, as to times of eating and the power man has of conforming to circumstances without material injury of health, is open to question. We rather think he has not, and that consequently his rules are to be accepted for what they are worth; as true, so far as the limited results warrant, but not as universally true; and this remark applies to many other of his deductions.

From the daily cycle of changes to which man is subject, the author proceeds to the discussion of the weekly cycle, which, though not so evident, yet he believes to exist, founding his persuasion chiefly on the variations of functions observed to result from a day of rest, as exhibited in the rate of pulsation and respiration, and the evolution of carbonic acid and urea—all, he thinks, tending to demonstrate that "a periodical day of rest is necessary to the well-being of the body if a suitable amount of exertion be daily made;" and this he holds to be a rule applicable to the industrious of all classes, exempting idlers only, who may be left to the exercise of their own discretion.

Such a conclusion, the result of scientific inquiry, well agrees with the dictum of Sacred Writ, "that the Sabbath was made for man, and not man for the Sabbath;" and let us hope that the labouring man will never be deprived of the blessing of the one day out of the seven for rest, recreation, and the refreshment resulting from these benefits.

The seasonal cycle, the subject of the fourth chapter, is the one which Dr. Smith discusses most at length, prefacing it with a brief account of the opinions of the ancients thereon, and at the same time expressing his regret that in modern times it has not had the attention he thinks it deserves, nor had applied to it the methods of scientific research most fitted for its elucidation. Nor is this, we think, surprising, considering that as our knowledge increases, the inquiry into seasonal influences becomes more difficult, the elements of the problem increasing in number, and in so many instances increasing also in obscurity. The attempt which Dr. Smith makes to throw light on it by the application of the same principles as those used in the inquiry into the daily and weekly cycles of changes, cannot but have our commendation, though his deductions do not always command our assent. His first aim towards the solution of the problem was to determine the quantity of carbonic acid evolved in respiration. By carefully made trials, chiefly on himself, he arrives at the result that "the average amount of the whole year evolved at rest and before breakfast was a little over eight grains per minute;" and that the quantity at the various periods, varying within narrow limits with the precise character of the seasons of each year, may be tabulated as follows:

	Fixed.		*Variable.*
Greatest	Jan., Feb., March, April, May (July).	Decreasing	June (July).
		Increasing	Oct., Nov., Dec.
Least	August, part of September.		

The quantity of air inspired was found to bear a close relation to the amount of carbonic acid expired, so much so that they might be considered correlative facts, and of the same significance in relation to vital power. The same remark applied to the rate of respiration, whilst the rate of pulsation followed a different order, being highest in the summer months, according, the author is of opinion, with the activity not of the lungs, but of the skin.

> "It is a common error (he observes), both professional and laical, that the skin and lungs exert a mutually compensating action, so that when the one fails, it is supposed to be the duty of the other to take on an increased action by way of maintaining a due balance of the economy."
>
> Adding, "But a moment's consideration of these two organs would suffice to show that they cannot have a common action; and that whilst the function of the lungs, *par excellence*, is to aërate the blood and to evolve carbonic acid, the action of the skin is to cool the body and to eliminate water.
>
> "Yet (he remarks), there is one common result—viz., a marked tendency to a reduction of the vital powers, for so surely as the blood is more perfectly decarbonized by frequent and deep inspiration and moderately slow pulsation, so surely is it imperfectly purified by rapid pulsation and slow and feeble respiration."

The excretion of urea, according to the author's researches, follows much the same ratio as the pulsation, being greatest in the warmer months, when the evolution of carbonic acid is least; "and hence" (he thinks), "the relation of the production of urea to the assimilative changes, or to other vital actions, receives new light." The following clear and brief summary of seasonal influences will be read with interest:

"306. *Summer.*—The summer season exerts the most marked power, and under its influence the body exhibits the following minimum and maximum conditions:—

"307.—There is the *minimum* of carbonic acid and vapour exhaled, of air inspired, of the rate and force of respiration, of alimentation, and assimilation, of animal heat generated, of muscular tone and endurance of fatigue; and, in general, of resistance to adverse influence.

"308.—There is the *maximum* of the rate of pulsation, of the action of the skin, and the elimination of vapour; of the dispersion of heat, of the supply of heat from without; and of excess of heat, of the elimination of urea and urinary water, of the distribution of blood to the surface, of the imbibition of fluids, of relaxation of the tissues, and of poverty and carbonization of the blood.

"309. *Winter.*—In the winter season the above conditions are, for the most part, reversed.

"310. *Autumn.*—Autumn is marked by the summer or winter condition, as the character of the season resembles the one or the other; but it is essentially a period of change from the minimum towards the maximum of vital conditions.

"311. *Spring.*—In the early and middle parts of the spring season every function of the body is in its highest degree of efficiency; but as it advances, these maximum conditions merge into those of summer.

"312.—Hence the effect of season is more than the physical phenomena of temperature and atmospheric pressure will explain, and is so universal, that even the same amount of exertion made at two different seasons produced different degrees of effect upon the vital changes; less carbonic acid being evolved from it in summer than in winter in proportion to the relative amounts when at rest at those two periods." (pp. 158–59.)

In the sixth chapter, bearing the title of Applications to Health and Disease, the author discusses the influence of season upon muscular power, sensibility, growth, food, disease, and medical treatment. These several topics, all of them of the first importance in relation to the individual and to society, we can notice only very partially. His first proposition is, that "the muscular force and power of endurance of the body varies with the season, and the least is found at the end of summer or the beginning of autumn." This seems in accordance with the physiological premises, and is also supported by experience, in the instance of prisoners exercised on the treadwheel. But it may be a question whether what is true in one climate is necessarily so in a different climate, or in the same climate with a different race. The Maltese seems to have his energies excited by the dry heat of that hothouse of an island—a heat that is most remarkable in the advanced summer. The same remark applies to the Hindu, the Malay, and the creole black of the West Indies. A temperature that is congenial to the white man—one of from 50° to 68°—on them appears to have, not a tonic bracing, but a relaxing, enervating effect. And, in some individuals of our own race, the effects of a high and of a low temperature appear to be similar—both depressing. Such peculiarities ought, we think, to be kept in mind in reading Dr. Smith's commentaries on the rapidity of the circulation, the difficulty of maintaining a fixed temperature, on lessened chemical action and relaxation of tissue; especially taking into account that, however valuable his physiological data are, they are but limited, and this equally as to subjects, the persons experimented on, and the locality of the trials.

Another proposition we have doubts about, that respecting viability, which in the instance of children he holds to be greatest in those who are born in the winter and spring months. Were it universally true, ought not the mortality of children of white parents born within the tropics to be in a greater proportion than those of the same race coming into existence in a cold or cool climate? Yet we believe it is otherwise; a high temperature—one varying from 70° to 80°, and even 85°—appears to be suitable to infant life. Very young children, even in India, with its great vicissitudes of temperature, enjoy commonly good health; little anxiety is felt about them till infancy is passed. In England there are other causes than mere seasonal ones which may have an injurious influence, and produce the greater mortality of very young children in the hottest months—such as, probably, less nursing care; a less healthy state of the mothers, especially in the working class, the great majority, and amongst whom the proportional deaths are greatest at the earlier ages.

On the subject of alimentation the author expresses his opinion "that there is less variation in the amount and kind of food taken by mankind than has been asserted." This proposition we believe to be true, and it is well supported by the arguments adduced by Dr. Smith; yet we can hardly go along with him in the belief that the same dietary nearly will serve and preserve the health of man in a hot and cold climate, in winter and in summer. Other propositions which he promulgates seem to us to run counter; such as that "free dejection from the bowels is more necessary in summer than in winter;" that "spring is the season the most fraught with danger from excess of transformation of food;" and, that "the free emission of urea is the most necessary in spring and summer." If food in part serves as fuel in administering to animal heat, does it not follow that in the colder climates, *cæteris paribus*, most food should be consumed? It is the common belief of the most intelligent arctic explorers, that the appetite in those regions is vastly keener, and the power of digesting food very much greater, than in a mild or warm climate. Would the Hudson's Bay Company make an allowance of eight pounds of meat daily to a huntsman, if one-half, or one-fourth of that ration would suffice to keep him in health and strength?

"That nitrogenous foods are more necessary in the hot season than we at present admit," we are disposed to allow. Such experience as we have had in hot climates favours this conclusion—at least in the instance of Europeans. Thus in this class substantial animal food, if they have the means, forms a part of most meals; vegetables are used only in great moderation, and fruits least of all; but even to this exceptions may be found. And here, again, it is well to keep in mind the caution we have before given of the adaptive power of man, and how he can become accustomed in accordance with that power to almost any kind of food, whether entirely vegetablo or entirely animal, or variously mixed.

The amount of blood, the author is of opinion, is probably greatest in spring and least at the end of summer; and as regards liability to disease, the tendency is at the former season to those of a sthenic, at the latter to the asthenic, he believing that the foundation of seasonal ailment is the varying degree of vital force acting within the body at different seasons of the year. This may be so, but it is open to question whether the data are sufficiently extensive to warrant the inference. The subject, it must be allowed, is full of difficulties, especially extending the view to brute animals. But, even admitting that a low state of vital power favours the taking on of diseased action—the contrary, perhaps, might be maintained—it does not follow that there are not other conditions of an extrinsic kind, which may have more weight in producing season-diseases, whatever they may be, than the quality of the *vis vitæ*—circumstances which, as causes, may differ greatly in different climates and in different countries. Take, for instance, diseases of the alimentary canal; the author states that these "diseases have their maximum intensity and frequency at the period of minimum vitality," in the third quarter, and that diarrhœa is the most marked illustration of the fact. Granted that diarrhœa is then most common, may it not be because the external circumstances we have adverted to are then most strongly marked, such as indulgence in fruits, suppressed perspiration from careless exposure to vicissitudes of temperature, not to mention other causes acting on the individual likely to derange the primæ viæ. In the report attached to the census of Ireland for 1851, we have an example in point, where it is stated that "diarrhœa appeared to be influenced by the seasons, almost in the same proportion as dysentery, prevailing most in spring and summer, and least in winter and autumn, but was especially low in autumn;" and probably for the reason that in spring the standard diet of the people was then most scarce and of its worst quality, whilst in autumn it was most abundant and in its most wholesome state. In tropical climates, as in Ireland, the extrinsic causes seem to be the preponderating. There diarrhœa and dysentery are most prevalent whenever the heat is unusually great, accompanied as that event commonly is by drought. Even in a temperate climate, the same appears to be witnessed. The authors of an interesting paper recently published, 'On the Influence of Atmospheric Changes upon Disease,'* associate these two diseases with prevalency of a high temperature—in England above sixty degrees—whether in spring, summer, or autumn.

Epidemic diseases are adduced by Dr. Smith in support of his argument. It would be easy by a selection of special facts to prove the contrary. Cholera has been prevalent and equally fatal at all seasons in India; so, too, has yellow fever in the West Indies.† How much peculiarities of climate and other agencies are concerned in the mortality of peoples, is well displayed by contrasting two countries such as

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* By Arthur Ransome, M.B., and George V. Vernon, F.R.A.S., in vol. i., third series, of Memoirs of the Literary and Philosophical Society of Manchester, 1861.

† See foot note p. 29 of Dr. Blair's Account of Yellow Fever in British Guiana.

Ireland and the United States of America—the one remarkably free from ague, but ill-fed; the other the contrary. In an article on the census of Ireland for 1851, in the number of this Review for July, 1857, is the following:

> "Reviewing the whole decennial period, it would appear that the maximum deaths from all diseases was during the harsh and uncertain weather of spring [the potato then most deficient], that the number diminished with the warm weather of summer, and still more with the mild and more constant weather of autumn, increasing with the cold and greater variability of winter. The precise number of deaths returned under each season is the following:—Under spring, 443,182; under summer, 373,748; under autumn, 192,005, or 251,177 less than in spring; under winter, 340,787."

In the same Review, the number for April, 1858, is the following passage; it is from an article on the Vital Statistics of the United States:

> "The influence of climate in both countries, England and the United States, varies with the seasons, as marked by the mortality; whilst in the former it is greater in winter and least in summer; in the latter, generally, it is greatest in summer and autumn, and least in winter."

And it is added:

> "This difference, we have little doubt, is connected with a greater prevalency of malaria in the new country than in the old; and the circumstance that whilst the population of England is pretty equally divided between the towns and the rural districts, that of the United States is so distributed that only about one-fourth of the whole are collected in towns, and consequently the great majority are exposed to the influence alluded to."

On the cure of disease, Dr. Smith has some admirable remarks, which are especially deserving of the attention of the practitioners of homœopathy and their patients, as regards delusions, and of the sound physician and the sensible portion of the public as regards the probable influences of seasons in restoring health. Not but we think that in this instance, as in the preceding, the data are somewhat too scanty for the large induction made, a defect perhaps almost unavoidable in such an inquiry. We are inclined, too, to think, that in the rules in connexion with season, which Dr. Smith lays down for the use of stimulants, he attaches undue importance to seasonal influences, as if these should determine the amount of stimulus to be administered rather than the state of exhaustion, so that, according to him, the stimulation that would be proper at the end of summer in a case of cholera or diarrhœa, would be highly improper, the symptoms being the same, in the depth of winter, as if the debilitating effect of season was more powerful and required more attention than that of the disease. It is laudable to endeavour to reduce medicine to a science, but we apprehend the time is far distant for such nice distinctions as those implied in some of Dr. Smith's rules.

The eighth chapter, on the Cycle of the Ages of Man in its ascending and descending series and intermediate stage in middle life, will well repay perusal. The phenomena of each are well described, traced through the more important functions, and in connexion with each series the subject of diet is specially considered. The author's summary of the principal conditions of these different stages will convey to our readers some idea of his special views. We shall quote, in order and without comment, resisting often the temptation so to do, not so much from occasional disapproval as from occasional doubts.

> "710. In infancy there is the maximum relation of both azotised and non-azotised food to the weight of the body, and this declines gradually through childhood, rapidly at adolescence, and slowly through adult life to old age. There is the greatest sensibility, the highest degree of animal temperature, and the most active state of the skin. This lessens through childhood to adolescence, finds its medium in adult life, and its minimum in old age. In childhood and youth there is the greatest excretion of carbon and urea to the weight of the body. This declines rapidly in adolescence, and thenceforwards very slowly until the end of life.
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> "711. In adult life we find a state which is medium in degree, uniform in character, and prolonged in its duration.
>
> "712. The protection against dangers from without is the least in infancy, then in old age, then in childhood, and the greatest in adult life.
>
> "713. The dangers from within are the greatest in old age and in infancy, then about puberty and in adolescence, and the least in adult life.
>
> "714. The diseases occurring in infancy and adult life are more acute in their character, but in the former they are asthenic, and in the latter sthenic, whilst those which occur in adolescence and old age are chronic.
>
> "715. Old age is established in childhood, but manhood in adolescence.
>
> "716. In adult life the danger from nutrition is from excess, but in old age, adolescence, and infancy, from defect.
>
> "717. In infancy there is the greatest, in childhood a less amount of danger from an erroneous dietary, whilst in adult life there is the greatest tolerance of such errors." (pp. 328–29.)

His remarks applicable to health and disease which follow this summary and complete the ninth chapter are excellent, such as we can adopt almost entirely in an unqualified manner, and with the firm persuasion that were the rules they inculcate followed out in practice, the minimum of sickness and mortality, the maximum of health and longevity, would be the lot and reward of social man.

The three last chapters, on the Cycle of the Generations of Man, are of the highest interest, both from the nature of the subject and the manner of its treatment. The main argument of the author is, that whilst the constitution and powers of man have remained much the same from the earliest period to the present time, yet they have from time to time been subject to slight variations—flux and reflux—one while running an acute course, distinguished for strength; another, the opposite, distinguished for debility, as witnessed in the rise, progress, and decline of a nation. He thinks we have evidence of this even in our own times—a change "which has occurred so suddenly that men of forty years old can bear witness to it;" and hence he argues, "we find a probability that similar changes affecting the general character of diseases may have occurred a thousand times in the world's history." Whilst we can give our evidence to the latter, the record of history, we have our doubts about the former—the opinion of the day. We cannot say that our experience, which extends somewhat beyond forty years, is favourable to the conclusion at which the author has arrived. That classes of the population, such as those whose habits have undergone a change, and their diet altered, may have had their constitutions affected thereby, it is easy to conceive. The artificers in our manufactories, for instance, but not the great majority of the people; those whose diet and habits have continued much the same.

Dr. Smith, in arguing the question, passes in review the epidemics which have occurred in rapid sequence during the period, and the changes which he believes have taken place in the habits, wants, and dangers of society, adducing them in evidence of his proposition. His account of these is well given, and is deserving of attention, yet we do not consider his evidence as altogether convincing. As to epidemics, such as cholera, yellow fever, influenza, inasmuch as they attack the strong as well as feeble, and their invasion is sudden and unexpected; inasmuch, further, as their origin is obscure, and not yet brought within the limits of science, we attach but little importance to them in the solution of the problem. And, as regards the changes he refers to, the influence of these, as already stated, we believe to be partial. Amongst the opulent class, amongst the students at our universities, amongst the boys at our public schools, little alteration has been made in dietary during the last half century; and in the individuals belonging to them, whether in adolescence, early manhood, or mature age, we are of opinion there is not less *vis vitæ* than heretofore; and, with one exception—that of there being less gout in those of mature age than in past times—the diseases to which they are subject, both in kind and character, are little different. The like remark, we think, applies to the men of our army and navy, in whom, if there be a difference, we are of opinion it is in favour of the present generation, and this owing to improved sanitary measures; in the navy, to a more wholesome and strengthening ration; in the army, to better quarters, and other improvements affecting dress, diet (this still not what it ought to be), and discipline and treatment. Much emphasis is laid by those who, with Dr. Smith, are of persuasion that the constitution of the people now is lower than it was in *vis vitæ*, and more prone to disease of the asthenic than of the sthenic kind, founded chiefly on the alteration that has taken place in the mode of treating disease, and especially in the great disuse of the lancet and of the antiphlogistic plan, and to the greater trust that is now placed in stimulants and the invigorating system. This portion of the argument, with all due deference, we cannot hold to be strong, keeping in mind how in all ages medical practice has been influenced by doctrine, and how the great majority of practitioners follow the lead of a few—the authorities in the profession. To state a few examples, do we not remember how purgatives became the vogue after having been advocated by Hamilton, blue pill after its laudation by Abernethy, the local abstraction of blood after its commendation in gastric fever by Broussais? Do we not remember how long the stimulating plan of treatment, on Brunonian principles, prevailed in Italy, and the expectant plan in France? nor have we yet forgotten how mercury was considered as the *sine qua non* in syphilis, as inculcated by John Hunter, to the ruin of many a constitution, and conducing to many a fatal issue. Speaking from our own knowledge, we are satisfied that in many acute diseases years ago the lancet was used too freely, and even in some chronic ones: in the acute, such as pneumonia and dysentery; in the chronic, such as diabetes and phthisis. The instances we revert to were witnessed in our hospitals, civil and military. Man, it should never be forgotten, is a most hardy animal, and is capable of enduring much. It should be kept in recollection, too, that whilst there are *laudatores temporis acti*, there is also a disposition, especially in argument, to aggravate existing evils. Dr. Smith alludes to the disgraceful overcrowding of our barracks, in some of which, he says, speaking from report, there is not more than forty or fifty cubic feet of air for each soldier, and that the air is so offensive that the non-commissioned officer cannot enter it. This is very bad, allowing even for a little exaggeration as regards the delicate non-commissioned officer, once a private, who made the report; yet we have in recollection when, some forty years ago, barrack accommodation was worse —when, for instance, three or four men had to sleep in one bed constructed of boards, after the plan of a guard-room stretcher, and this in Scotland; or as bad, and not longer ago, when in the West Indies and in Barbadoes, sleeping in hammocks, there was as much crowding as between decks in a man-of-war. Imperfect as our barracks are at present, and needing improvements, it must, we think, be allowed that they were once worse, and that during the last fifty years, owing to the urgency of medical officers, there has been a slow abatement of their evils.

We are glad to see that Dr. Smith does not adopt the notion that we are not in strength and build of body equal to our fathers. And in not adopting it, we think he weakens his argument—if his argument be, as we had supposed—that the constitution of man now has less of vigour than formerly, and is not so well adapted to resist adverse influences; and in case of disease, the product of these influences, not so well fitted to bear the depleting system. If we have not read him aright, it may be, from our attaching to the term sthenic a sense not altogether the same as that in which he uses it. Our notion is that as it implies strength, the man of strength will be most subject to diseases of a sthenic kind, and *vice versâ*. The man whose constitution is anywise enfeebled, whether by excesses, as in the instance of the drunkard, or from deficient support, as in the instance of the famine-stricken wretch, will be most liable to disease of the opposite type; the most perfect health and the purest sthenic condition being, we think, as nearly as possible correlatives. This, too, we would fain believe, is the opinion of the author; and it seems implied when he speaks of the changes which have taken place in Ireland since the famine; "with the potato" (he says), "the Irishman lived, and when it failed he died; but since that period, the corn-crop has been introduced, and with a higher kind of food the fever and the scrofula which perennially infested the country have nearly passed away, and now we find both a better organization and one far less liable to disease." Yet, we may remark, there has been since that change, with an improvement as to strength and health, no change in accordance in the mode of treatment of disease. Twenty-five years ago, venesection was there largely employed, as much so as in England; as in England, it has there gradually fallen into disuse, and now it is almost never resorted to. This we learn from a friend of large medical experience in that country, and in expressing the fact we use nearly his own words.

Many other passages we had marked for comment, but we must pass them by for want of time and space. So thoughtful a work needs to be read with thought; and no work that we are acquainted with, at least of recent times, deserves more and will better repay attention in its perusal. It affords a striking example of the progress which has been made in physiological science, especially in the statistical branch of it, comparing the results it brings out regarding the more important vital functions with those which formed the amount of knowledge at no very distant period. It was in the first year of the last century that Martin Lister edited the 'Statica Medicina'—a work also of aphorisms —of 'Sanctorius Sanctorius,' of which he said "non aliud profecto inventum in medicina, præter unum sanguinis circuitum huic comparandum est;" that discovery to which he refers with so much eulogy being the very simple and elementary one that the body sustains a certain loss by insensible perspiration.

PART SECOND.

Bibliographical Record.

Art. I.—*A Practical Treatise on the Use of the Ophthalmoscope; being the Essay for which the Jacksonian Prize in the Year* 1859 *was awarded by the Royal College of Surgeons of England.* By J. W. Hulke, F.R.C.S., &c.—*London*, 1861.

A work on this subject has for some years been much wanted, both by student and practitioner; the essay by Bader and Roberts in this journal, the little work of Mr. Hogg, and various papers scattered in the periodicals, could not be considered as perfect guides to the use of the instrument, nor did they enable the practitioner to survey the results of its use arrived at by others. On the Continent this want has been pretty well supplied; in France, by the clearly and elegantly written lectures of Follin, and by the illustrated works of Guerineau and Sichel; in Italy, by the diffuse yet interesting treatise of Inaglino; in Germany, by the compilation of Zander, the grand folios of Ruete and Jaeger, &c.: in many respects the essay on this subject by Liebreich, prefixed to the French edition of Mackenzie, remains yet undoubtedly the best in any language.

Mr. Hulke has undertaken to satisfy this want in England, and we may at once say that we believe he has to a considerable extent succeeded. His work is divided into three portions: the first treats on the ophthalmoscope as an optical instrument, gives a description of some of the more useful forms of apparatus, and a few remarks on their management. We think a few words on the real and apparent magnitude of the various parts examined would have been useful, the two being almost always confounded; thus, in the description of cases we often find it noted that the optic papilla was very large or very small, without the slightest proof of the real existence of any such state. Again, the subject of oblique illumination deserved to have been treated at greater length, as by its means the cornea, iris, lens, and even the anterior portion of the vitreous body, may be most advantageously examined; for these parts the ophthalmoscope furnishes a useful, yet quite subordinate, method of examination. We do not find the powers of the lenses most generally useful anywhere stated.

The second portion describes "the ophthalmoscopic appearances of the healthy ocular structures," and will prove a very valuable aid to the beginner in studying what is and what is not morbid.

The third portion treats on "the ophthalmoscopic appearances of diseased structures and congenital imperfections." Although without exception admirable so far as it goes, we think this portion might have been much extended with advantage; and, at all events, references to where longer descriptions might be found would have much increased its value without augmenting the size of the book.

It is printed on good paper, in a clear and rather large type, and is illustrated by four plates containing nine coloured figures, and a number of woodcuts. We have no doubt that this work will materially aid in diffusing a correct knowledge of this subject in England: the student may fully rely on what he finds there, and the author has shown that he himself possesses a very excellent acquaintance with this branch of ophthalmic science. We heartily recommend this work to all who may wish to study this portion of our medical art.

ART. II.—*Das Schräg-Verengte Becken von Seiten der Theorie und Praxis nach dem Gegenwärtigen Stand der Wissenschaft.* Von Dr. A. E. SIMON THOMAS.—*Leyden*, 1861.

The Pelvis Obliquè-Ovata. By Dr. S. THOMAS.

DR. SIMON THOMAS, the distinguished Professor of Midwifery at Leyden, has here presented us with the most elaborate and the most valuable description of this remarkable form of pelvic distortion which has appeared since the great work of Naegele. The author informs us, that he was led to investigate the subject fully owing to the interest excited in his mind by the occurrence of two cases in his own practice. His researches, embracing not only his own experience but also an analysis of all the cases recorded in literature or preserved in museums, must be regarded, in one sense at least, as complete. An idea of the scope of the work may be formed by the following sketch of its arrangement and contents. The first part is devoted to the methodical description of the specimens of distorted pelvis. The second part discusses the general questions of pathology and treatment. His classification of the pelves described is distributed under the following heads:—

1. The obliquè-ovate pelves which already existed in anatomical collections in 1839, and remained unknown to Naegele. Of these he describes eight.

2. The obliquè-ovate pelves which were observed after the publication of Naegele's monograph. Of these the author has collected thirteen examples. So that we have 21 undoubted cases to add to the 29 collected by Naegele.

3. Pelves which were considered to be obliquè-ovate during life, and which apparently belong to this kind. Two cases only are related—namely, one by Hayn (1852), and one by Hohl (1856).

4. Pelves which without foundation have been esteemed to be obliquè-ovate, or of which it is unproved, or partly improbable, that they belong to this kind. Eight cases, including Professor Meig's, two by Von Ritgen, and others, are referred to this head.

5. Other pelves, which through vicious form of the sacrum or other

causes, are deformed, resembling the obliquè-ovate pelvis, but which possess a normal ilio-sacral articulation. Three pelves of this description are recorded.

In the second part, the Professor considers the opinion of Naegele as to the frequency of the deformity. Assuming the complete fusion of one side of the sacrum with the os innominatum, as an essential character of the obliquè-ovate pelvis, he then discusses the mode or modes in which this primary anchylosis may originate.

Referring to the controversy which arose as to Naegele's theory of the origin of the distortion in a vice of original conformation, Dr. Thomas classifies the 50 known cases in a manner to test the accuracy of this doctrine, and to illustrate the various causes which may lead to the deformity.

Thus there are, in the first place, cases in which it was known that the patients had suffered in childhood, or before the anchylosis was known, from disease of the pelvic bones. He cites nine cases of this description from Betschler, Hayn, Fabbri, Von Ritgen, S. Thomas, Von Holst, Rosshirt, Sinclair, and Hecker.

Secondly, there are cases in which a fracture of the os pubis exists on the same side as the anchylosis, which was considered as the cause of the anomaly. Of these, he cites two cases from Otto and Lambl.

Thirdly, there are cases with traces of periostitis or exostosis on the hip-bone. Three are cited from Naegele and Voigtel.

Fourthly, the cases of coxarthritis, of which there are five examples found in Naegele, Sandifort, and Danyau.

Fifthly, there are cases exhibiting no residue of disease, but the history of which is imperfect. Of this class are many of Naegele, Martin, Nichet, and Bartels.

Sixthly, there are cases in which there are no visible marks of bone-disease.

Thomas adds the description of the pelves of two children exhibiting such diseases of the ilio-sacral articulation as, had they lived, would have terminated in obliquè-ovate distortion.

He gives the following summary of his conclusions:—

1. In every obliquè-ovate pelvis, the anchylosis is the primary abnormality, and is an acquired evil.

2. Inflammation of the ilio-sacral joint is necessary to produce anchylosis.

3. This inflammation may arise at any age, even during foetal life.

4. The inflammation may arise from an internal cause primarily in the joint, or it may arise from a traumatic lesion extending from neighbouring joints.

5. The anchylosis brings about an atrophy and a contraction of the abnormally united bones.

6. The earlier the anchylosis, the more complete is the deformity.

7. The defect of the ala of the sacrum in cases in which the anchylosis arose early, is only apparent.

8. If the anchylosis arises after puberty, and the pelvic bones have

acquired full growth, then only a wrinkling of the neighbouring bones is observed.

9. This bone-atrophy is similar to that which is observed in anchylosis of moveable bones.

10. After healing of the primary disease which led to anchylosis, the traces of the original joint are so completely lost that a superficial examination might deceive, and section only can clear up the cases.

11. The remaining deformities beyond the anchylosis—as the obliquity and narrowing of the pelvic canal, the flattening of the pelvic wall, the smallness of the greater ischiatic notch, the scoliotic bend of the lumbar vertebræ, &c.—are secondary, and must be referred partly to atrophy of bones, partly to unequal pressure from disturbance of the equilibrium of the body.

The diagnosis of this deformity, the Professor says, is not difficult if the characters of it are present to the mind of the obstetrician. The distortion may be inferred when it is obvious that labour is retarded mechanically, *although the promontory cannot be reached;* when the two crests of the ilia are not in the same horizontal level, and neither scoliosis nor anomalies of the lower extremities are present; when the two posterior superior spines of the ilia are unequally distant from the crista sacralis; when there are scars near the posterior superior spines of the ilia; when the history teaches that injuries of the pelvis have been sustained.

With regard to the influence on labour, the deformity is very unfavourable. Natural delivery is probable only when the child is small and the deformity not great. Breech-presentation is more favourable for the mother, less so for the child. In head, as in breech cases, the position with the back to the flattened side of the pelvis and forwards is the most favourable.

The rules of treatment flow from the foregoing considerations. The forceps will be necessary if the deformity is not great, and the child's head small. In many cases perforation or cephalotripsy will be required, or the Cæsarean section, if the child is alive, even in cases of moderate deformity. Turning is never indicated on the ground of the form of the pelvis alone, but may be called for in certain complications of labour. If possible, all these operations should be anticipated by the induction of labour.

This admirable work concludes with a tabular view of the history, general and parturient, of all the cases; and is illustrated by well-executed figures of obliquè-ovate pelves. It leaves, in short, hardly anything to be desired. The labour and profound obstetric knowledge displayed reflect the highest credit upon the Leyden professor.

Art. III.—*Compendium of Human Histology.* By C. Morel, Professor Agrégé à la Faculté de Médecine de Strasbourg. Illustrated by 28 Plates. Translated and edited by W. H. Van Buren, M.D., Professor of Anatomy, New York.—1861. pp. 207.

The American editor, apologizing for the scantiness of original labour in his own young and busy country on the ground that workers are of necessity too much engaged in active and practical duties, has given in his preface good reasons for his reproducing this book from the French. To the "excellence and fidelity" of its plates, and the clear and concise manner in which the minute anatomy of tissues is portrayed in this work, we can bear witness. Indeed, it is not too much to state that we know of no illustrations in which the appropriate character of the various objects considered is more closely and more finely rendered. The text descriptive of the plates is simple and intelligible, and accompanied by occasional observations by the editor.

Art. IV.—*An Elementary Treatise on Human Anatomy.* By Joseph Leidy, M.D., Professor of Anatomy at the University of Pennsylvania.—*Philadelphia*, 1861. pp. 663.

This work is very copiously adorned by illustrations, the majority of which are acknowledged as having been borrowed from other sources. Some original ones by Dr. H. D. Schmidt, are graphic and quite new. An introduction treats very briefly of elementary structures and principles, and the description of each anatomical system is prefaced by an examination of the microscopical characters of the structures composing it. A series of notes at the foot of the pages contains a list of synonyms of terms and designations used in the various descriptions of parts.

Art. V.—*A Manual of Chemistry, Descriptive and Theoretical.* By William Odling, M.B., F.R.S., Fellow of the Royal College of Physicians, Secretary to the Chemical Society, and Professor of Practical Chemistry at Guy's Hospital. Part I.—*London*, 1861.

Professor Odling has at last given us an instalment of his long-expected work. The author is the most earnest and brilliant expositor of the Unitary Notation among English chemists: his extensive reading, his ingenuity in conjecture, and his happy facility in perceiving the various relationships of chemical compounds, thoroughly qualify him for the important task which he has undertaken. A Manual of General Chemistry, in which the views originally propounded by Laurent and Gerhardt should be clearly displayed with their recent developments, was greatly needed. Many professors and teachers now advocate the newer and more consistent chemical doctrines, and have consequently felt the want of suitable books of reference and manuals of practice for the use of their pupils. So far as regards instruction in analysis, this want has been supplied by two handbooks, the larger of these, by Messrs. Northcote and Church, being the first original work in the Unitary Notation published in this country; and the other, by Mr. Conington, being an adaptation from the German of Prof. Will. In the use of such special works as these, Prof. Odling's treatise will form a necessary adjunct, as a store of the most important facts and phenomena of chemistry, and as a systematic and satisfactory exposition of its laws.

The present manual differs from others not only in the adoption of the unitary notation, but in the order in which the contents of the work are arranged. This plan, which has been carried out so far as regards carbon-compounds by Prof. Kekulé, in his 'Organic Chemistry,' may be briefly characterized as follows. Hydrogen is regarded as the typical element, non-metallic bodies and metals being described in succession, according to their more or less complicated relations to that type. Thus, after a few pages devoted to "General Considerations," the distribution, preparation, properties, and relations of hydrogen are clearly discussed, and then the "monhydric" elements are treated in a similar manner. Among the non-metals (to which alone Part I. is devoted), chlorine, bromine, iodine, and fluorine belong to this class. When various facts relating to the distribution, preparation, general properties, and substitutions of chlorine have been detailed, we find chlorhydric acid, chlorides, and the numerous oxides and oxygen acids of chlorine, similarly treated; and so also with bromine, iodine, and fluorine, the congeners of chlorine. After the description of the most important compounds of these four elements, a brief but extremely interesting and instructive summary is given of their most conspicuous relations to one another. We then pass on to the consideration of the dihydric elements, oxygen, sulphur, selenium, and tellurium, the remainder of the volume describing the "Nitrogen Group," or trihydric elements, nitrogen, phosphorus, arsenic, antimony, and bismuth.

The advantages arising from the employment of the unitary notation are very conspicuous in the volume before us. Many substances whose chemical relations seemed doubtful before, now take their places in recognised series, filling up gaps therein. Other bodies, whose origin or reactions were clear enough, but whose position and formula were doubtful when simply expressed according to the unitary notation, come immediately into harmonious relations with allied substances. Dr. Odling has seized every opportunity of unfolding the new views, and has succeeded in clearing away many anomalies by his suggestions and experiments. He points out the chief features characteristic of his work in the following words:

> "As a rule, the atomic weights selected for volatile elements represent single volumes, and those for volatile compounds double volumes, of their respective gases or vapours.
>
> "The great majority of compound bodies are expressed as unitary molecules, by unitary formulæ, instead of by additive, or, as they are commonly called, rational formulæ. Throughout, the algebraic sign of addition is never used to express combination.

"The equivalent notation, by means of dashes, is here employed for the purposes of elementary teaching.

"The arrangement of the book is more than ordinarily systematic. The mutual relations of the elements and of their analogous compounds are largely dwelt upon, as are also the mutual relations of the various heterologous compounds of the same element. The properties of classes of bodies, chlorides, oxides, sulphates, &c., are described with greater fulness than is customary in text-books.

"The compounds of mineral and organic chemistry are not considered apart in separate sections. Moreover, the doctrines of series, types, and substitutions, are applied indiscriminately to both branches of chemical science."

It is difficult to give a fair idea of the peculiar merits of Prof. Odling's Manual of Chemistry without quoting from many pages of the volume; however, a few illustrative extracts, containing statements either novel or striking, may induce some of our readers to make further acquaintance with the original work.

Pages 1 to 30, treating of general considerations, commence with an introduction in few words to the main facts concerning the numerical relations of the elements, their natural groupings, and the distinctions between atoms and molecules. The important doctrine of equivalent substitution is taught at greater length. Adopting hydrogen as the standard of atomic weight and of specific gravity, it is also made the standard of equivalency or interchangeable value. Any radicle, simple or compound, capable of entering into the place previously occupied by hydrogen in a body, is named mon-, di-, or tri-hydric, according to the number of atoms of hydrogen which one atom of it can displace, various degrees of equivalency being marked by dashes placed over the left hand of the symbol. Thus, the relations to hydrogen of the four great typical compounds, hydrochloric acid HCl, water H_2O, ammonia H_3N, and marsh-gas H_4C (each formula representing the condensation to two volumes of two, three, four, and five volumes respectively), may be clearly seen in this table, given on p. 17:

Prot-equivalent radicles.	Bi-equivalent radicles.	Ter-equivalent radicles.	Tetr-equivalent radicles.
$HCl - H = Cl'$ Chlorine.			
$H_2O - H = HO'$ Eurhyzene.	$H_2O - H_2 = O''$ Oxygen.		
$H_3N - H = H_2N'$ Amidogen.	$H_3N - H_2 = HN''$ Imidogen.	$H_3N - H_3 = N'''$ Nitrogen.	
$H_4C - H = H_3C'$ Methyl.	$H_4C - H_2 = H_2C''$ Methylene.	$H_4C - H_3 = HC'''$ Formyl.	$H_4C - H_4 = C''''$ Carbon.

Here the fact is displayed very clearly that "the equivalency of the radicle increases by one unit for every atom of hydrogen abstracted."

The author shows (p. 18) how, by an enlargement of the idea of "double decomposition," most chemical changes, such as the direct union of two elements, the substitution of one element for another, the breaking-up of a compound into its elements, and the liberation of a single element in the free state, may be referred to this process. This generalization, though not originally pointed out by Prof. Odling, is explained by him very lucidly in the numerous particular cases which he has occasion to describe. The doctrine of organic radicles, bodies which may or may not have an actual existence apart from the compounds into which they enter, but which can be transferred to other compounds, is explained in all its phases, and its intimate connexion with the doctrine of double decomposition pointed out.

We meet throughout the book with such tabular views of related compounds as the following:

"The acids and potassium salts of chlorine:

Chlorhydric acid . .	HCl . . .	KCl	Chloride.
Hypochlorous acid . .	$HClO$ . . .	$KClO$	Hypochlorite.
Chlorous acid . . .	$HClO_2$ . . .	$KClO_2$	Chlorite.
Chloric acid	$HClO_3$ . . .	$KClO_3$	Chlorate.
Perchloric acid . . .	$HClO_4$ . . .	$KClO_4$	Perchlorate." (p.51.)

Again, the relation of the anhydrides to the acids is shown by the examples given below, which we have collected together:

	Acids.		Anhydrides.
Hypochlorous . . .	$2HClO$	$- H_2O =$	Cl_2O
Iodic	$2HIO_3$	$- H_2O =$	I_2O_5
Sulphurous	H_2SO_3	$- H_2O =$	SO_2
Sulphuric	H_2SO_4	$- H_2O =$	SO_3
Phosphorous	$2H_3PO_3$	$- 3H_2O =$	P_2O_3
Phosphoric	$2H_3PO_4$	$- 3H_2O =$	P_2O_5

The following lists comprise a few of the compounds hitherto regarded as in some degree anomalous: here and there a substance of well-ascertained position is inserted, for the sake of comparison.

$K_2SOOOO = K_2SO_4$	Potassium sulphate.	
$K_2SSOOO = K_2S_2O_3$	„	thiosulphate (hyposulphite):
$K_2SSSSS = K_2S_5$	„	pentasulphide.

Here sulphur and oxygen appear interchangeable.

The analogies indicated in the names and formulæ assigned in the first column to the following bodies are interesting:

Sulphurous chloraldehyd . . .	Cl_2SO	Bichloride of thionyl.
Sulphuric chloraldehyd	Cl_2SO_2	Bichloride of sulphuryl.
Nitrous chloraldehyd	$ClNO$	Chloride of azotyl.
Nitric chloraldehyd	$ClNO_2$	Chloride of nitryl.
Chloride of azotyl	$(NO)Cl$	Chloronitrous gas.
Bichloride of azotyl	$(NO)Cl_2$	Chloronitric gas.
Acid sulphate of azotyl	$(NO)HSO_4$	
Hydrate of azotyl	$(NO)HO$	Nitrous acid.
Oxide of azotyl	$(NO)_2O$	Nitrous anhydride.
Thionyl-diamine	$(SO)''H_4N_2$	Thionamide.
Sulphuryl-diamine	$(SO_2)''H_4N_2$	Sulphamide.
Phosphorous chloraldehyd : . .	PCl_3	Terchloride of phosphorus.
Phosphoric trichloraldehyd . . .	PCl_3O	Oxychloride of phosphorus.
Sulpho-phosphoric trichloraldehyd	PCl_3S	Sulphochloride of phosphorus.

ART. VI.—*A Manual of the Dissection of the Human Body.* By LUTHER HOLDEN, F.R.C.S., Assistant-Surgeon to, and Lecturer of Anatomy at, St. Bartholomew's Hospital. Illustrated with numerous wood engravings. Second Edition.—1861. pp. 576.

IN this edition much additional material is presented to the student, and many parts will be found to have been re-modelled. The illustrations would doubtless have been rendered much clearer and more easily and quickly understood had they been coloured; but then, of course, the volume would have been more costly. The student is recommended to supply the omission by lightly tinting the arteries red, the veins blue, and the nerves yellow. The foot-notes render the work more valuable by the frequent allusions which they contain to French and German writers on the subject.

ART. VII.—*On "Supporting the Perinæum."* By DR. GRAILY HEWITT. *London*, 1861.

IN this pamphlet Dr. Hewitt invites his obstetrical brethren to re-examine the foundations of that good old rule which tells us to support the perinæum in labour. The author goes through his task with considerable method and fulness of inquiry, but, as it appears to us, with rather stronger prejudice in favour of his own conceptions, opposed as they are to those of the mass of obstetric practitioners. For example, he observes (p. 39): "How it came to be imagined that pressing the fourchette forwards was an advantage it is not easy to explain; and the very fact that so many recent writers appear to agree in considering it an advantage shows how little the matter has been really studied by them." This is certainly not complimentary to one's brethren. It is, moreover, suicidal. To argue that an author has not studied a subject, because he has taken the trouble to write down his ideas upon it, would apply to the writer of this pamphlet as well as to those whose opinions he denounces. Dr. Hewitt denies the fact that supporting the perinæum by pressing it forwards, at all tends to prevent laceration. Of this, every practitioner in midwifery has the means of judging by personal observation. A thin membrane, put upon the stretch by a spherical body, more easily tears, if pressure be exerted upon one side only. Apply an even concave pressure outside to oppose the convex distension from within —that is, "support the membrane," and you greatly lessen the risk of tearing. So it is with the perinæum, and even more distinctly. This structure is composed of a layer of mucous membrane and a layer of skin, having an abundance of very loose distensible connective and elastic tissue between. Now, if Dr. Hewitt will consider for a moment the effects of supporting and pressing forward the outer layer—the skin—he will see that by so doing, we draw upon the surrounding skin, and actually bring into aid of the stretched perinæum an additional amount of elastic membrane, which really enlarges the ring through which the head must pass. The skin glides forwards from without, just as the mucous membrane glides forwards from within, pushed by the advancing head.

Dr. Hewitt's conclusion is that, instead of supporting the perinæum, when this structure is rigid, we should incise it. Undoubtedly there may be cases where this procedure is called for; but experienced practitioners will probably, for some time to come, retain the opinion that in a far greater number of cases "support" is necessary, or at least desirable. That it affords real relief and a sense of security to the patient cannot be questioned, and this is a presumptive indication in its favour not to be despised.

We are amongst those who appreciate highly the services of those who occasionally disturb rooted doctrines, calling up rules that seem settled by custom and authority for re-investigation. In the opusculum before us, Dr. Hewitt has rendered this useful service.

ART. VIII.—*Household Medicine; containing a familiar Description of Diseases, their Nature, Causes, Symptoms, Treatment, &c. &c., adapted for Family Use.* By JOHN GARDNER, M.D. of the Royal College of Physicians, Edinburgh.—*London*, 1861. pp. 520.

WE have no intention of following the author of this work in the various proofs which he adduces of the advisability of making the public more or less acquainted with medical facts and opinions. Some reasons assigned are, we concede, of considerable weight; but we must ever remember that a spurious acquaintance with disease is apt to beget a flattering self-reliance which may, when too late, discover that the time has already passed at which the services of the regular medical attendant might have been rendered available.

If there is a demand for such works, however, the better they are of their kind, at any rate, the less injurious they will prove. Dr. Gardner has apparently attempted with success to put sound teaching into a popular shape, and whether we agree with him or not as to the necessity of certain medical reforms which he would initiate, we willingly allow that his work is very readable and easily to be understood, and contains much valuable information.

ART. IX.—*Observations in Clinical Surgery.* By JAMES SYME, Professor of Clinical Surgery in the University of Edinburgh.—*Edinburgh*, 1861.

ANY contribution to surgical literature by a person holding the eminent position which is universally accorded to Mr. Syme, must be an acquisition to the whole profession; but the present volume has the additional recommendation that besides many other most interesting questions, which want of space only compels us to pass over on the present occasion, it illustrates more distinctly than any previous production that startling proposition which Mr. Syme recently announced at the Royal Medical and Chirurgical Society of London,* viz., that the old operation for aneurysm might with propriety be substituted for the Hunterian method in many cases in which the latter had been assumed to be the only justifiable operative proceeding. Mr. Syme's present volume contains, besides the account of the case related in his paper in the 'Medico-Chirurgical Transactions,' short notes of three other cases in which he has performed the old operation for aneurysm with success. Of these four operations two were performed in the axilla, one in the buttock, and one at the root of the neck—the latter probably the boldest and most successful operation which has been performed in modern times. All these operations (like all those which Mr. Syme has given to the public in this volume) were completely successful, and this remarkable success is one of the strongest features in Mr. Syme's book. Great as his dexterity in operating is known to be, and sound as we may allow his judgment, Mr. Syme is but human, and cannot therefore always succeed; nor does he distinctly assert that he never operates without a cure, or undertakes any course of treatment which does not turn out as he wished. Yet from reading the present book a tyro would be tempted to conclude that cutting for the stone, operating for aneurysm, splitting open the perinæum, and such like things, are mere trifles—made dangerous by the bungling of surgeons in other places, but quite innocuous when performed in Edinburgh. We are quite sure that this is not the fact, and that Mr. Syme does not mean to represent it as being so. He has done and said enough to convince the whole world that he is one of the most expert and most successful surgeons in it; and if now he desires to render his great experience useful for the purpose of showing the probable dangers of opposite courses of treatment, he will forgive his critics (who claim also to be reckoned among the number of his hearty admirers) for remarking that this must be done, not by dwelling exclusively on the successful cases, but by stating fairly and frankly the result of *all* the cases. If the necessary materials have not been preserved, we have nothing to say against accounts, such as this, of cases which have turned out luckily; but let us remember that they give us, not a judicial summary, but an *ex parte* statement only—a statement, however, of the highest interest, and which no one can read without both pleasure and profit. In particular, we would commend our younger readers to ponder carefully the paragraphs in which so bold an innovator—as Mr. Syme can be on proper occasion—expresses the contempt which he feels for the various so-called improvements by which some of our oldest, best, and, above all, simplest operative procedures are now made complicated and often abortive. It is in hydrocele and lithotomy that Mr. Syme especially notices this tendency, but he might have made similar observations in many other departments of surgery with equal truth. "The fiddlefaddle instructions," he says, "that emanate from some schools of the present day, not only for using but even for holding a knife, sufficiently denote the poverty of intellect whence they proceed, and the lowness of aspiration to which they are addressed." If there was nothing else in the book, the vigorous defence which he has given of the old lateral operation of Cheselden, and his decided opinion that "the various proposals which have been suggested for its improvement are so many steps in a wrong direction," would suffice, in our judgment, to render it worthy the serious notice of every practical surgeon. But the few points we have glanced at can only be taken as a sample of a book which ought to be read and re-read by every operating surgeon, and which only the pressure upon our space prevents us from noticing at such a length as its importance would justify.

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* See Medico-Chirurgical Transactions, vol. xliii.

ART. X.—*On Food.* By EDWIN LANKESTER, M.D., F.R.S. *London*, 1861. pp. 385.

THE half of this volume comprises lectures delivered by the author at the South Kensington Museum: the remainder consists of notes prepared for lectures at the same institution, and afterwards extended into the present publication, but not orally delivered, in consequence of the course having been discontinued. The public rather than the profession is addressed, but as the author informs us in his preface that the lectures contain also the substance of his course on Materia Medica and Therapeutics at the St. George's School of Medicine, and of a course on the Vegetable Kingdom in relation to the Life of Man, delivered at the Royal Institution, they may be considered to possess higher pretensions than those of a merely popular treatise. That such, indeed, is the case, we most readily allow, as they will not only instruct the unprofessional reader, but will also not fail to interest the accomplished practitioner. On those ever debateable topics, alcohol and tobacco, their uses and abuses, Dr. Lankester has written chapters remarkable for their moderation and good sense. Amidst so much that is absurd which we are at the present time asked to believe, it is pleasant to meet with an author who, carefully treading the maze of chemical, physiological, and other perplexities with which these subjects have been fenced around, can lead his readers to a clear and safe conclusion on the matter.

ART. XI.—*The Diseases of the Prostate; their Pathology and Treatment.* By HENRY THOMPSON, F.R.C.S.—*London*, 1861. pp. 364.

WE can cordially commend this volume to our readers as a most valuable practical and scientific treatise. The profession is mainly indebted to the institution of the Jacksonian Prize of the Royal College of Surgeons for the possession of this work. In 1860 that prize was awarded to Mr. Thompson for an essay on "The Healthy and Morbid Anatomy of the Prostate Gland." From this has resulted the present comprehensive treatise. As Mr. Thompson's writings on the diseases of the prostate have been previously well known, we shall, we believe, best fulfil our obligations of regard to the interests of our readers, and of justice to the author, if we briefly mention those points which Mr. Thompson has more especially advanced by his own investigations. These are, Firstly, under the head of the anatomy of the prostate, the assignment of the "third" or "middle lobe" to the abnormal history of the organ. After having carefully prosecuted numerous dissections, Mr. Thompson cannot find in healthy bodies below fifty years of age any formation in the situation described capable of being recognised as a third or middle lobe. There exists, the author states, in the middle line, a median or commissural portion which possesses a specific character, and contains a large proportion of glandular structure disposing it to enlargement. Secondly, the analogy between enlargements and tumours of the prostate and those of the uterus. Thirdly, the causes and effects of enlarged prostate, with its treatment, and complication with stone in the bladder. Fourthly, researches in relation to malignant and tubercular disease of the prostate. Fifthly, the consideration of "the bar at the neck of the bladder." We must not omit to notice the excellent illustrations that accompany this volume.

ART. XII.—*Principles of Forensic Medicine.* By W. A. GUY, M.B. Cantab., F.R.C.P., Professor of Forensic Medicine and Physician to King's College, Medical Superintendent of Millbank Prison. Second Edition.—*London,* 1861. pp. 534.

THOSE who were conversant with the first edition of this work—and they were many, besides the class for which it was immediately written—will find the present one superior in several respects. The arrangement remains unaltered, but many parts which permitted it have been condensed, and references and illustrative cases have been shortened; thus room has been left for considerable expansion in various directions, and for the addition of many valuable woodcuts and microscopical designs. Most alteration will be found in the department devoted to toxicology; and especially is this the case in connexion with the subject of "TESTS." The reader will observe that the subject-matter of a paper by the author, on the "Production and Identification of Crystals of Arsenious Acid and Crusts of Metallic Arsenic," which appeared in Vol. I. of Beale's 'Archives of Medicine,' and which we remember to have read with much interest, is rendered available for the purposes of the general work. In connexion with this volume by Dr. Guy, we would call the attention of the Profession to two papers by the same author; these are reprints—one, on the Microscopical Characters of the Crystals of Arsenious Acid, from the 'Transactions of the Microscopical Society;' the other (being the substance of part of the Croonian Lectures at the Royal College of Physicians for 1861) from the 'Pharmaceutical Journal.' The latter is a paper on the Colour Tests for Strychnia and the Diagnosis of the Alkaloïds, and is chiefly devoted to the determination of the four following queries:—1st. The best form and mode of application of the colour-test. 2nd. Of the colour-tests, which is to be preferred? 3rd. Are the colour-tests, or is the selected test, open to any serious objection? And 4th. Is it possible, by means of the colour-tests, or by any suitable modification of them, to distinguish the alkaloïds from each other? Two elaborate Tables, showing the comportment of the numerous alkaloïds with various re-agents, are appended in reference to this last question.*

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* We understand that it has been Dr. Guy's practice for some time to illustrate his lectures on Forensic Medicine by microscopical preparations of strychnia, arsenic, &c. &c., collected, many of them by volatilization, in exceedingly minute hermetically-sealed tubes; and passed round to his class, accompanied by a hand-microscope, which he has had constructed for the purpose.

PART THIRD.

Original Communications.

Art. I.

On Marriages of Consanguinity. By Gilbert W. Child, M.D. Oxon, M.R.C.P.; Physician to the Radcliffe Infirmary, Oxford, and Consulting Physician to the Warneford Lunatic Asylum.

It is in a time pre-eminently distinguished for scientific progress, like the present, that we have most need to review the reasoning upon which accepted conclusions are based, lest in using the latter as grounds upon which to advance our knowledge, we should be but misleading ourselves and others. The history of science, and, most of all, of medical and hygienic science, abounds with instances in which progress has been delayed by mistaking hypothesis for fact, and building a theory upon an unwarranted assumption. When something of this kind has been done, when an opinion, however ill-founded, has once become accredited or orthodox, it is wont to form part, as it were, of the very minds of those to whom it has been taught, and a stronghold of prejudice is thus built up which it is most difficult to overthrow, but which must be overthrown before men's minds are in a condition fairly to estimate the evidence in favour of any other view of the subject-matter.

This reflection is forcibly suggested by a consideration of the prevailing opinions upon the subject which gives its title to the present paper, "Marriages of Consanguinity," together with the grounds which appear to have given rise to it.

In writings* upon medical and hygienic subjects we constantly find the marriages of blood relations enumerated, as a matter of course, among the causes of degeneration of race, sterility, insanity, scrofula, &c., exactly as if it had been ascertained by the most careful research that they really are so; and the results of breeding in-and-in, as it is called, in the case of domesticated animals, are referred to in proof of this, as if the two cases were really analogous. In the class of writings to which I refer, it is assumed that such marriages are contrary to some "law of nature," and that the evils charged upon them occur in the way of natural consequence; but no attempt has been made, so far as I am aware, to explain what this mysterious law of nature is, what are the limits within which it acts, or what is its *modus operandi.* We are constantly, in letters and articles in medical periodicals, threatened with the grievous consequences of the breach of this law, and reminded that it is the duty of physicians to set their faces against such marriages on all occasions, and sometimes edified with some statistics by way of warning from example; but the same fault of loose reasoning runs through all the writing on the subject which has come under my observation.

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* See for instances, Lancet, July 7th, 1860; Medical Times, April 27th, 1861.

In the cursory and almost incidental allusions to this matter of which I have hitherto spoken, contained as they are in letters, papers on general hygiene, and other allied subjects in medical periodicals, it is not fair certainly to look for any systematic account of it. Such papers are, however, useful for my present purpose, inasmuch as they serve to show the general state of opinion upon the subject among those under whose notice it comes only in the general course of their professional reading and experience. It is to those who write specially upon it that we must go for a more particular account of the question, and I shall therefore, in the course of this paper, refer especially to an article upon marriages of consanguinity by an American physician, Dr. Bemiss, of Louisville, which was reprinted in the 'Journal of Psychological Medicine' for April, 1857, p. 368.

Before proceeding further, however, it will be well to state clearly what are the objects which I propose in writing the present paper. I desire, then, not so much to give a positive opinion as to the hygienic effects of marriages of consanguinity, as to examine the facts which are known about such marriages, and to endeavour to show clearly what is the value of these facts, how far they can be explained by known laws, and whether when this is done any residue will remain which requires to be accounted for by the assumption of some special law of nature which these marriages transgress; and, finally, whether an impartial examination of facts, such as is now proposed, will be found to justify the unqualified condemnation of such marriages commonly pronounced by medical men.

I. It is worth while, in the first place, before proceeding to other considerations which more immediately concern us, to notice the account given us of some marriages of the kind under discussion in the historical books of the Old Testament, and I may remark that the question here is a purely historical one, and touches in no way upon theological grounds. We find there that Abraham's wife was his half-sister;* that his son Isaac married his first cousin, once removed, and his grandson Jacob again his first cousin. Certainly, if the ordinary opinion of the effects of close intermarriage were correct, the twelve patriarchs, of whom several were the offspring of the third successive generation of such marriages—the first of them far closer than would be possible now—ought to have reached a very considerable degree of degeneracy; yet surely he would be a bold man who should assert his belief that such was really the case, and indeed we have every reason to believe the contrary, for we find them in fourteen generations multiplying into a nation of six hundred thousand fighting men, or, on a fair computation, some ten million persons. Dr. Bemiss, in the paper above referred to, notices these cases as apparently antagonistic to the notion of degeneration of race as resulting from close intermarriage, and proposes two methods of evading the difficulty—viz., either by supposing that, "as the Jews were a people chosen for an especial purpose, they existed under abnormal conditions;" or by presuming that the whole organization of man in the patriarchal times was so superior to our own, that the natural law of degeneration was inoperative in their case, or operated only to the extent of gradually diminishing the term of men's lives down to that at which it is now fixed. To such hypotheses as these, the best answer will be found in a remark upon Berkeley's system of idealism, attributed to Hume: "It admits," he said, "of no refutation, and produces no conviction." So it is with these hypotheses. They cannot, indeed, be disproved; but it is surely contrary to all sound philosophy thus to assume an unknown and unaccountable cause of a phenomenon, having already assumed the phenomenon itself in order to afford support to a theory.

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* See Genesis, ch. xx. v. 12; xxiv. v. 15; xxix.

Again, the law of Moses, which is on all hands admitted to have provided with special care for the physical well-being of the Jewish people,* contained a provision that all heiresses should marry within the tribe to which they themselves belonged. This was a powerful, though indirect, incentive to the marriage of blood relations; and it is noticeable that in the very case which gave occasion to the enactment of the law,† four heiresses are mentioned as all marrying first cousins. Had such marriages been naturally productive of the ill effects commonly attributed to them, it is hardly conceivable that this law would have been enacted at all.

But to this part of the subject I do not wish to attach much importance.

II. I proceed, in the next place, to examine the supposed analogy between marriages of consanguinity in mankind and the breeding in-and-in of the domesticated animals; and here I believe that I shall be able to show

(1.) That there is no real analogy between the two cases.

(2.) That the results of in-and-in breeding are not so disastrous as they are commonly supposed to be.

(3.) That its results are such that we may learn from them in what cases the marriages of blood-relations are likely to produce ill effects, and in what they are not.

Perhaps the best way of illustrating this portion of my subject will be to lay before my readers the pedigrees of some well-known animals, and I will choose short-horned cattle as being, probably (with race-horses, to which I shall also allude), the kind of domesticated animals which have been bred with the greatest care, and whose pedigrees have been most accurately kept. Any one acquainted merely with the current opinion of the day, which takes exception in general terms to close breeding, will, I think, be astonished, as I was, when, by examining the 'Herd-book' for himself, he learns what close breeding really means. I will only give two or three examples; but any one who cares to do so can multiply them almost indefinitely for himself by the help of the above work.[*] "Comet," one of the most celebrated of the earlier bred short-horned bulls, and the progenitor of many of the best existing stock, was bred as follows:

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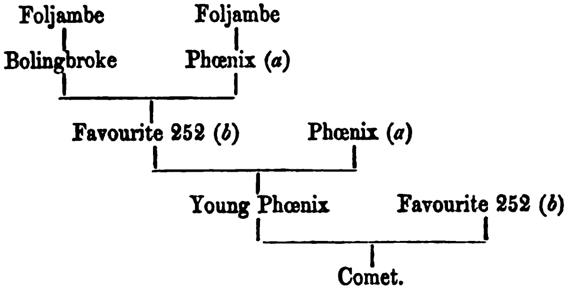
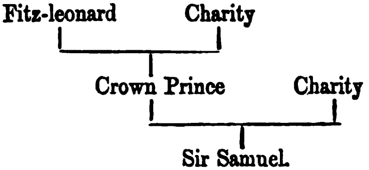
* Numbers, ch. xxxvi. ver. 5.

† Ibid., ver. 10.



The letters *a* and *b* are appended to show where the same animal recurs.

Again, a celebrated cow, called[†] "Barmton," has a pedigree in which the same bull, "Favourite," 252, appears as the sire of four successive generations. Finally, to give a more modern instance, a bull, called[‡] "Sir Samuel," bred as lately as 1855, has the following pedigree:—



Such instances might, I think, almost suffice for my purpose, but I have not been satisfied with merely taking them from the published pedigrees in the 'Herd-book,' but have taken advantage of an opportunity which offered itself of discussing the matter personally with one of the most intelligent and successful breeders of short-horned cattle in the kingdom. This gentleman, I should add, is himself strongly opposed to close breeding, his evidence therefore, so far as it tells in its favour, has the more value, as being the admission of an opponent, rather than the *ex-parte* statement of an advocate. He informs me that there is a well-known breeder of the present day who has had only three distinct crosses in the last twenty years, the results of which appear to be as follows:—

(1.) He still has very finely-formed cattle.

(2.) His bulls are highly prized as crosses by breeders having a different stock.

(3.) His herd generally are good grazers and carry flesh well, and the animals are individually healthy, but there appears to be a diminution of fertility, and this seems to be on the side of the males chiefly.

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[*] See Coates's Herd-book, vols. i. ii. iii. (Bulls) p. 25, ed. 1846.

[†] Ibid., vols. i. ii. iii. (Cows) p. 36, ed. 1847.

[‡] Ibid., vol. xii. p. 216, 1858.

In another case, a breeder had succeeded some years ago, chiefly by means of land affording very luxuriant feed, in rearing one or two animals distinguished especially by their large size. He, too, bred in-and-in, and though the progeny did not appear to deteriorate in other respects, they lost value by becoming less perfect in shape, while still enormously large. My informant further told me, that in the breeding of stock the degrees of consanguinity which would be the closest possible among mankind, are simply not looked upon as in-and-in breeding at all.* Similar instances of close breeding to the above may be found in the case of race-horses, but it is unnecessary for my present purpose to bring them forward here. I may remark, however, that the celebrated "Flying Childers" was, on the side of his dam, at any rate, very closely bred.

Before proceeding to draw any conclusions from this portion of my subject, I wish to remind my readers of the following considerations :—

(1.) That man is, in the strictest sense of the word, physiologically an animal, and in no part of his nature is he more strictly an animal than in the function of reproduction, and that, consequently, conclusions derived from the study of this function in animals may, due regard being had to difference of circumstances, be properly applied to man.

(2.) That every individual of a species has some peculiarities, and that every individual peculiarity is, so far, a departure from the ideal standard of the race.

(3.) That since like produces like, the closer animals are bred the more will individual peculiarities be strengthened and developed.

(4.) That such individual peculiarities, however harmless in the first instance, will generally, if increased and developed beyond a certain limit, become so great a departure from the original type as to constitute a positive defect.

Now bearing in mind these obvious and generally admitted principles, we may, I think, from the above evidence, draw the following conclusions :—

(1.) That close breeding is not, *per se*, contrary to any "law of nature."

(2.) That, as might be expected *à priori*, it has a tendency to intensify individual peculiarities, and where these are morbidly developed, may then lead to degeneration of race.

(3.) That unless parents are themselves diseased, close breeding does not tend to develope disease in their progeny.

(4.) That where very close and continued through many generations, close breeding has a tendency to diminish fertility, and seems to do so by lessening the generative power of the male sex.

It is, perhaps, worthy of remark, that writers upon the subject who have spoken of close breeding as contrary to nature, have overlooked the fact, that while proof is necessarily wanting of what happens in this matter to animals in the wild state, there is at least a probability that close breeding takes place to some extent in many of the gregarious tribes. In any application of the analogy of breeding in the lower animals to consanguineous marriages among mankind, we must bear in mind—

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* See Observations on Breeding for the Turf, &c., by Nicolas Hanckey Smith. London, 1825; also, Stud-book, for pedigrees.

(*a*) That the risk of ill consequence in the former, as compared with the latter, is immensely lessened by the power we possess of selecting healthy stock to breed from; but

(*b*) The degree of in-and-in breeding frequently practised in the case of animals, is so much closer than among mankind as to destroy all analogy between the two cases. No breeder of cattle would speak of the offspring of a cow—say with her sire's brother—as closely bred at all.

III. I pass now in my investigation from the case of animals to that of man, but in doing so, it is worth while to try and estimate the relative value of the two methods for the purpose in hand, when I think it will appear that the theory of the matter may be best learned from the consideration of animals, but that the principles so acquired may require to be checked in their application by reference to the experience gained by observations upon mankind. In the case of such animals as are carefully bred, we are able to govern their circumstances much more absolutely than we can those of men; we know the birth, parentage, and constitution of the parents, can avoid all kinds of cachexia, and can correct the defects of the one parent by means of opposite good qualities in the other: indeed, it is the power of arranging these details well which makes the difference between the skilful and unskilful breeder, and which brings success to the one and failure upon the other; and when the offspring is produced, we are able to place it in the circumstances most favourable to its future development and improvement. Moreover, the market value of good animals with pedigrees is such as to induce a large number of intelligent men to employ their time and capital in the business of breeding them. Thus in our breeds of horses and cattle, and in their pedigrees, we have a series of careful experiments carefully recorded, and such, therefore, as ought to give to conclusions legitimately derived from them, a high degree of scientific value; on the other hand, it is hardly possible to exaggerate the difficulties in the way of arriving at any satisfactory results where mankind are the subject of investigation. Isolated instances are utterly valueless, experiment is manifestly impossible, and the only method remaining open to us is that of a careful and laborious collection of statistics. Statistics, however, on such a point as this, are very difficult to collect, and very worthless when collected.

Suppose a man to obtain, as Dr. Bemiss, in the paper above referred to, has done, the particulars of some 53 cases of intermarriage of cousins, and to argue from the condition of the offspring of these; what is the value of his conclusions from such data? To say nothing of the actual smallness of the number of cases, and of the doubtful nature of the ill effects shown to have followed in some of them, of which I shall have to speak again, let us only consider the difficulty or rather the impossibility of arriving at any satisfactory knowledge of the previous history of the parents in all these cases. How many family histories and secrets have to be unravelled which it is the interest or supposed interest of every one concerned to hide and suppress, or which, when related, as they sometimes are, to the medical man, are coloured and garbled intentionally or otherwise by the imperfect memory or keenly interested feelings of the narrator? How often, again, does a taint of syphilis or insanity exist in a family of which many of its members are kept in entire ignorance? These are but a few of the innumerable obstacles which stand in the way of any one trying to investigate a matter of this kind by the method of statistics in the case of human beings, for even should we accept the somewhat questionable theory that errors in statistics correct themselves, it could hardly help us out in a case like this, where the inducements to suppression and misrepresentation are such as to leave the errors all on one side. That Dr. Bemiss has not altogether overlooked the difficulties which beset his task, is evident from the following passage, which occurs in the early part of his paper. He says:

> "Reference may be found to the unfortunate influence of marriages of consanguinity upon offspring in various medical works of the previous and present century, but *no facts* are adduced to support the conclusions of the authors, nor have any statistics illustrating their effects been presented to the profession, so far as I am aware, except some facts included in Dr. Howe's valuable reports on idiocy."* [The italics are my own.]

After this statement, I may fairly take Dr. Bemiss' paper as an example of the grounds upon which the marriages of which he speaks are to be condemned, and should I be able to show that even this does not represent such marriages as so mischievous in their effects as they are commonly supposed to be, I shall have succeeded, to some extent at least, in the destructive as well as the constructive portion of my argument.

Dr. Bemiss' statistics may be divided into two sets—viz., those which concern thirty-four marriages the particulars of which were collected by himself, and seventeen others mentioned in Dr. Howe's report. Of these the results are as follows, looking first at Dr. Bemiss' cases:

28 between first cousins	7 sterile.
6 between second cousins	27 fruitful.
—	—
34	34†

Total number of children 192, giving an average of 5·6 to each marriage, and of 7·1 to each fertile marriage.

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* An article in the Lancet of Dec. 22nd, 1860, upon the same subject, quotes the facts from Dr. Bemiss' paper, but adds no others of more recent date. The only paper of any kind I have yet seen written from the same point of view as my own, is a short letter in answer to the above article, published also in the Lancet of Feb. 22nd, 1861, by Mr. Anderson Smith.

† The average of births to each marriage in England is, or lately was, about 4·51 to 1. See article Population in the Encyclopædia Britannica.

Of 192 children, 58 died in early life, and the remaining 134 reached maturity.

Causes of the early deaths: 15 consumption, 8 spasmodic diseases, 1 hydrocephalus.

Of the 134 adults, 46 are returned as "healthy;" 32 "deteriorated, but without absolute indications of disease;" 9 are unaccounted for; and 47 stated to be diseased.

The following is the classification of the cases of disease: 23 were scrofulous, 4 epileptic, 2 insane, 2 mute, 4 idiotic, 2 blind, 2 deformed, 5 albinoes, 6 affected with defective vision, 1 choreic; giving a total of 51; some, therefore, must have suffered from two or more of the above diseases.

Upon examining these figures, no one can fail to be struck by the fact that while the fertility of the marriages is great, the proportion of deaths occurring in the offspring before "maturity" is below that stated by Dr. West as the average mortality under the age of five years. It is to be noted also that, as shown in the earlier part of this paper, the first, if not the only indication of degeneration as a result of close breeding among animals, is a diminution of fertility.

The number returned as scrofulous or dying from consumption, is certainly large, but is partly accounted for by the fact mentioned by Dr. Bemiss that in three of the families there was reason to believe that a scrofulous taint already existed, and that these alone supply sixteen of the cases. Again, the phrase "deteriorated, but without absolute indications of disease," seems to imply that the writer has dealt out rather hard measure to the objects of his investigation, more especially when considered in relation to the very slight ailments which are enumerated amongst the cases of disease; such as albinoïsm, defective vision, and chorea, which together form nearly one-fourth of the whole number. Albinoïsm is rather a peculiarity than a disease, shortsight is too common and too slight a defect to be reckoned a mark of degeneration, and chorea is in most cases a transient and curable complaint. All three, moreover, are remarkable for their hereditary character; but Dr. Bemiss does not mention the previous condition of the families in these particulars.

A fair consideration of these points, and a consequent re-construction of the statistics, will leave the results pretty much as follows:

Of fair average health	86
Unaccounted for	9
Diseased	39
	134

Thus giving us a total of 39 diseased persons in 134, or in round numbers, 2 in every 5, and this when there is an admittedly unusual amount of scrofula hereditary in some of the families.

I pass on to Dr. Howe's 17 cases, with which Dr. Bemiss fortifies his opinion; the particulars are as follows:

17 marriages produced 95 children, i.e., 5·58 each.
Of 95 children,
37 were of tolerable health.
1 was deaf.
1 was a dwarf.
12 were scrofulous and puny.
44 were idiots.
Total . 95

Of these it may be enough to say that they manifestly prove too much. To say that all but half the children of the marriages of cousins are idiotic, is simply to say that the cases from which the statistics were drawn up are not fair cases. Marriages of cousins are not so infrequent, either in this country or elsewhere, that such monstrous results, if they really occur, could escape observation. In this respect, too, the two sets of statistics contrast remarkably with each other; for while we have in Dr. Bemiss' 192 cases, but 4 idiots, or rather more than 2 per cent., in Dr. Howe's 95 there appear 44 idiots, or more than 46 per cent. If the idiocy in both cases is due to the consanguinity of the parents, why are the results so disproportionate?

I have thus far endeavoured to show that the facts with which we are acquainted bearing upon the subject of the effects of consanguineous marriages are not of a character to afford support to the general opinion that such marriages are in themselves contrary to some law of nature, and calculated to lead to degeneration of race. I will proceed now to explain in what way I conceive that such an opinion may have arisen, and under what circumstances a superficial view of facts may have tended to support it.

It should be remembered, then, that all such marriages as those under discussion were and are strictly prohibited in the Church of Rome. This prohibition was first removed in England by the Marriage Act of 1540 in the reign of Henry VIII. It is natural, therefore, that many people at the time should have looked upon this removal of restrictions as a somewhat questionable concession to human weakness, and upon the marriages made in consequence of it, as merely not illegal, rather than in themselves unobjectionable; just as, should the Marriage Law Amendment Bill pass into law, there can be no doubt that many would now look upon marriage with a sister-in-law as a very questionable proceeding in a social and religious point of view, although they might possibly be unable to impugn its strict legality. Under such circumstances, nothing is more natural, especially in an age when men were much more open to theological than physiological considerations, than that they should attribute any ill effects which might seem to follow from such unions to the special intervention of Providence. Such ill effects would be marked and noticed whenever they occurred, and would soon become proverbial; and when, in a later age, men began to pay more attention to the breeding of animals and found that excessively close breeding seemed in some cases to produce similar results, they would be led to establish a false analogy between the two cases, and to infer the existence of a law of nature which close breeding and consanguineous marriages equally infringed.

Something like this I conceive to be the true history of the common opinion upon this subject; an opinion which, as far as I can discover, rests on no satisfactory record of observed facts.

It remains now only to state what I believe to be the natural results of the marriage of blood relations, and to show under what conditions it is likely to be mischievous, and under what harmless, or even beneficial; and this I shall best do by supposing a case by way of example. Let us suppose a grandfather, A, who is affected with some form of scrofula. The scrofulous diathesis descends to his two sons, B and C. B has two sons, D and E. C has a daughter, F, the latter of course first cousin to D and E. D marries into another family, unconnected in blood, and free from any scrofulous taint; E marries his cousin, F. In this case, clearly, the chance of D's children being healthy is infinitely greater than that of E's; but the reason is, not because E married his cousin, but because he married one in whom the same cachexia was latent, as in himself, and latent, too, in a constitution, probably, in many respects the counterpart of his own. Here, too, is a case in which the analogy of the lower animals does in its degree apply; for, just as breeders expect and very often succeed, by close interbreeding, in reproducing a great variety of the peculiarities of a single individual, so in the case I have supposed we shall be likely to get, not only the tendency to the same disease, but likewise the same general constitution of mind and body—the same or similar idiosyncrasies.

Let us now suppose, on the other hand, the grandfather and his two sons, and their respective wives, to have been free from any such taint of cachexia, and that we have the two grandsons, D and E, and the granddaughter and cousin, F, all sharing this normal and healthy constitution. Now, if D marry into a scrofulous family, and if E, as before, marry his first cousin, F, there can, I think, be no doubt that the chances of healthy offspring will be almost the reverse of what they were in the former case. There will be every reason to expect the children of the cousins to be healthy, though they will no doubt present family peculiarities strongly developed; while there will be great danger of scrofula appearing in the offspring of D, albeit he has not married a blood relation. Still the risk run by D, in the second case, is much less than that incurred by the cousins in the first.

These two cases, as I have put them, are no doubt strongly marked; and we should hardly expect to meet with the exact counterpart of either of them in actual practice; but to any one familiar with disease, and observant of family resemblances, a variety of modifications of them will at once occur, and cases more or less like them are met with every day. They serve to illustrate the following propositions, with which I conclude my paper.

I. That the marriages of blood relations have no tendency, *per-se*, to produce degeneration of race.

II. That they have a tendency to strengthen and develope in the offspring, individual peculiarities of the parents, both mental and physical, whether morbid or otherwise; and therefore in practice they often do induce degeneration.

III. That there are some cases in which it would be actually safer (as far as the chance of healthy offspring is concerned) for a man to marry a blood relation, than a woman not so related with whose family history he was unacquainted.

IV. That by means of a proper regard to known facts relating to hereditary transmission, a physician may predict with great accuracy the probable result, as regards the health of the offspring, of a marriage of blood relations in any particular case, if only he be sufficiently acquainted with the hygienic history of the family.

Hence in those cases in which a physician is consulted on such a matter, it is his duty, not, as some have asserted, to save himself all trouble by denouncing the match, but to discover as far as possible the conditions of the particular case, and to give his opinion according to the results of his inquiry.

ART. II.

Observations on Yellow Fever. By ROBERT LAWSON, Deputy Inspector-General of Hospitals.

Division I.—Influence on the Secretions.

HAVING been stationed in Jamaica from September, 1856, till June, 1860, I was enabled to examine some of the peculiarities of yellow fever as it occurred there during that period. Part of the results have appeared already in the pages of the 'British and Foreign Medico-Chirurgical Review;' the present communication embraces observations on the secretions during, and the morbid appearances left by, the disease, which are of importance both in practice and in giving more precise notions as to its nature. Many of the subjects in the following remarks were mentioned by Blair in his papers on the Yellow Fever at Demerara; others have been alluded to by La Roche, in his work on 'Yellow Fever;' but I have endeavoured to push the observations further, and to trace the connexion between the different phases of the disease more fully than either, or than I have seen elsewhere.

Urine.—The liability to suppression of urine in yellow fever, and the serious consequences resulting therefrom, have long been known; but it is only of late years that the composition of the secretion has been receiving attention, and as yet it is far from having obtained what its importance demands. In fact, men in practice have their time so fully occupied, especially during the prevalence of an epidemic, that anything beyond the most simple examination is out of their power.

The urine during the first days of yellow fever presents the ordinary characters seen in febrile affections. Its quantity is rather less than natural; its colour somewhat higher, though clear, and of moderate specific gravity. From the third to the fifth day of the disease, the quantity is often diminished to fifteen, or even twelve ounces, or less, in the twenty-four hours; its colour from six to seven of Vogel's scale, and specific gravity from 1018 to 1030 at 60°; it continues acid, and presents more or less sediment. Should the patient survive the fifth day, the quantity generally increases and often becomes copious—fifty, sixty, or even eighty ounces being passed in twenty-four hours. With this increase the colour becomes lighter, unless when obscured by blood or bile, and the specific gravity less; and as the patient convalesces, the secretion gradually assumes its usual appearance.

Suppression of urine is most common from the fourth to the sixth day of the disease; but this period may pass, the flow become copious, and yet suppression occur many days later. At either period the result is almost universally fatal.

A cloudiness appears in the urine on the morning of the fourth day of the disease; and if a specimen, obtained at this time, be allowed to stand for an hour or two in a cylindrical vessel, a sediment will subside, frequently amounting to one-fourth the bulk of the fluid, or even more. On examination with the microscope, this is found to be composed almost exclusively of scaly epithelium from the bladder. On the morning of the fifth day, an equally copious deposit will occur, but differing from that of the previous day in being composed of granular tube casts from the kidneys, frequently with but a trace of scaly epithelium. The solid transparent casts called waxy are not uncommon at this period. After the sixth day the casts become gradually more hyaline, the quantity diminishes, and in a few days thereafter they nearly disappear. When it was possible to fix the date of the first accession of the fever, the desquamation of the bladder seemed fairly developed on the morning of the fourth day; while the casts from the kidneys, though probably formed at the same time, were not copious in the urine before the morning of the fifth day. It is possible these might occur earlier in some cases, but I have not met with any in which this was ascertained satisfactorily. In cases of mixed typhoïd fever, presenting on post-mortem examination the distinct raised ulceration of Peyer's glands in the ileum, and which terminated with suppression of urine, albumen and granular tube-casts were found in it, but these occurred longer after the first accession of the disease than in pure yellow fever.

The casts are usually $1^{t}.0$ to $1^{t}.3$* in diameter. They are highly granular from the first, with few traces of the outline of the original epithelium. The solid waxy casts are of the same sizes; if these be illuminated with a pencil of small angle, and viewed with an object-glass of large angle of aperture, epithelium more or less granular can always be seen in them. The substance connecting the granular and epithelial matter in both descriptions of casts is soluble in caustic potash; acetic acid dissolves it in the granular casts, but seems not to affect the waxy. These reactions induce me to believe the waxy part of the casts is casein, which, it will be shown below, is common in the urine in yellow fever.

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* In this method of expressing microscopic measurements, the thousandth of an English inch is taken as the unit, and its fractional parts are given decimally, which offers far greater facilities for comparison, and for conversion into foreign measures, and *vice versâ*, than the method by vulgar fractions usually employed in this country. See Beale's Archives of Medicine, No. 8 (April, 1861), p. 292.

It is rare to find a blood-globule in the tube-casts, even at the commencement, and many cases of the disease run their course without a single globule being detected in the urine. In others there may be hæmorrhage from the kidneys or urinary passages, in which case blood-globules are found in the secretion, or in the casts. A third series occurs in which the urine is of the colour of blood, without a globule being found in it; in these the epithelium and substance surrounding the granular matter of the casts are deeply impregnated with hæmatine, though not a single entire globule can be detected among them.

If the case proceed favourably, the casts diminish in number, and become less granular and more hyaline on the sixth or seventh day, and soon disappear altogether. If it run on to suppression of urine at this period, they maintain their granular character; but if the flow of urine be increased after the fourth or fifth day, and the suppression do not occur until a later period, the casts become less granular and less numerous until just before that event takes place.

The urine is frequently coloured deeply by bile. This may be distinguished from hæmatine by the brownish-yellow colour a thin stratum gives with transmitted light, while urine coloured by hæmatine always presents a blood red. The former gives a green colour with nitric acid, varying in depth from a light pea-green to a greenish black, and occasionally, when not too deep, the usual changes to the violet and red can be perceived; while with the latter this acid produces a coagulum, from a dirty brownish grey to a dark liver colour. The two, however, may exist in the same specimen, when the reaction will partake of both characters.

The discharge of blood, whether in the form of globules or of hæmatine, and that of bile, in the urine, usually occur from the fourth to the sixth day, and either may continue for several days thereafter. The excretion of bile in this way is always beneficial. Blood, if in the form of globules—constituting hæmorrhage, in short—is generally beneficial, from whatever organ it proceeds, if the flow be not so copious as to depress the vital powers too much, and with its appearance unpleasant symptoms are often dissipated. When originating in the kidneys, it seems to act as a local depletion, obviating that engorgement of these organs so liable to arise at this period, and favouring the flow of urine. It is very different when the discharge is in the form of hæmatine without globules; then it seems to be strictly a secretion, for the epithelium of the casts, and that in the convoluted tubes of the kidneys themselves, is deeply impregnated with it, while blood-globules are not to be seen either around the Malpighian bodies or inside the tubes. This form of discharge is often copious, always unmanageable, and almost of fatal import.

The colour of the urine has been stated already to be as high as from 6 to 7 of Vogel's scale, from the fourth to the sixth day. This arises from its ordinary colouring matter, the uræmatine. In addition, this secretion sometimes contains, for long periods, and in healthy persons as well as those labouring under disease, a large amount of uroxanthine, or its derivatives, urrhodine and uroglaucine. These do not deepen the colour, but as they seem connected with some peculiarities in the form of yellow fever, I notice them here.

The presence of urrhodine, or uroglaucine, may be determined by adding a few drops of urine cautiously to a drachm of hydrochloric acid in a small test-tube, when a colour will be developed where the fluids meet, varying from red through purple to blue, according to the predominance of either of these pigments, and more or less intense according to their quantity. Their presence is indicated in another way. If a portion of urine treated with nitric acid, in the usual manner for the detection of albumen, be set aside for some hours, if containing more than a trace of urrhodine it will become of a deep reddish-brown colour, while the tint of uræmatine is not altered materially by this process. While these pigments were present in small quantity only, fever seemed to have less of the epidemic character, though the cases which presented themselves displayed an earlier and more serious implication of the liver, and more intense jaundice, than when they were more general and more copious. Just previous to my leaving Jamaica, the nitric acid gave the deep colour pretty generally, after having been absent from the early part of 1859. Upon this I stated the probability that the following season would be unhealthy, which prediction has proved correct.

If urine when heated, or treated with nitric acid, present a coagulum, this is generally believed to be albumen; but it may contain other matters which coagulate when so tested, and in yellow fever it does so very commonly. Globuline is coagulated by heat, and casein by nitric acid, as well as albumen, and these are by no means infrequent in the urine of fever in Jamaica. The following table shows the reactions of these three substances with different tests, and affords the means for distinguishing them:

	Heat.	Nitric acid.	Acetic acid.	Heated with dilute solution of carb. soda.
Albumen .	Coagulates at 146° Fahr.	... Coagulates	... Unaffected	... Unaffected.
Globuline .	Coagulates at 200° Fahr.	... Coagulates	... Unaffected	... Dissolves.
Casein . .	Does not coagulate.	... Coagulates	... Coagulates	... Dissolves.

In examining urine it was, in nearly every instance, passed through a paper filter, to remove epithelium, tube casts, mucus, or other extraneous matters; portions were then placed in three test-tubes, one of which was heated, another treated with nitric acid, and the third with acetic acid in a similar manner. The indications at the time were noted, and again after twelve to twenty-four hours.

On heating a specimen cautiously, with the tube inclined, the fluid along its upper edge sometimes became opaline, and the appearance then spread rapidly through the whole before the temperature was sufficiently high to form steam. At other times small bubbles of steam were generated and passed up, nearly reaching the surface, before an opalescence appeared, indicating a much higher temperature of the fluid. The coagulation in the former case was from albumen, in the latter from globuline. Sometimes there was a slight coagulation at the lower temperature, and a much more copious one at the higher, indicating an excess of globuline, though no free blood-globules were seen in the urine. In common, however, the albumen so far exceeds the globuline that the latter cannot be detected in this manner; but it may be separated from the albumen by boiling alcohol, which I have done several times.

The nitric acid test was confirmatory of that by heat, and served to distinguish discoloration by blood from that by bile, as well as indicate the presence of urrhodine. Part of the specimen can be examined for chlorides, after the subsidence of the coagulum.

When acetic acid was added to a specimen containing casein, the colour became lighter, and more or less opaline, but in general subsidence did not take place to any extent for some hours. After twelve hours the precipitate had usually fallen, and constituted a fine amorphous, rather compact deposit, at the bottom of the tube. The supernatant fluid was then poured off, the precipitate washed and allowed to subside again, when the fluid was separated, and fresh water being added, a few drops of solution of carbonate of soda dissolved it at the temperature of the air (80° to 86° Fahr.); this observation has been repeated in so many instances, with the same result, as to leave no doubt concerning it.

Urine rich in urates gives a precipitate with acetic acid, if near the point of saturation, and at a low temperature, and the precipitate dissolves in carbonate of soda. This differs from casein in the precipitate forming immediately, and subsiding quickly; and, on decomposing the soda solution with an acid, by giving uric acid in a crystalline form, which can be recognised with the microscope. If a portion of the same urine be warmed, it will no longer give a precipitate on the addition of an acid, and if allowed to cool gradually the uric acid will be deposited in a crystalline form on the sides and at the bottom of the vessel. It was almost universally in the latter forms that it occurred in Jamaica.

I had seldom examined the soda solution for uric acid, but very frequently evaporated some drops of the secretion itself, acidulated with hydrochloric acid, on a slip of glass; and in the most decided instances of the presence of casein, did not find traces of uric acid, though hippuric was plentiful. The following case will illustrate this, as well as several other points noticed in this paper:

Henry Goodwin, a black soldier, aged about thirty, had a paroxysm of fever on the 10th August, 1859, but did not report himself sick. On the 12th, the third day of the disease, he went to hospital in the afternoon with a sharp attack of fever; the conjunctivæ were then deeply yellow. From this time till the morning of the fourth day he passed sixteen ounces urine, which was slightly albuminous. On the fifth day there was a remission; passed twenty-eight ounces urine. On the sixth day he continued much the same, the urine containing granular and epithelial tube casts in moderate quantity, with a little albumen and casein, very little urea, but much creatine and hippuric acid; no uric seen. The urine had not been examined microscopically before this day. On the seventh day fever ensued again; he had passed thirty ounces urine from the previous morning, of the same character as last. About noon it became bloody, giving the colour of venous blood by transmitted light. It was then strongly acid, specific gravity 1014 at 84°, did not deposit a sediment, but had a few flocculi of granular and epithelial tube-casts, and epithelium from the bladder, but not a blood-globule could be detected. As the fluid filtered with extreme slowness, part was heated without filtration; coagulation took place partially, some time before ebullition, but increased as that occurred, and a sediment of a reddish colour fell, leaving the supernatant fluid pretty clear. Some of the latter gave a deposit with acetic acid, which dissolved in dilute carbonate of soda. A portion of the urine was treated with acetic acid; coagulation was produced almost immediately, and after some hours a deposit of one-seventh of the bulk of the fluid took place, of a reddish colour, and over it a thin loose stratum of deeper red colour, leaving the supernatant fluid clear dark amber; both deposits dissolved with carbonate of soda. As on the previous day, hippuric acid and creatine were copious, but there was little urea, and no uric acid. On the morning of the eighth day the fever continued; forty-two ounces of urine had been passed from the previous noon, of darker colour than before. Early this morning gallic acid was commenced, in three-grain doses every fourth hour. A specimen of the urine passed this forenoon was darker in colour than that of the previous day; it contained several granular tube-casts, which were tinged red, but none contained blood-globules, neither were any free globules seen. A coagulum of one-third formed by heating; but with acetic acid it was, to-day, not more than one-thirtieth of the whole, the supernatant fluid remaining of a clear deep cherry red. On the morning of the ninth day the urine collected in the previous twenty-four hours amounted to eighty ounces, of similar character; there was less fever, but a tendency to collapse; the gallic acid was increased to three grains every third hour. In the afternoon, ten ounces of a much lighter coloured secretion was passed, when the gallic acid was diminished a half. On the morning of the tenth day there was more fever; no urine had been passed since the preceding day; the gallic acid was then stopped. On the morning of the eleventh day, no urine having been passed for thirty-six hours, a catheter was introduced, and four ounces drawn off, of a dark brown muddy appearance. It contained numerous short solid pieces of granular tube-casts, of $2^{t}\cdot 0$, $1^{t}\cdot 0$, and $0^{t}\cdot 75$ in diameter; these seemed infiltrated with a clear material, which in many had accumulated pretty thickly outside the granular portion, and with it was tinged throughout of a reddish brown colour. These casts were unaffected by strong acetic acid. No blood-globule was seen in the casts, or free in the urine itself. With heat the urine gave a coagulum amounting to one-fifth of the whole, and with acetic acid one amounting to one-tenth; the former dissolved in caustic potash, but was unaffected by carbonate of soda; the latter dissolved in carbonate of soda. The man died in the evening of this day.

This case is of much interest in the following respects:—1st. As showing the existence in the urine, at the same time, of albumen, casein, globuline, and the colouring matter of the blood. 2nd. In the absence of all trace of blood-globules throughout. 3rd. In the immediate reduction of the quantity of casein on the eighth day, under the influence of gallic acid. 4th. In the formation of waxy tube-casts under these circumstances, and the rapid diminution of the urine. Gallic acid passes through the kidneys unchanged, and may be detected in the urine within a very short time of its administration. It does not coagulate albumen, but precipitates casein immediately, and if brought in contact with the latter, sufficiently concentrated, as in the tubes of the kidneys, would cause its coagulation between and around the epithelium and granular matter constituting waxy casts, as in this case. Gallic acid would thus be a remedy of very questionable advantage in cases in which the urine contained casein.

The occurrence of albumen in the urine of yellow fever was first remarked, so far as I am aware, by Staff-surgeon Collins, at Barbadoes, in 1848,* and the same gentleman met with two cases in which the albumen was replaced by a substance having the properties of casein.† Blair seems to have doubted the correctness of the latter observation,‡ and there is no indication in La Roche's work, that any other person had detected this substance in the disease.

In watching a case from day to day it is found that, sometimes in the course of the third day, or by the morning of the fourth, heat indicates a small portion of globuline or albumen in the urine; and on the morning, or in the course of the fourth day, albumen generally appears in such quantity as to obscure the indications of globuline. The fifth day the albumen is more copious, and would seem to attain its maximum on that or the following one, after which it gradually declines. Casein appears about the fourth day, and accompanies either globuline or albumen. I do not remember to have seen it alone in fever. When albumen becomes very copious, the casein frequently disappears, and, under ordinary circumstances, seems to do so before the albumen. In some cases, globuline and casein have continued during the course of the disease, without albumen having been detected.

The quantity of albumen, &c., may be estimated approximately, from day to day, by the space the sediment occupies in the test-tube relatively to the bulk of the original fluid, and may be expressed conveniently in parts of the whole, either as a decimal, or vulgar fraction. The albumen was found to vary in different cases from a mere trace to 1·0; the globuline, from a trace to 0·7, though in general not exceeding 0·1; and the casein from a trace to 0·2, but generally less than 0·1.

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* Blair on the Yellow Fever at Demerara, third edition, p. 98. † Ibid., p. 99.

‡ Blair: Report on the Recent Yellow Fever Epidemic at Demerara, 1856, p. 18.

The albumen varies greatly. At one time most cases present it to a considerable amount, at another there is much less, though the cases may be very severe, or even fatal. When the liver was severely implicated, and there was marked tenderness over it, with an early yellowness of the surface, the urine contained less albumen than when the hepatic affection was less prominent. This peculiarly existed during the greater part of 1859, and early part of 1860.

Though the urine have a high specific gravity from the third day onwards, the urea seems much diminished. I have not determined the quantity with precision, but much less was obtained on evaporating a small portion of the fluid, acidified with nitric acid, than from healthy urine. La Roche gives the result of an analysis to the same effect.*

Though urea be deficient, creatine is unusually copious in the febrile affections of Jamaica, whether pure remittent or yellow. If two or three drops of urine from such cases be evaporated on a piece of glass, numerous needle-like crystals, visible to the unassisted eye, more or less branched, appear before the fluid has quite dried, and frequently cover the surface extending from one side of the specimen to the other, among the substances deposited during the evaporation. At a certain stage in the operation small rhomboidal plates appear, or more frequently hexagons, with the obtuse angles of the rhomboid removed, and, as the concentration proceeds, these are seen to shoot out both ways from the acute angles, forming the needle-like crystals mentioned: the needles always present a swelling in the position of the original hexagon, and the prolongations seem to have a triangular section, with the base on the glass, and the apex uppermost. These crystals present the nacreous appearance peculiar to creatine; they are soluble in water, ammonia, and dilute nitric, hydrochloric, and acetic acids, from which they are deposited again, generally in the form of hexagonal plates. Alcohol does not seem to affect them. Sometimes, though the specimen under examination be rich in creatine, it remains fluid and the creatine does not crystallize; if a drop or two of alcohol be added under these circumstances, numerous rhomboidal or hexagonal crystals appear, but they generally redissolve as the alcohol evaporates.

In one case, in which these crystals were numerous, and the portion of the urine from which they were obtained was evaporated to dryness over a water-bath, the residue was exhausted by alcohol, to remove urea, &c., and the creatine was separated from the remainder by ammonia. On filtering and concentrating the respective solutions, a quantity of creatine was obtained, about equal to that of the urea.

Creatinine exists in many of these cases in considerable quantity, besides creatine, but as it requires a troublesome process to isolate, it was less easily detected, and not so often looked for. In one case in which the urine had been treated as in the last paragraph, a portion of the concentrated alcoholic extract gave very little urea, even when nitric acid was added, but the remainder displayed numerous clusters of beautiful navicular crystals of creatinine. When a drop of this secretion acidified with nitric acid is evaporated on a slip of glass, in addition to the nitrate of urea, crystals are often seen resembling the dendritic masses of creatinine figured by Robin and Verdeil,* but these mostly extend from one side of the axis of the mass only, and are not symmetrical, as represented by those authors; at other points they present the characters delineated at Fig. 2 *a* of the same plate. The exact composition of the crystals in question I cannot say, but if digested in ether, the latter on evaporation affords the symmetrical crystallization represented by Robin and Verdeil, with more or less hippuric acid, which usually accompanies them. I have recently obtained similar results from a specimen of urine from a case of bronchitis in this country.

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* La Roche on Yellow Fever, vol. i. p. 361.

Rounded masses resembling leucine were occasionally seen, when a few drops of the secretion were allowed to evaporate on glass, but this was rare. I do not recollect to have met with tyrosine, as figured by Frerichs in the frontispiece of the Sydenham Society's edition of his work on 'Diseases of the Liver.'

Uric acid was frequently present, and even in considerable quantity, when the urea was much diminished, and the chlorides almost or entirely absent. This was not found in every case, however, and I am not prepared to indicate the peculiarity of those in which it was observed.

Hippuric acid is formed copiously in the urine of febrile cases in Jamaica, as indeed in every other instance in which it was looked for. Uric acid was often plentiful in the same specimens, neither seeming to take the place of the other. When present in any quantity, it is easily detected by placing two or three drops of the secretion on a slip of glass, adding a drop of nitric acid, and evaporating slowly, when the peculiarity of the crystallization is quite characteristic.†

The chlorides in the urine undergo a marked decrease in the case of yellow fever, as met with in Jamaica. The only notice I have seen on this point is that given by La Roche,‡ on the authority of Dr. Wragg, of Charleston, who was of opinion that hydrochloric acid was thrown out largely from the kidneys. The details of Dr. Wragg's process are not given, nor the period of the disease to which his observations refer, which are important circumstances. My observations were made in the usual manner, by acidifying with nitric acid a portion of urine with its albumen removed (when so plentiful as to mask the operation), and then adding nitrate of silver. The freedom of the nitric acid from chlorine was previously ascertained.

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* Robin et Verdeil: Traité de Chimie Anatomique et Physiologique, Atlas, pl. xxvii. fig. 2.

† British and Foreign Medico-Chirurgical Review, vol. xxviii. p. 487. I have found hippuric acid in advanced pregnancy and in chronic bronchitis, in this country, where no benzoic acid had been administered previously, and apprehend it is more common than is generally supposed.

‡ La Roche on Yellow Fever, vol. i. p. 359.

With these precautions, the chlorides were found perceptibly less as soon as the urine contained traces of albumen, and on the evening of the fourth day and course of the fifth, when the desquamation of the urinary passages was in active progress, there was never more than a trace, sometimes not even a trace, to be detected. These began to reappear about the seventh day, when the case progressed favourably, and increased from day to day thereafter.

Blair remarks* that albumen was not detected in the urine of cases of intermittent which occurred contemporaneously with the epidemic, and he saw the value of the distinction in diagnosis. My observations are to a similar effect, having found, concurrent with decided yellow fever, intermittents and remittents, which presented the usual characteristics of these affections, and neither displayed desquamation of the bladder or kidneys, nor albumen, globuline, or casein in the urine, while the chlorides remained undiminished throughout. I met with one case, however, exactly resembling those of yellow fever, which occurred about the same time, save that there were neither tube-casts nor albumen in the urine, nor were the chlorides much diminished. This individual had previously had frequent attacks of inflammation of the sheaths of the tendons in the wrists, hands, ankles, and feet. How far the kidneys may have been influenced by the peculiarity of constitution indicated thereby, must remain an open question at present.

Albuminuria is common in Jamaica, and it is quite possible that febrile symptoms might arise in such a case, and lead to a doubt as to its nature. It may be distinguished from yellow fever, however, by the absence of the desquamation of the bladder on the fourth day, followed by that of the kidneys, and by the urine retaining a fair proportion of chlorides, though loaded with albumen.

Alvine Evacuations.—The alvine evacuations have not received the attention they require; until they have been as closely examined as the urinary, much valuable information regarding disease will remain untouched. My own observations on the subject have neither been so numerous nor so minute as those on the urine, still they afford some hints which are not noticed elsewhere, and which seem of value in explaining the characters of yellow fever.

Blair has given a short chapter on the character of the stools, as he saw them in Demerara,† which agree in the main with what was found in Jamaica. He describes the evacuations as feculent at first, with more or less admixture of mucus, and a matter he denominates "melanotic," and as giving off a very disagreeable odour. These were succeeded by what he calls the "caddy stool," a liquid light-coloured evacuation, depositing a dirty grey sediment, containing crystals of triple phosphates, and uric acid, and numerous little amorphous masses of black opaque matter, which he regards as its constant ingredient. As the disease advanced the caddy stool was replaced by a very scanty mucous stool, consisting of clear mucus, with broken-up epithelial matter, and myriads of epithelial granules, either uncoloured, or variously tinted of yellow, or green colour, by bile, or brown or black with the elements of blood. These may present several of the crystalline forms of the caddy stool, and are contemporaneous with diminished urine and black vomit. The elements of blood were sometimes so copious as to give the evacuation the appearance of black vomit.

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* Report, 1856, p. 21.

† Ibid., pp. 24–28.

In Jamaica the bowels were seldom costive or difficult to move. The first evacuations were always feculent, more or less modified by medicine, and generally offensive. About the fourth day, or earlier, the brown feculent character which had already been becoming less marked, often disappeared when the lighter-coloured stool Blair designates "caddy" took its place; this, though frequently liquid as he describes, was by no means always so, for I have seen it consistent, and formed, in many cases; it was never very copious. Its chief characteristic was the want of the brown colouring matter (usually thought bilious), supplied by the glands of the mucous membrane of the colon,* which is altogether different in colour from bile, and gives a different reaction with acids. These discharges may even have a yellowish or greenish tinge, from bile, however, while the proper brown is nearly or entirely absent. They seem to differ little from the clayey evacuations which accompany jaundice and other affections in this country, only less copious, and essentially depend on the colon performing its secreting function imperfectly, a circumstance which though frequently associated with retention of the biliary secretion, is not necessarily so, either in diseases of this country or the tropics.

The persistence of these light-coloured evacuations is always of serious import, as they are frequently followed by black vomit, or other forms of hæmorrhage. Their disappearance, on the other hand, on the occurrence of a more natural feculent evacuation about the fourth or fifth day, is usually the harbinger of a safe termination, and speedy convalescence. The suppression of the natural secretion of the colon would therefore seem to be intimately connected with the vicarious appearance of hæmatine in some form elsewhere. The importance of this principle in yellow fever is very great, as it directs attention to exciting the secretion of the colon, as the natural way of obviating many of the unmanageable symptoms of this disease, a point of late years too much overlooked.

The evacuations occasionally contained the elements of blood. These varied in appearance from blood little changed, through a fluid of the colour of dark venous blood with very few globules, to one exactly resembling black vomit, of a blackish brown colour, with scarcely a blood-globule to be seen. The more hæmorrhagic forms came from the lower part of the small intestine, or colon, and those without the globules from the stomach or duodenum. The mucous membrane of these organs respectively, in such cases as were examined, being found congested, softened, and easily abraded. In the intestines these fluids sometimes present a very different aspect in different parts. I have seen a fluid like very dark venous blood, with acid reaction, in the stomach and duodenum, which, in the lower part of the jejunum, had a black colour with alkaline reaction, and under the microscope a greenish tint, changes effected by an admixture of bile. In this case there had not been black vomit, but from the stomach to the cœcum the canal was filled with this bloody fluid. Some assistance may be obtained towards deciding on the part these discharges come from by the nature of the contained epithelium. The black vomit from the stomach, in addition to columnar epithelium, contains numerous small granules with occasional granular cells; that from the intestine has the columnar epithelium in abundance; while the discharges from the colon alone contain casts more or less complete from the tubular glands of its mucous membrane. A discharge may contain all these, but one form or other will predominate, according to the locality where it was produced.

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* See British and Foreign Medico-Chirurgical Review, vol. xxviii. p. 488.

A full chemical analysis of the alvine evacuations in yellow fever is very desirable. I made various attempts at qualitative examination, but as the methods were defective, and the results consequently uncertain, it would be useless to notice them further.

Discharges from Stomach.—Though the discharges from the stomach have attracted attention from the earliest period, much difference of opinion exists as to their nature and origin, and their value as characteristic of the disease.

At the commencement, if the stomach be irritable, the matters rejected, in addition to the ordinary ingesta, are mucous, more or less tinged with bile. To these succeed, in many cases, a clear fluid, with an acid reaction, which seems to have been particularized first by Blair, and which he denominated "acid elimination," or "white vomit." This, again, is followed by black vomit. All these may occur in succession, in the same individual, but one or more, or even all, may be absent in a genuine case of yellow fever.

The early vomitings are accompanied by a good deal of nausea, and much straining. With the white vomit there is extreme oppression at the precordia, often with a burning sensation; the straining during the efforts to vomit is very great, and, after a painful endeavour to relieve the stomach, the patient will often turn back in bed without having thrown off anything. When a little is rejected it is usually clear mucus, sometimes very acid, but sometimes this is less marked. The more copious the white vomit is, the less acid is it found to be, and the oppression at the epigastrium seems less.

When black vomit comes on, the discharge takes place without any very decided effort, and often without any apparent exertion of those muscles which are deeply engaged in the ordinary efforts of vomiting; and the oppression at the precordia, so remarkable with the white vomit, has often completely disappeared.

The transition from white to black vomit first manifests itself by the appearance of brown specks in the clear mucus, which have been likened to pinches of snuff. These increase in number, the mucus becomes more limpid, and tinged more or less of the same colour, and a sediment separates. The fluid in this condition often remains decidedly acid. As the brown matter increases in quantity, the acidity frequently becomes less marked, and sometimes is insufficient to redden litmus.

The nature, and place, and mode of origin, of black vomit, have given rise to much discussion. Many have thought it a morbid secretion of the liver; others have attributed it to a dissolved state of the blood, allowing it to exude through the mucous membrane; others have considered there was hæmorrhage from the capillaries, and that the blood-globules were destroyed in the acid secretions of the stomach; others, again, have attributed the black vomit to a secretion from the mucous surfaces of the organs in which it was found.

On examining specimens of characteristic black vomit by the microscope I found much columnar and glandular epithelium, the latter granular; and many free granules which were colourless, pretty clear, spherical, and sometimes corrugated on the surface; these were half the diameter of blood-corpuscles, and of a different colour. The colouring matter was brown, amorphous, and no blood-globules were detected. Spores, torulæ, and other extraneous matters were common. These appearances agree in the main with those described by American authors; but I have not met with the masses "of modified and disintegrated blood-corpuscles," or "the granular detritus and irregular masses, apparently the results of degradation of blood-corpuscles" described by La Roche.* The discharge is sometimes much more of the colour of venous blood than the usual coffee-ground appearance, and may even contain blood-globules, little altered, from hæmorrhage, but in its most characteristic forms these may be, and most frequently are, completely absent.

When little black vomit had been ejected, or formed in the stomach, a large portion of the mucous membrane was often of a deep brown colour. When more of that had been formed, even though it remained in the organ, the lining membrane presented a less extensive discoloration, a few brown streaks only remaining, or even these were absent. It is clear, from this fact, that the discoloration of the mucous membrane does not arise from imbibition of the coloured fluids in contact with it. Yet on placing a section from the discoloured portions under the microscope, the tubular glands were found with their epithelium in a granular condition, and thoroughly impregnated with a brown colouring matter—the granules, however, remaining pretty free from it. Vessels could be detected among the tubes, in various places, distinctly, with entire blood-corpuscles in them. As has been stated by Blair and others, it is quite a mistake that the blood, generally, is in the dissolved state so often supposed by many authors.

The facts of the glandular epithelium in the tubular glands of the stomach being coloured brown and containing numerous granules, coupled with the disappearance of that colour as black vomit becomes copious, and the occurrence of similar elements constituting the characteristic portions of the vomit itself, appear to leave no doubt as to its place of origin, and as to its being a true secretion, though occurring in the course of disease. It is quite analogous in this respect to what has been described above as having taken place from the kidneys in the course of this disease, and to what I have elsewhere shown takes place in a state of health from the glands of the mucous membrane of the colon.* It is a significant fact, too, that those cases of yellow fever in which the colon ceases to perform this part of its function, are those most inimical to black vomit, or similar discharges, or hæmorrhage from other organs, while natural-coloured alvine discharges are the surest signs of amendment. The occurrence of hydrochloric acid in considerable quantity in the white and black vomits, coincident with the diminished elimination of chlorides from the kidneys, affords another indication of the stomach exercising a vicarious eliminative action in the disease.

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* On Yellow Fever, vol. i. p. 314.

Blair has given a table of the days of occurrence of white and black vomits, from which it appears that the former manifests itself most frequently on the third and fourth days of the disease, though frequently also (and nearly in equal numbers each day) on the second and fifth days; while in other cases it took place as late as the twelfth day. The black vomit appeared most frequently on the fourth, fifth, and sixth days, though cases were by no means uncommon on the third and seventh days, and instances were seen on the first, and as late as the thirteenth day of the disease. As these discharges may be regarded as efforts of the system at crises, it is clear they may be looked for at the various periods of the disease when critical evacuations might be expected; and though most frequent from the third to the sixth day, yet it is possible they might occur either sooner or later. Blair's table, however, may require modification; he was under the impression that the access of yellow fever was characterized by well-marked symptoms, which left no doubt as to its period of invasion; this, however, is not always the case, and the exceptions are more numerous than he contemplated. The following extract from one of the older writers on West Indian fever is more correct:

> "It is worth remarking, that the fever sometimes appears in a very slight way, with languor, loss of appetite, some degree of headache, disturbed sleep, and whiteness of tongue; the patient being able all the while to go about his usual employment. In symptoms so moderate, the presence of a fever is hardly acknowledged, though the readiness with which they rise into a severe disease, on the least irregularity, or any anxiety or distress of mind, leaves no doubt of their nature."†

Cases answering this description must have occurred to every one of any experience in the tropics; and the difficulty I have had in fixing the period of accession of the disease in such, makes me doubtful as to the weight to be attached to Blair's determinations for the earlier days in his table.

Hœmorrhages from other Organs.—I have known three cases of discharge of bloody fluid from the lungs in the last stage of yellow fever;

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* British and Foreign Medico-Chirurgical Review, vol. xxviii. p. 488.

† Observations on the Diseases of the Army in Jamaica, by John Hunter, M.D., Physician to the Army, p. 95. London, 1788.

and oozing from the gums, nose, and conjunctiva are not uncommon. Copious discharges from the vagina are met with in females. As in none of these cases, however, had I examined the fluid with the microscope, I cannot give any information as to the condition of the blood it contained.

(To be continued.)

ART. III.

An Inquiry into the Influence of the Abuse of Alcohol as a Predisposing Cause of Disease. By WILLIAM MARCET, M.D., F.R.S., Assistant-Physician to the Westminster Hospital, &c. &c.

THE object of the present communication is to determine, by a series of observations on hospital out-patients, the influence of the abuse of alcohol as a predisposing cause of disease. After having thoroughly considered the subject, I came to the conclusion that the only method of investigation calculated to yield reliable results was to examine all the patients who came under my care at the Westminster Hospital, as to their habits of sobriety, at the same time making a careful diagnosis of each case; my purpose by adopting this process being to afford means of establishing the relative proportions of sobers and drinkers according to the diseases for the relief of which they applied. I thought that, after carrying on these observations during twelve months, a sufficient number of data would be obtained for the object I had in view.

Several difficulties now offered themselves to this mode of investigation.

1st. The uncertainty relative to the amount of fermented or distilled liquor taken habitually by a patient, or indulged in for some time on a past occasion. This I overcame by careful examination and cross-questioning, hardly ever dismissing a case until I had made out in my mind whether the amount of the patient's libations could possibly in any way, and at any time, have affected his health. If there were doubts as to this which I could not overcome, I introduced the case with a query before the statement relating to the sobriety. I considered as drinkers certain patients who had assumed habits of perfect sobriety after having at some time or other, for many months, or several years in succession, led an intemperate life. Again, I introduced as drinkers those who were usually drunk once a week, many being the worse for liquor on Saturday evenings; also those who, although seldom or ever drunk, took daily, or often, an amount of alcoholic beverage sufficient to exhilarate much their spirits and keep them in a usual state of excitement; and finally, patients who, apparently not affected by drink, took it in much larger quantity than can possibly be consistent with health' which occurred mostly with the view of getting over certain hard manual labour.

2nd. The difficulty arising from the utter impossibility of finding time to examine carefully both male and female patients, the latter, moreover, appearing often indignant at any doubts being entertained as to their sobriety, and I was at the outset obliged to give up including women in my series of observations.

3rd. Another difficulty I met with after keeping up my notes for a period of twelve months, resulted from the comparatively small number of patients with which I had to conduct my inquiry, this number amounting to 695; and for this reason I found it necessary to avoid entering into many subdivisions, adopting wide groups. I experienced also some trouble on account of patients applying for relief several times at intervals of weeks or months, in the course of the year; after some hesitation I determined on omitting to report every visit subsequent to the first series, if the patient was obviously suffering from a relapse of the same disease. But if the patient returned to the hospital to be treated for another complaint, I again entered him into my journal, treating the case altogether as a new one. After carrying on these investigations for some time, I found that individuals under nineteen years of age were very seldom guilty of being drinkers; I therefore took no notice of patients under that age.

Each page of my note-book was divided into eight columns. In the first, the patients were numbered, beginning every day at No. 1; the second column contained the date of admission; the third, a statement whether the patient applied with a letter constituting him a regular out-patient, or a ticket for one consultation; the fourth, the age; the fifth, the employment; the sixth, a statement as to whether the patient was a sober man or a drinker; the seventh, the diagnosis; the eighth, headed *Observations*, contained a report of the characteristic symptoms on which the diagnosis was founded.*

I saw the patients regularly twice a week, and I must here acknowledge the kind and valuable assistance I received from my friend, Dr. Dapples, who kept up these observations for me from the middle of August till the latter part of September, while I was out of town; this gentleman having often kindly assisted me when engaged taking my notes, was well qualified to continue the work during my absence.

The tables were drawn up with the greatest possible care. The employments of the patients, from their great variety, had to be condensed into fifteen groups, headed, Coal-porter, Cabman, Stableman, Shoemaker, Hawker, Labourer, Mason, Sailor, Carrier (carman), Carpenter, Painter, Shopkeeper, Engineer, Tailor, and Porter (messenger); these included no less than one hundred and twenty-seven different kinds of employments. I grouped together those employments bearing the greatest analogy with each other, and which were carried on under similar sanatory condition. This implies that indoors employments were in no way grouped with employments carried on in the open air—an important point, considering that Dr. Guy has shown the degree of mortality from certain diseases to vary according to employments being carried on indoors or in the open air.*

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* The hospital notes referred to in this paper were taken from the 1st December, 1859, to the 1st December, 1860.

I classified the diseases into nine groups, namely—

1st. Alcoholism.
2nd. Febrile disorders.
3rd. Diseases of the lungs.
4th. Poisoning by lead.
5th. Diseases of the stomach and intestines.
6th. Diseases of the skin.
7th. Inflammatory affections of the muscles.
8th. Diseases of the nervous system (non-alcoholic).
9th. Diseases of other organs and tissues, mostly inflammatory.

The ninth class of diseases includes a number of affections which could not be entered into the other groups; they exhibit, however, this connexion, that they are mostly of an inflammatory character, and attack glands and mucous membranes. It was impossible to divide them into separate groups, as they include 22 diseases and only 54 patients, giving an average of 2·5 patients for every group.

The class *Diseases of the lungs*, being comparatively very extensive, I thought it would be an advantage to consider separately the cases of laryngitis (16 cases), phthisis (34 cases), bronchitis (166 cases), and pneumonia (33 cases), without, however, removing them from the group *Diseases of the lungs.* The disorders under the head *Poisoning by lead*, occurred entirely in men using lead paint; I attempted to enter these cases into other groups, but found it impracticable, on account of the combination of nervous and gastric symptoms which attend these affections; I was therefore reluctantly obliged to make them into a separate group of only eleven patients, this number being, however, too small to yield any special results.

I have disposed the information imparted by my hospital notes under the form of the following *fundamental table*, which is the groundwork of my inquiries. This table shows at a glance the proportions of sobers and drinkers in connexion with diseases and employments:—

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* See the Journal of the Statistical Society, vol. vii.: A Third Contribution to the Knowledge of the Influence of Employments upon Health, by Dr. William Augustus Guy. According to the author of this paper, the ratio which deaths from consumption bear to those from all other diseases, is higher in the case of men employed within doors than in those working in the open air, being in the one case 1 to 1·96, and in the other 2 to 2·56 (or 1 to 1·28).

TABLE, SHOWING THE PROPORTION OF SOBERS AND DRINKERS

S. means Sober. ? means Doubtful.

		17 Coal-porter.			20 Cabman.			17 Stableman.			60 Shoemaker.			39 Hawker.			174 Labourer.		
		S.	?	D.	S.	?	D.	S.	?	D.	S.	?	D.	S.	?	D.	S.	?	D.
Alcoholism . . 16	15 Chronic alcoholism	0		1	0		1	0		0	0		2	0		2	0		1
	1 Delirium tremens																		
Febrile disorders 26	12 Fever	0		0	0		1	0		0	1		2	1	1	3	6		4
	14 Ague																		
Diseases of the Lungs (air-passages) . 268	16 Laryngitis	0		0	0		1	0		0	1		1	2		1	0		5
	3 Pleuro-pneumonia	0		1	0		0	0		0	0		0	0		0	2		4
	1 Pleurisy																		
	10 Emphysema																		
	3 Pulmonary apoplexy																		
	1 Hæmoptysis																		
	1 Spasm of the glottis																		
	34 Phthisis	0		1	0		0	1		0	1		2	1		0	2		4
	966 Bronchitis	0		4	3		3	1		1	6		7	5	1	6	29	2	13
	33 Pneumonia	2		1	0		1	0		1	2		0	2		0	4		3
Poisoning by lead, 11	11 Poisoning by lead	0		0	0		0	0		0	0		0	0		0	0		0
Diseases of the Stomach and Intestines . 82	50 Gastralgia	0		0	0		1	1	1	1	0		1	1		0	8		10
	2 Gastorrhagia	0		0	0		0	0		0	6		2	3		2	4		2
	2 Cardialgia																		
	6 Intestinal hæmorrhage																		
	10 Diarrhœa																		
	8 Enteritis																		
	?1 Peritonitis																		
	1 Prolapsus ani																		
	2 Tænia																		
Diseases of the Skin . . . 19	1 Erysipelas	0		0	0		1	0		1	1		1	0		0	1		2
	6 Prurigo																		
	3 Eczema																		
	1 Scabies																		
	1 Impetigo																		
	2 Porrigo																		
	1 Lepra																		
	2 Purpura																		
	1 Scurvy																		
	1 Carbuncle																		
Inflammatory Affections of Muscles . . 151	140 Rheumatism	3		3	3		2	3		2	6		7	3		4	23		19
	8 Gout																		
	3 Strain (followed by rheumatic symptoms)																		
Diseases of the Nervous System (non-alcoholic) . . . 68	4 Paralysis (local)	0	1	0	1		0	1		1	3		4	0		0	9		4
	4 Hemiplegia																		
	7 Neuralgia																		
	5 Apoplexy																		
	3 Vertigo																		
	12 Cerebral congestion																		
	1 Cerebral concussion																		
	12 Cephalalgia																		
	1 Nervous irritability																		
	11 Melancholia																		
	3 Debilitas nervosa																		
	2 Epilepsy																		
	2 Paralysis agitans																		
	1 Muscular spasms																		
Diseases of other internal organs and tissues, mostly inflammatory . . . 54	2 Disease of liver	0		0	1		1	1		1	3		1	1		0	8		5
	5 „ kidney																		
	1 (Diuresis)																		
	7 Disease of heart																		
	2 „ spleen																		
	1 Œdema of face																		
	1 Disease of internal ear																		
	1 Otitis																		
	1 Coryza																		
	1 Epistaxis																		
	2 Gingivitis																		
	18 Pharyngitis and tonsillitis																		
	1 Cynanche parotidea																		
	1 Catarrh of urethra																		
	1 (Gonorrhœa)																		
	1 Cystitis																		
	1 Orchitis																		
	1 Dropsical effusion																		
	2 Glandular swellings																		
	1 Mercurialism																		
	2 Periostitis																		
	1 Cystic tumour																		
Number of S., ?, and D., per employment		5	1	11	8		12	8	1	8	30		30	19	2	18	96	2	76
Proportion of S. and D. per employment		1 : 2·20			1 : 1·50			1 : 1			1 : 1			1·06 : 1			1·26 : 1		

IN CONNEXION WITH DISEASES AND EMPLOYMENTS.

D. means Drinker.

42 Mason.			33 Sailor.			20 Carrier.			55 Carpenter.			38 Painter.			68 Shopkeeper.			40 Engineer.			30 Tailor.			42 Porter.			Sum of Sobers and Drinkers.			Proportions of Sobers to Drinkers.	
S.	?	D.	S.	?	D.	S.	?	D.	S.	?	D	S.	?	D.	S.	?	D.	S.	?	D.	S.	?	D.	S.	?	D.	S.	?	D.	S. D.	
0		2	0		0	0		0	0		0	0		1	0		6	0		0	0		0	0		0	0		16	0·0 : 16	
0		0	1		1	0		0	1		1	1		0	0		0	2		0	0		0	0		0	13		12	1·08 : 1	
1		1	1		0	0		0	1		0	0		0	0		1	0		0	0		0	0		0	6		10	1 : 1·67	
2		0	0		0	0		0	1		0	0		1	1		1	2		0	1		1	0		2	9		10	1 : 1·11	1·29 : 1
2		2	1		1	1		1	4		2	2		0	2		1	0		0	1		1	1		0	19		15	1·27 : 1	
3		3	3		5	5	1	1	7		8	7	0	2	6		4	4		7	5		2	10		2	94		68	1·38 : 1	
2	1	0	0		1	1	1	1	2		1	1		0	2		1	2		0	0		0	0		1	20		11	1·82 : 1	
0		0	0		0	0		0	0		0	6	1	4	0		0	0		0	0		0	0		0	6		4	1·50 : 1	
0		2	0		1	0		1	1		1	0		2	10		0	0		1	2		1	3		1	26		23	1·13 : 1	1·56 : 1
0		1	3		0	1		0	1		0	1		1	0		0	1		0	2		1	1		0	23		9	2·55 : 1	
0		1	1		0	0		0	3		1	1		0	1	1	0	0		1	0		0	2		0	11		7	1·57 : 1	
6	1	4	5		0	4		1	8	1	3			0	10		3	5		2	4		1	7	1	3	94		54	1·74 : 1	
2		1	2		1	0		1	4		1	1		1	12	1	2	7		0	4		1	2		1	48		18	2·67 : 1	
5		0	3		3	0		0	3		0	1		0	3		0	5			3		0	5		0	42		12	3·50 : 1	
3	2	17	20		13	12	2	6	36	1	18	25	1	12	47	2	19	29		11	22		8	31	1	10	411	15	269	1·53 : 1	
1·35 : 1			1·54 : 1			2 : 1			2 : 1			2·08 : 1			2·47 : 1			2·64 : 1			2·75 : 1			3·70 : 1			1·53 : 1				

TABLE *showing the Proportion of Sobers and Drinkers according to Employments.* (*Abstracted from the preceding Table.*)

Indices of drinking tendencies.	Employments.	Total No.	Sobers.	Drinkers.	Doubtful.	Proportion of Sobers to Drinkers.
						S. D.
1	Coal-porter . .	17	5	11	1	1 : 2·20
2	Cabman . . .	20	8	12	0	1 : 1·50
3	Stableman. . .	17	8	8	1	1 : 1
4	Shoemaker . .	60	30	30	0	1 : 1
5	Hawker . . .	39	19	18	2	1·06 : 1
6	Labourer . . .	174	96	76	2	1·26 : 1
7	Mason	42	23	17	2	1·35 : 1
8	Sailor	33	20	13	0	1·54 : 1
9	Carrier (Carman)	20	12	6	2	2 : 1
10	Carpenter . . .	55	36	18	1	2 : 1
11	Painter . . .	38	25	12	1	2·08 : 1
12	Shopkeeper . .	68	47	19	2	2·47 : 1
13	Engineer, Smith .	40	29	11	0	2·64 : 1
14	Tailor	30	22	8	0	2·75 : 1
15	Porter	42	31	10	1	3·10 : 1
		695	411	269	15	1·53 : 1

I shall now explain the construction of this fundamental table. The horizontal headings consist of the fifteen employments; with every employment is a number, showing how many patients it includes, and under each employment on the left is the letter S. for sobers, and on the right is the letter D. for drinkers; between the two a query is inserted for doubtful. The first employment on the left is that which yields the greatest proportion of drinkers; the second is that which yields the next greatest proportion of drinkers; and so on, till the last on the right, which yields the smallest proportion of drinkers, and consequently the largest proportion of sobers. The headings in the vertical column most on the left consist of the titles of the nine different groups of diseases, beginning with that group containing the greatest proportion of drinkers, proceeding downwards with that containing the next greatest proportion of drinkers, and so on, the last group including the least proportion of drinkers, and consequently the greatest proportion of sobers. With the title of each group of diseases is a number, showing how many patients belong to it. Opposite the title of each group of diseases a bracket has been placed within which are inscribed the names of the diseases which form the group, and with every disease there is a figure corresponding to the number of individuals who have been affected by it. The table is divided into other vertical columns; each column is headed by an employment, and exhibits the number of sobers, drinkers, and doubtful in that employment; these numbers are of course also placed horizontally opposite the group of disease to which belong the patients they represent. The last vertical column but one on the right indicates the total number of sobers and drinkers for each group of diseases; and the last column on the right shows the whole proportion of sobers and drinkers for each group of diseases. Finally, at the bottom of the table there are two horizontal lines, the first showing the *total number* of sobers and drinkers per employment, and the second the *proportion* of sobers and drinkers in each employment.

Having proceeded so far, I extracted from the table the numbers showing the proportions of sobers and drinkers in every employment, and placed them in a tabular form (see p. 490), beginning with those employments containing most drinkers, and ending with those containing the least; these fifteen proportions showed the relative drinking tendencies of each employment. By the side of every employment I placed a symbol of the simple multiples, beginning by 1, and proceeding *seriatim* up to 15; these figures, therefore, may be considered as indices of drinking tendencies. Thus, coal-porters (index 1) exhibit the greatest proportion of drinkers, since for every 1 sober there are no less than 2·20 drinkers; cabmen (index 2) include the next greatest proportion of drinkers, for every 1 sober there being 1·50 drinkers; and so on up to porters (index 15), whose tendency to drinking is the least, there being 3·10 sobers for 1 drinker.

The data being arranged as described above, furnished materials for my inquiries.

Influence of Alcohol as a General Predisposing Cause of Disease.

On glancing over the fundamental table (p. 489), the construction of which I have attempted to explain, the inquirer's attention will at once be arrested by the first group of diseases—*alchoholism.*

First Group: Alcoholism.—Every patient suffering from chronic alcoholism, or delirium tremens, is a drinker; indeed, in the 16 cases of this affection, alcohol is not a predisposing cause, but the exciting cause of the illness; it is, consequently, hardly fair to take these cases into consideration for the purpose of determining the general action of alcohol as a *predisposing* cause of disease. Yet I have thought it better to preserve them, as their being overlooked might appear a serious omission, and their number is so few as to have no material influence on the general researches. Should a question arise as to which employment is most subject to alcoholism, it would be natural to anticipate that coal-porters would be particularly liable to these affections—this employment possessing the greatest proportion of drinkers; such is not, however, the case, and this interesting fact is well worth recording. We find the greatest proportion of cases of alcoholism among the shopkeepers; for there is 1 out of every 11·3 shopkeepers suffering from alcoholism, while there is only 1 out of every 17 coal-porters who had contracted this illness. Now we find that shopkeepers are much more sober than coal-porters, for the degree of sobriety of shopkeepers is represented by No. 12, while that of coal-porters is represented by No. 1. The reason of this curious phenomenon is obviously that shopkeepers drink, taking but little exercise, being occupied indoors and in unhealthy districts and dwellings; thus their standard of health is lowered, which prevents them from resisting the baneful action of alcoholic excesses, and at the same time their respiration being deficient, they are unable to rid themselves by the process of respiration of the alcohol absorbed. Thirty-one different employments have been classed under the head *shopkeeper;* of these, commercial travellers and interpreters are perhaps the only two entailing exercise.

Second Group: Febrile Disorders: include the greatest proportion of drinkers compared to sobers irrespective of their employments, this proportion being 1 drinker to 1·08 sober. The proportion of drinkers to sobers taken collectively in all other diseases is 1 drinker to 1·55 sober, so that the proportion of drinkers to sobers attacked with a febrile affection is considerably greater than the corresponding proportion for all other diseases. This predisposition of drinkers becomes more obvious by comparing the proportion of sobers to drinkers in the groups of diseases under our present consideration with the corresponding proportion for the last group in the table, the latter including no less than three times and a half more sobers than drinkers; it follows that when living in a district where these affections are endemic, it is of great importance to lead a perfectly sober life. It may also be concluded that after having once contracted ague, rules of strict sobriety are among the most useful precautions to adopt in order to prevent a return of the illness. This influence of alcohol as a predisposing cause to febrile diseases probably results from the abuse of alcohol interfering with the healthy process of nutrition and lessening the general standard of health—a morbid poison exerting thereby the more readily its baneful action.

Third Group: Diseases of the Lungs.—The next group of diseases consists of all affections of the air-passages, and includes 1 drinker for every 1·29 sober; this proportion of drinkers is therefore nearly as great as in the preceding group; the interest in the present instance is enhanced by the fact, that there are as many as 268 patients suffering from pulmonary diseases—the great number increasing the degree of correctness of the results. This confirms the received opinion that, in comparison with other diseases, drinkers are much more predisposed to affections of the respiratory organs than sobers; it shows, moreover, that in no other disorders, except in fever and ague, this predisposition of drinkers compared to that of sobers is so great as in diseases of the air-passages. This fact may be satisfactorily explained, for the lungs of drinkers being a medium through which alcoholic vapours pass on their way out of the body, it is but very natural to infer that a constant state of irritation is kept up in these organs, which under the slightest exciting cause becomes a condition of disease; indeed, admitting this explanation, alcohol might be considered in itself as an exciting cause of pulmonary affections.

I have divided the group, *diseases of the lungs*, into five classes; the first class is *laryngitis*, including every case of evident inflammation or irritation of the larynx which has not proceeded to the bronchial tubes, as determined by auscultation and percussion. The result, from my inquiry, which applies to this disease is perhaps the most interesting of all. The number of drinkers affected with laryngitis is larger than the number of sobers: there being 1·67 drinker for every sober. In no other disease, or group of diseases throughout the whole table, is the proportion, and consequently the predisposition, of drinkers so great as in that under our present consideration. Why is this? Obviously because the larynx is exposed to the irritating action of the alcohol which is swallowed, from its coming in contact with the epiglottis and glottis, and because, moreover, the alcoholic vapours coming from the lungs and passing through the larynx, contribute to establish and keep up this morbid condition.

Pneumonia is the fifth class of diseases of the lungs; drinkers are comparatively to sobers, less predisposed to it than to the other classes of the same group, there being one drinker suffering from pneumonia for every 1·82 sobers; this may be accounted for if it be admitted that pneumonia is not strictly speaking an inflammatory disease, but the result of a special morbid action, respecting the development of which alcohol would play but a secondary part.*

Fifth Group: Diseases of the Stomach and Intestines.—If we compare the proportion of drinkers to sobers in the present case (1 D. to 1·56 S.), to the corresponding proportion of drinkers to sobers for all other diseases taken collectively (1 D. to 1·53 S.), we shall not find that there exists a greater predisposition from the abuse of alcohol to gastric and intestinal affections. But if we establish this comparison with the other groups of diseases taken individually, we shall observe that, although the degree of predisposition from alcohol to diseases of the stomach and intestines is less than in febrile disorders or pulmonary affections, yet it is greater than in the cases of diseases of the skin, gout and rheumatism, diseases of the nervous system (non-alcoholic), and diseases of the other internal organs and tissues (last group).

The group diseases of the stomach and intestines is divided into two classes, and by this means I am enabled to point out a very remarkable fact—viz., that *gastritis*, a condition of general inflammation or irritation of the stomach, with the accompanying modifications of the normal functions of this organ, is much more liable to be brought on by drink than disorders of the intestines properly so called. Drinkers suffer from gastritis in the proportion of 1 to 1·13 sobers, while drinkers are affected by intestinal disorders in the proportion of 1 to 2·55 sobers; so that the proportion of drinkers to sobers in the case of gastritis is twice as great as the corresponding proportion for disorders of the intestines; and if we compare the predisposition from alcoholic excesses to inflammatory affections of the stomach with the corresponding predisposition to all other diseases taken collectively, we shall find the predisposition to the former

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* I shall not take into consideration the fourth group—poisoning by lead—the number of patients it includes being too small.

(1 D. to 1·13 S.) much greater than the predisposition to the latter (1 D. to 1·57 S.) This excessive liability to gastritis from the abuse of alcohol, appears to me to result from the quantity of alcohol which passes into the duodenum being less and weaker than that which is admitted into the stomach, partly on account of the absorption which has taken place in this organ, partly from the alcohol being diluted in the intestines by the intestinal secretions, and possibly also from alcohol undergoing some chemical transformation in the bowels.

Sixth Group: Diseases of the Skin.—I have but little to say with respect to cutaneous affections; drinkers are affected, comparatively to sobers, much in the same proportion as in the case of diseases of the stomach and intestines. In cutaneous affections as in the case of fevers, it is admitted that the exciting cause of the disease depends on the action of a poison present in the system; in some instances we can trace this poison to contagion, in others to hereditary causes; the higher the general standard of health the greater the power of the body of resisting this morbid tendency, the influence of the poison being thereby kept in abeyance; any circumstance lowering the healthy condition of the body will thereby expose it to suffer from cutaneous affections under the influence of an exciting cause. Alcoholic excesses are undeniably among the most powerful depressing agents, and on this account predispose to diseases of the skin. In addition to this mode of explaining the influence of alcoholic excesses as a predisposing cause of cutaneous affections, it might be surmised, from the experiments of Messrs. Lallemand, Perrin, and Duroy,* that the passage of alcohol through the skin on its way out of the body, by increasing the vascular state of this tissue, predisposes it to suffer from inflammation.

Seventh Group: Gout and Rheumatism.—The interest of the inquiry in this case is increased by the large number of patients affected, which is 151. We observe that for every drinker there are 1·74 sober who apply to be treated for the disorders of this group. The predisposition from alcohol to gout and rheumatism is consequently a little less than the corresponding predisposition to all other diseases taken collectively (1 D. to 1·47 S.); but on the other hand, and I consider the following conclusion as more important, drinkers are, comparatively to sobers, *less predisposed to gout and rheumatism* than to fever and ague, diseases of the lungs, gastric and intestinal disorders, and cutaneous affections; and drinkers comparatively to sobers are *more predisposed to gout and rheumatism* (1 D. to 1·74 S.) than to diseases of the nervous system (non-alcoholic) (1 D. to 2·67 S.), and all other diseases of the internal organs and tissues (1 D. to 3·50 S.)

Eighth Group: Diseases of the Nervous System (Non-Alcoholic.)—I have taken care to exclude from this group every case of alcoholism, for these, although instances of nervous affections, are produced by alcohol acting as an *exciting* cause, and by grouping them with diseases of the nervous system, it would obviously lead to erroneous results as to the influence of alcohol as a *predisposing* cause to this group of disorders. We observe that the predisposition from alcohol to nervous affections (1 D. to 2·67 S.) is decidedly much less than to all other diseases taken collectively (1 D. to 1·44 S.) On the other hand, if we compare the predisposition from alcohol to nervous affections with the corresponding predisposition to all otherdiseases considered individually, we find that, with the exception of one group, the influence of alcohol as predisposing to nervous affections is the least of all. This result is remarkable; it might have been anticipated that alcohol acted as a strong predisposing cause of disease of the nervous system; for it is a well-known fact that the nervous substance has the power, to a certain extent, of condensing within its tissue the alcohol which has been absorbed into the blood, and consequently it would appear but natural that, alcohol interfering with the healthy nutrition of the nervous centres, the nervous system would become thereby more liable to (non-alcoholic) disease. According to my inquiries, however, this is not the case. Magnus Huss, the leading authority on alcoholism, believes that nervous temperaments are more capable of resisting the long-continued abuse of alcohol than sanguine temperaments. Might there not be some connexion between this and the fact that alcohol predisposes but very slightly to (the non-alcoholic) diseases of the nervous system?

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* These gentlemen have obtained positive evidence of a small proportion of the alcohol taken into the stomach being eliminated from the body through the skin. (Du rôle de l'alcohol et des anæsthetiques dans l'organisme.) The experiment which illustrates this interesting phenomenon has been exhibited to the Society of Arts by Dr. Edward Smith.

Ninth Group: Diseases of other Internal Organs and Tissues, mostly Inflammatory.—These affections form the last group: they include all the cases which could not be entered into any of the other groups; still they are not altogether without connexion with each other. I have attempted to indicate this connexion by the words *mostly inflammatory.* Diseases of the mucous membranes exclusive of those of the stomach and intestines, are prominent in this group; it appears from the present inquiry that alcohol predisposes but very slightly to these affections (1 D. to 3·50 S.), both when compared to the predisposition from alcohol to all other diseases collectively (1 D. to 1·44 S.), and also when compared to the predisposition from alcohol to all other diseases taken individually. Indeed, the influence of alcohol as predisposing to this last group of disease is the least of all. It might be observed, however, that diseases of the liver and kidneys are well known to be frequently the result of long-continued hard drinking. This is not borne out by the present inquiry, because the small number of these cases which figures in the table precludes the possibility of drawing any inference as to the special degree of predisposition alcohol exerts respecting them. My conclusions with reference to each group must be taken in a general point of view, without entering into the details, except where I have divided groups into classes.

On inquiring into the number of patients which constitute each group, it will be observed that diseases of the lungs and inflammatory affections of the muscles—viz., gout and rheumatism—include the greatest. I shall now attempt to show the comparative influence of alcohol as predisposing *employments* to these two groups of diseases; each group will be considered separately. The other groups do not include a sufficient number of patients to allow of the relative predisposition per employment, due to the influence of alcohol, being established.

On the Influence of the Abuse of Alcohol as Predisposing Employments and Individuals, per Employments, to Diseases of the Lungs.

Rather than give a general outline of the method employed for conducting this inquiry, I shall consider at once the *diseases of the lungs*, illustrating with respect to this group the arguments and operations which will likewise be adopted when treating of the influence of alcohol as predisposing employments and individuals, per employment, to gout and rheumatism.

The influence of alcohol as predisposing each employment to diseases of the lungs must be examined under two heads (Table A and Table B, p. 497).

1st. The influence of alcohol as predisposing each employment, taken as a whole, to diseases of the lungs.

2nd. The influence of alcohol as predisposing *drinkers* (compared to sobers) in each employment to pulmonary affections.

Table A (under first head) is formed by placing in a vertical column the fifteen employments, following each other from top to bottom *seriatim*, according to their degree of predisposition to diseases of the lungs. This degree of predisposition is shown by the proportions placed opposite each employment on the right, which exhibit for the corresponding employment the relation existing between the number of cases of pulmonary diseases and all other affections. By the side of every employment in this table, and on the left, is inscribed the index of drinking tendency of that employment. (See p. 490.)

Now, it is obvious that if the employments in Table A followed each other in the same order as in the Table, p. 490, showing the drinking tendencies of each employment, the degree of intemperance of any employment would exhibit its predisposition to diseases of the lungs, for the greater its habits of drinking, irrespective of every other circumstance, the greater would be its liability to pulmonary affections, and *vice versâ*. Of course it can hardly be anticipated that this is likely to take place, for we know how many other causes besides excesses in alcohol predispose to diseases of the lungs. Considering the employments one by one, we find no connexion between their predisposition to the group of disease under consideration and their drinking tendencies, neither do we find any such connexion when the employments are considered three by three; taken five by five, we detect, however, some kind of relation between the drinking tendencies and predisposition to diseases of the lungs, and when dividing the

Table A. *Diseases of the Lungs—Predisposition of Employments to Diseases of the Lungs, according to their Drinking tendency.*

Indices of drinking tendency.					Disease of the lungs.		All other diseases.	
			1	Coal porter	1	:	0·89	Employments including most drinkers most predisposed to diseases of the lungs.
			10	Carpenter	1	:	1·12	
20			5	Hawker	1	:	1·17	
			7	Mason	1	:	1·47	
			9	Carrier	1	:	1·50	
14	27		2	Cabman	1	:	1·50	
			6	Labourer	1	:	1·56	
		47·5	15	Porter	1	:	1·63	
34			13	Engineer	1	:	1·66	Employments including least drinkers least predisposed to diseases of the lungs.
			14	Tailor	1	:	1·73	
	55		8	Sailor	1	:	1·75	
33			11	Painter	1	:	1·92	
			4	Shoemaker	1	:	2·00	
			12	Shopkeeper	1	:	2·58	
			3	Stablemen	1	:	3·25	
19	38	72·5						

Table B. *Diseases of the Lungs—Predisposition of Drinkers, per Employments, to Disease of the Lungs.*

No.	—	Comparative proportions of Sobers and Drinkers suffering from diseases of the lungs.				Comparative proportions of Sobers and Drinkers suffering from all other diseases.			Indices of predisposition.*
		S.		D.		S.		D.	
1.	Shopkeepers	1·37	:	1	....	7·20	:	1	5·25
2.	Engineers	1·14	:	1	....	4·00	:	1	3·51
3.	Coal porters	1	:	3·50	(0·29 : 1) ...	1	:	1	3·45
4.	Sailors	1	:	1·40	(0·71 : 1) ...	2·16	:	1	3·84
5.	Carpenters	1·33	:	1	....	3·00	:	1	2·25
6.	Tailor	1·75	:	1	....	3·75	:	1	2·14
7.	Cabman	1	:	1·67	(0·59 : 1) ...	1·20	:	1	2·03
8.	Porter	2·20	:	1	....	4·00	:	1	1·82
9.	Shoemaker	1	:	1	....	1·11	:	1	1·11
10.	Labourer	1·27	:	1	....	1·28	:	1	1·01
11.	Stableman	1	:	1	....	1	:	1	1·00
12.	Mason	1·66	:	1	....	1·44	:	1	0·87
13.	Carrier	2·33	:	1	....	1·67	:	1	0·71
14.	Hawker	1·43	:	1	....	1·00	:	1	0·69
15.	Painter	3·33	:	1	....	1·87	:	1	0·56

* These numbers show the proportion of sobers in all other diseases. The greater that proportion, the smaller must be the proportion of sobers in diseases of the lungs; or, in other words, the larger must be the proportion of drinkers in diseases of the lungs.

whole employments into two groups only, this connexion becomes undeniable.

The degree of relation in question is established by inquiring into the indices of drinking tendencies affixed to each employment in Table A, adding them three by three (five groups), or five by five (three groups), or 7½ by 7½ (two groups), and then examining whether or not these sums increase from top to bottom. On considering the indices five by five, there is partly an increase, inasmuch as the first sum is 27, the second 55, and the third 38. Here the increase exists only between the two first groups; but when considered under two groups, then the increase is obvious, the sum of the drinking indices of the first group being 47·5, and that of the second 72·5. This establishes a slight connexion between the predisposition of employments to diseases of the lungs and their drinking tendencies.

It is important to observe that this table gives but a very general idea of the influence of alcohol as a predisposing cause, per employment, to diseases of the lungs, for the following reason. Supposing we consider an employment containing but very few drinkers, or in other words of very sober habits, such as shopkeepers (Index 12), it is perfectly obvious that alcohol can exert but a very small influence as predisposing that trade, *as a whole*, to pulmonary diseases; still *drinking shopkeepers* individually are found to be highly predisposed to suffer from these affections. Or, in other words, the fact that alcohol predisposes but slightly shopkeepers to disease of the lungs, results from there being very few drinking shopkeepers, and not from drinking shopkeepers being but little liable to these affections. From this consideration I have found it necessary to introduce a second table (B), consisting of two series of proportions, juxtaposed, the first series showing the proportions of sobers and drinkers in diseases of the lungs; and the second series showing the proportions of sobers and drinkers in all other diseases; on examining together these two proportions for each employment, and calculating the relation which existed between them, the result showed the actual comparative predisposition of the drinkers of each employment to diseases of the lungs. This relation is very striking in shopkeepers (No. 1). For every drinking shopkeeper who suffers from a pulmonary affection, there are 1·37 sobers affected in a similar way, while for every drinking shopkeeper applying to be treated for all other diseases, there are no less than 7·20 sobers. The relation between 7·20 and 1·37 is 5·25, therefore 5·25 represents the influence of alcohol as predisposing *drinking shopkeepers* to diseases of the lungs. Proceeding downwards with the Table B, we have engineers, coal-porters, sailors, and finally hawkers and painters; drinkers belonging to these two last employments are least of all, compared to sobers (in the same employments), predisposed to diseases of the lungs.

Why are drinking shopkeepers so much more predisposed to pulmonary affections than sober shopkeepers? Probably for the same reason they are so very liable to alcoholism (p. 491). Moreover, the respiration being deficient, the alcohol absorbed remains longer in the body, and its irritating action whilst circulating through the delicate and morbidly predisposed pulmonary capillaries is consequently prolonged. It will be observed that drinking engineers (mostly engine-drivers and inspectors) are also much more subject to suffer from affections of the lungs and air passages than sober engineers, although not in so large a ratio as shopkeepers. This may be considered as resulting from the depressive influence of alcohol, combined with the fatigue from working in confined and very hot engine rooms, and the sudden changes of temperatures which these men are so much exposed to; in addition to these circumstances, the respiration of mephitic gases from the furnaces and coal dust account for the lungs being readily affected when they are predisposed to disease by the circulation of alcohol through their tissue.

The last employment but one in the table is *hawkers;* drinking hawkers are, with one exception, the least of all, compared to sober hawkers, subject to diseases of the lungs; the index of predisposition of drinking shopkeepers to diseases of the lungs being 5·25, and that of drinking hawkers 0·69. (See Table B.) This result is perfectly in accordance with what might have been anticipated. The employments of shopkeepers and hawkers are in every respect widely different from each other. Shopkeepers lead a sedentary indoor life, taking very little exercise; hawkers are constantly moving about in the open air, many of them wheeling or carrying heavy loads. The respiratory function of shopkeepers must become more or less impaired from the mode of life they lead; in the case of hawkers, on the contrary, the action of the lungs is developed to its utmost, owing to exercise in the open air, and more especially to the cries which are the principal feature of their trade. At each inspiration nearly as much air is admitted into the lungs as they are capable of containing in the fullest state of expansion; and it is a natural consequence from this excessive respiratory action, that any alcohol present in the blood will be very rapidly expired, so that the poison does not remain in the system long enough to injure the pulmonary organs. Moreover, the constant excessive action of the open air on the lungs of hawkers would, it may be presumed, give tone to these parts, so that they become possessed of the power of resisting to a great extent the baneful action of alcohol circulating within their capillaries, and on its way outwards through the membrane of the air cells.

I shall not proceed any further with these remarks; my purpose at present is more especially to establish facts as far as a numerical method will allow, and let the reader account for the results as he thinks best.

On the Influence of Alcohol as predisposing Employments and Individuals, per Employments, to Gout and Rheumatism.

1. *The influence of alcohol as predisposing each employment, taken as a whole, to gout and rheumatism,* is shown by Table A (p. 500) for this

TABLE A. *Gout and Rheumatism—Predisposition of Employments to Gout and Rheumatism, according to their Drinking tendency.*

Indices of drinking tendency.			—	Gout and rheumatism.	All other diseases.	—	
		1	Coal porter.	1 :	1·83	Employments including most drinkers, most predisposed to gout and rheumatism.	Employments including most drinkers most predisposed to gout and rheumatism.
		3	Stableman .	1 :	2·40		
19	...	... 15	Porter . .	1 :	2·82		
		7	Mason . .	1 :	2·91		
	—	... 2	Cabman. .	1 :	3·00		
	28						
18	...	... 9	Carrier . .	1 :	3·00		Intermediate tendency to drinking, intermediate predisposition to gout and rheumatism.
		6	Labourer .	1 :	3·14		
		——10	Carpenter .	1 :	3·58		
		48					
20	...	... 4	Shoemaker.	1 :	3·62		
	41	... 12	Shopkeeper	1 :	4·28		
		... 5	Hawker . .	1 :	4·57	Employments including least drinkers, least predisposed to gout and rheumatism.	Employments including least drinkers least predisposed to gout and rheumatism.
30	...	... 13	Engineer .	1 :	4·71		
		14	Tailor . .	1 :	5·00		
		8	Sailor . .	1 :	5·60		
33	51	——11	Painter . .	1 :	8·50		
		72					

TABLE B. *Gout and Rheumatism—Predisposition of Drinkers, per Employment, to Gout and Rheumatism.*

—	Comparative proportions of Sobers and Drinkers suffering from Gout and Rheumatism.				Comparative proportions of Sobers and Drinkers suffering from all other diseases.			Indices of predisposition.[*]
	S.		D.		S.		D.	
1. Hawker . . .	0·75	:	1	...	1·14	:	1	1·52
2. Porter . . .	2·33	:	1	...	3·43	:	1	1·47
3. Shoemaker . .	0·85	:	1	...	0·96	:	1	1·13
4. Engineer . .	2·50	:	1	...	2·66	:	1	1·07
5. Labourer . .	1·21	:	1	...	1·28	:	1	1·04
6. Mason . . .	1·51	:	1	...	1·31	:	1	0·87
7. Carpenter . .	2·66	:	1	...	1·80	:	1	0·67
8. Tailor . . .	4·00	:	1	...	2·55	:	1	0·64
9. Shopkeeper . .	3·33	:	1	...	2·06	:	1	0·62
10. Stableman . .	1·50	:	1	...	0·83	:	1	0·55
11. Carrier . . .	4·00	:	1	...	1·60	:	1	0·40
12. Sailor . . .	4·00	:	1	...	1·33	:	1	0·33
13. Cabman . . .	1·50	:	1	...	0·50	:	1	0·33
14. Coal porter . .	1·	:	1	...	0·21	:	1	0·21
15. Painter . . .	all sobers			...	1·75	:	1	

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[*] These numbers show the proportions of sobers in all other diseases. The greater that proportion, the smaller must be the proportion of sobers in gout and rheumatism; or, in other words, the larger must be the proportion of drinkers in gout and rheumatism.

group of affections. The predisposing power of alcohol in the present instance is very evident, and much more distinctly marked than in the case of pulmonary affections. If the whole fifteen employments be divided into as many as five groups, we find an undeniable relation between the drinking tendencies of these five groups and their predisposition to gout and rheumatism, for with one slight exception, the sums of the indices for each group increase gradually, proceeding from the top to the bottom of the column; these sums being 19, 18, 20, 30, 33. When the fifteen employments are divided into three groups, the sums of the indices will be 28, 41, 51, in which case the difference between the sums of the indices will be sufficiently great to prove beyond doubt that employments are predisposed to gout and rheumatism, according to their drinking tendencies, within those limits. I need not allude to the division of the employments into two groups only; the sums of the indices in this case have, however, been entered into Table A (48 and 72).

2. *The influence of alcohol as predisposing drinkers compared to sobers, per employments, to gout and rheumatism,* is evinced by an inspection of Table B (p. 500) for this group of diseases.

The result in this case is very different from that derived from Table B (p. 497), for diseases of the lungs. With respect to gout and rheumatism the table under our present consideration shows that drinkers in every employment, compared with each other, are much more equally predisposed to gout and rheumatism than to diseases of the lungs; or, in other words, the nature of an employment has less influence in modifying the predisposition of drinkers (compared to sobers) to gout and rheumatism, than in the case of pulmonary affections. This conclusion is derived from the slight difference between the numbers of Table B (p. 500), in the column headed *Indices of Predisposition*, the first index—for hawkers is 1·52, the last—for coal porters is 0·21;* the corresponding numbers for diseases of the lungs were 5·25 for shopkeepers, and 0·56 for painters; this result is perfectly in accordance with that derived from Table A, for gout and rheumatism; for a moment's reflection will show, that unless drinkers in all employments were, to a certain extent, equally predisposed to the group of affections under our present consideration, it would not be possible that the employments, three by three, should be predisposed to these disorders proportionally to their drinking tendency.

I shall not attempt to proceed any further with this inquiry, lest I should enter into such minute details as are inconsistent with the accuracy of the numerical method of investigation I have adopted. I trust the researches and conclusions which form the subject of the present communication may be of interest and practical utility; at all events, I feel assured there are few questions so important, in a medical and social point of view, as the influence of the abuse of alcohol as a predisposing cause of disease.

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* The last in the column are the painters; but the number of these rheumatic and gouty patients is too small to take them into account on this occasion.

ART. IV.

On the Nithsdale Neck, or Goître, in Scotland. By ARTHUR MITCHELL, A.M. & M.D., Deputy Commissioner in Lunacy.

THE connexion between goître and cretinism being generally regarded as very intimate, it appeared to me of interest to ascertain whether bronchocele prevails in any part of Scotland, and if so, whether the form of idiocy in that district presents any of the cretinoïd characters—the nature of my duties giving me the opportunities necessary for doing this. I have already officially visited more than three-fourths of the parishes of Scotland, and with much of the rest I am well acquainted. In every locality visited I have endeavoured, by careful personal observation, and by frequent inquiries, to determine the existence or non-existence of goître as an endemic affection; and of all that I saw and heard I have preserved written notes. The general result is briefly as follows:

Goître exists nowhere in Scotland, as an endemic disease, except in the southern counties. The extent of the district which it occupies may be laid down as including the greater part of Roxburgh, the upper parts of Selkirk and Peebles, the eastern parts of Ayrshire, where it touches Lanark and Dumfries, the upper districts of Lanark, the whole of Kirkcudbright and Dumfries, the west of Berwick, and the east parishes of Wigton.

The endemicity reaches a maximum in the upper Valley of the Nith. As you approach the outer margins of the whole district named, the affection becomes less and less frequent; but even there, as compared with the rest of Scotland, it is common. In the counties of Dumfries and Kirkcudbright I had myself an opportunity of seeing 93 cases, and of these about 60 were well marked, the rest being less so, but still undoubted cases of goître. A considerable proportion of the 93 were seen in Upper Nithsdale, and I have therefore given to the affection the name of the *Nithsdale neck.*

The accuracy of my own impressions, as formed on observation and from verbal reports, I tested in various ways, so as to leave as little doubt as possible about the correctness of the conclusions.

Our asylums, for instance, furnish a proof of the comparatively great frequency of this affection in the southern counties. They at the same time show, so far at least as *acquired insanity** is concerned, not only that there is no necessary or intimate connexion between the two diseases, but that there is possibly no connexion at all. In other words, it does not appear that the insane from an ungoîtrous district will, because of their insanity, be goîtrous; nor that the insane from a goîtrous district will be found more liable to goître than the ordinary population, *living under similar health conditions.*

I communicated with the physicians of our public asylums, as also with those of the licensed poor-houses and private asylums, requesting information as to the number of goîtrous persons among the insane under their care, and as to the parishes from which these had come, and the form of mental malady under which they laboured. I have pleasure in stating that this information was in nearly every instance furnished to me, and the result I now embody in a tabular form.

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* By this I refer to mania, melancholia, dementia, &c., or those forms of insanity which usually appear after puberty.

The table is drawn up from statements in writing furnished by the medical gentlemen residing in the various establishments, or professionally in charge of them. In no instance have I used observations made by myself.

Name of Institution.	No. of inmates, as on 1 Jan. 1859.	No. of patients affected with goitre.	County to which these belong.	Remarks.
Royal Asylums: 1. Morningside; 2. Montrose; 3. Perth; and 4. Dundee	1321	0	—	—
Private Asylums; 1. Garngad; 2. Baldovan; 3, Longdale; 4. Lillyblank; 5. Newbigging; 6. Hallcross; 7. Campie Lane; 8. Millholme; 9. Springbank; 10. Gilmer House; and 11. Eastport	627	0	—	—
Lunatic Wards of Poorhouses: 1. Cunningham Combination; 2. Govan; 3. Inverness; 4. Falkirk; 5. Stirling; 6. Rhins of Galloway; 7. Greenock; 8. Barony; 9. S. Leith; 10. City of Glasgow; 11. Abbey; 12. Burgh; 13. St. Nicholas	546	0	—	—
Sum	2494	0	—	—
Royal Asylum, Aberdeen	290	3	Aberdeen	One small, but distinct; one large, but decreasing; and one dubious.
Royal Asylum, Gartnavel	504	2	Caithness. Dumbarton.	
Sum	3288	5	—	—
Crichton Institution and Southern Counties Asylum, Dumfries	447	17 5 3 1 1 3 30	Dumfries. Kirkcudbright. Wigton. Ayr. Lanark. England.	15 marked, 13 slight, and 2 doubtful.*
Total	3735	35		

From this it appears that in twenty-seven of the institutions for the insane in Scotland, with a population of two thousand four hundred and ninety-four, there is not a single case of goître. In two others, with 794 patients, there are 5 cases; while in Dumfries alone, which is the district asylum for the southern counties, there are 30 patients affected with goître out of a population of 447. It appears still more striking, when I state that 26 of the 30 are found in that division of the Dumfries Asylum which specially serves the district, which may be regarded as filled from it, and which has only 277 inmates; and further, that, with two exceptions, all of the 26 are entered as belonging to the counties which I have called goîtrous. In other words, among 3288 lunatics found in twenty-nine asylums scattered over Scotland, only 5 goîtres occur, while in one-seventh of that number found in the Asylum at Dumfries, there are six times as many. Had the proportions been equal, instead of 5 there ought to have been 221.

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* Of the whole 35 cases, 26 are females and 9 males. The majority labour under dementia, but cases of melancholia, mania, monomania, and idiocy are included.

It was not without satisfaction that I found these facts so fully supporting my own impressions. Sporadic cases of enlargement of the thyroïd I have encountered here and there all over Scotland, in the sane and insane, and I look upon the five cases out of the Dumfries Asylum as of this nature.

I would here again remark, that the foregoing table, supported by all that I have observed myself, goes to show that a lunatic—be he imbecile, idiotic, demented, melancholic, or maniacal—appears to have no predisposition to this affection, in virtue directly of his lunacy, in so far at least as Scotland is concerned.

Assuming that on the whole the boundaries which I have assigned to the goîtrous district are correct, I shall now endeavour to show that the maximum of intensity occurs, as I have asserted, in the upper valley of the Nith. In addition to what I had opportunities of observing myself, I have on this point the advantage of possessing the opinions of many of the resident medical men, partly through personal intercourse, and partly by correspondence.

Dr. Chalmers, of Thornhill, in Nithsdale, informs me that in the poor-house there, of fourteen females above the age of twelve, eight furnish "*unmistakeable evidence of goître*," or more than one-half. As is usually the case, males are less liable to the affection than females, and accordingly in six men above the age of twelve it occurs only once, and that not very distinctly. He further states it as his opinion, "that about *one* woman in *ten* is affected with the disease in that part of the country." Dr. Grierson is substantially of the same opinion.

Dr. Kay, of Sanquhar, communicates his own view and that of Mr. Laurie, in these words—"We are still of opinion that every fifth or sixth female here has more or less of it."

What I saw myself led me to conclusions as to its extent very nearly the same as the foregoing.

In one of the annual reports of the Crichton Royal Institution, Dr. Browne states that during the preceding year not less than eight cases in the valley of the Nith had been under observation, in which goître was complicated with some form of mental disease.

Dr. Carlyle considers the disease to be "of frequent occurrence" in the neighbourhood of Langholm; and Dr. MacLeod writes me, that it "prevails to a great extent" in the district about Hawick. It is also very general about Wanlockhead; and in the Castle Douglas and New Galloway districts it is far from uncommon. As you enter Wigton from Kirkcudbright, and proceed westward, it seems gradually to decline in frequency. Over the whole district it prevails, but nowhere does its force appear to be so great as in Upper Nithsdale.

Taking the accuracy of this for granted, I am now led to the interesting question—Do the forms of idiocy in these districts differ in any respect from the forms which are found throughout the rest of Scotland? or, more precisely, Does cretinous idiocy appear to occur in association with the goîtrous manifestations? It becomes necessary, however, before answering this question, to define what I regard as a cretin.

The most distinctive characteristics are to be found in the physiognomy and in the condition of the skin.

As regards the first, the nose of a cretin, which is short and flat, is always depressed and broad at its root; the eyelids are loose and flabby; the lips thick and swollen; the general face prognathous; the mouth open; the tongue large and often protruding; the teeth irregular and decayed; the palate high and narrow; the cheeks and face generally full, loose, and wrinkled; the hair thin, and in the adult often wholly absent on the chin, in the axillæ, and on the pubis; the complexion blanched, and the expression dull, heavy, stupid, and child-like.

The state of the skin determines many of these physiognomical traits; but further, over the whole body the skin appears to be too large, lying sometimes in deep folds, having seemingly undergone a relative hypertrophy, or the skeleton a relative atrophy.

The head is small and retreating anteriorly; yet it has often a large look, and occasionally it is absolutely large. It may, however, be either small or large, long or short, there being no constant character either as to form or size.

The tendency to general dwarfage is always present, and the members are usually out of proportion to each other, as, for instance, a big foot or hand on a short leg or arm. These peculiarities are, of course, not equally strong in every case, the range of difference being great.*

The mental phenomena are those of idiocy, but more, perhaps, than in any other form is it purely the infant mind—a general negation of mind, or arrest of development, without distortion, perversion, or irregularity. There is a dull, slow, heavy, passive, oppressed stupidity. The extent of the mental negation differs widely, ranging from imbecility to the most absolute idiocy.

In part, the physiognomy is probably referable to premature synostosis of the occiput to the sphenoïd and of the anterior and posterior sphenoïds to each other, as Virchow has pointed out.

As is well known, this form of idiocy occurs with great frequency in certain localities, and such localities, so far as I know, never exist without a coincident frequency of goître. Goître may, and often does, extensively prevail without cretinism, but not the latter without the former. Nevertheless, I am inclined to think that cretinism cannot be regarded merely as the highest manifestation of the goîtrous influence: that influence, whatever it is, must be plus or minus something beyond mere force, in order to induce cretinism, *or* there may be two distinct though coexisting and allied influences.

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* I think this is very much what would answer to Virchow's cretin; but it is given here as drawn from what I have myself observed.

Two things further must be borne in mind: a cretin in a goîtrous locality is far from being himself necessarily goîtrous, and again all idiots in goîtro-cretinous localities are not of necessity cretins. The causes of the general forms of idiocy exist in such places, as elsewhere, and their product is the same. Cretinism is not endemic idiocy, but an endemic form of idiocy. And, though usually endemic, sporadic cases of cretinism, as of goître, may occur, and these are not distinguishable from the cretinism of locality, being identical with it in the characteristics of configuration, physiognomy, and mind, and therefore as truly cretinism as sporadic goître is goître.

So far as Scotland is concerned, these sporadic cases of cretinism are few—not in all, I believe, more than thirty or forty. Idiots answering to the portraiture given are rarely met, and I have found no locality in Scotland in which they are encountered with greater frequency than in others. No local influences, in short, appear to operate in their production.

The question which I proposed is therefore answered. Cretinism is rare throughout the whole of Scotland, and it appears to affect no district, the goîtrous one of which I write being included. Out of a large number of idiots seen in the three south-western counties, only two were regarded as cretinous, and one of these was born in Ireland, and the other in England. Idiocy in the goîtrous districts was not found to be in any respect modified or affected, either as regards form or frequency. I endeavoured carefully to investigate this point, and I feel satisfied that my conclusion is accurate. I *expected* to find it otherwise. I thought that in comparing the idiocy of this district with that of the districts to the North and East of Scotland, I should detect in the contrast some cretinous tendencies, though I might not find full cretinism. But it was not so. I was unable to discover any difference.*

The *cause* of goître is one of the great *questiones vexatæ* in the profession, and I am not in a position to attempt its solution. I have, however, collected a few facts, which illustrate some of the characteristics of its only Scotch habitat, and which it may be well to put on record.

Of late much has been said of the climate or meteorology of goîtrous districts. This is but a renewal of the attention which Foderé gave to it in his classical work on cretinism.

As concerns the county of Dumfries, which embraces the whole region of maximum intensity, I am fortunately able to give a minute description of some of the elements of climate. There is certainly no other part of Great Britain, and it is probable that there is no other part of Europe, where, in the same number of square miles, observations of rainfall and temperature have been made for a series of years at so many points. For this we are indebted chiefly to the Duke of Buccleuch, whose enlightened interest in agriculture led him to establish a large number of meteorological stations. He was ably seconded by Mr. Stuart of Hillside, Mr. Little of Carlesgill, Dr. Russel of Thornhill, and by the late Dr. Dunbar of Applegarth. I am indebted to these gentlemen for the facts which I have brought together and arranged. I am also indebted to Mr. Burgess, the late Secretary of the Scottish Meteorological Society, for the trouble he has taken in procuring information for me where blanks occurred.

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* Mr. Greenhow, who has had large opportunities of studying goître at Segowlee, writes in the 'Medical Times and Gazette' of Nov. 30, 1861, as follows: "Among all the goîtrous patients treated by me, no cretins were met with, yet there are many in the country round. These persons are doubtless often goîtrous, for they are exposed to the same causes of goître as others; but if many hundreds of goîtrous persons are brought promiscuously together and no cretins are found among them, there is strong *primâ facie* proof that the two diseases are not necessarily connected."

I shall divide the stations into three sets, according to their situation in one or other of the three great valleys formed by the Nith, the Annan, and the Esk. This, which is the natural arrangement, will include at least seven-tenths of the whole county. These valleys have not the east and west direction which is usual in Scotland. They run from north to south, all opening into the Solway. This is a feature of some interest, as westerly and south-westerly winds, before reaching them, must be cooled by passing over high ground, and must have a part of the moisture with which they are loaded, condensed, and ready to fall as rain.

The mean elevation of the stations is great, and in one case exceeds that of any station in Scotland by 220 feet. I refer to Wanlockhead, which stands higher than any other village north of the Tweed. This elevation of course is itself a cause of an increased rainfall, but it is far from being sufficient to explain the excess which occurs in this district, since at other stations equally high no such result is observed.

The three following tables exhibit the mean annual rainfall at various stations in each of the three great valleys. The height of the stations above the sea-level is also given, and the number of years from which the averages are calculated:

TABLE I.—*Upper and Middle Nithsdale Rainfall.*

Station.	Height above sea level.	Quantity of rain in inches per annum.	No. of years from which means are calculated.
Morton, Thornhill	244	39·9	5
Durrisdeer, School	459	32·7	3
Closeburn, Wallacehall	206	31·3	4
Penpont, Manse	195	37·8	5
Keir, Manse	155	42·0	4
Tynron, Auchenbrack	629	46·0	5
Glencairn, Hastingshall	354	54·5	5
Glencairn, Manse	534	49·8	2
Kirkconnel, School	534	41·2	5
Sanquhar, Crichton ditto	499	39·8	4
Wanlockhead	1564	57·0	4
Drumlanrig	186	45·6	3

TABLE II.—*Upper and Middle Annandale.*

Station.	Height above sea level in feet.		Average annual rainfall in inches.		No. of years from which foregoing averages are calculated.
Hutton, Corrie School	680	...	47·5	...	3
Dryfesdale, Lockerbie ditto	296	...	38·4	...	3
Johnstone, Goodhope ditto	374	...	47·3	...	3
Wamphray, ditto	317	...	41·6	...	3
Applegarth, Sandyholm	224	...	34·0	...	24
Kirkpatrick-Juxta School	338	...	41·5	...	4
Moffat, Town	348	...	43·6	...	2
Moffat, Ewes Water School	549	...	45·0	...	3
Kirkmichael, School	239	...	37·2	...	3
Lochmaben, School	171	...	36·8	...	3
LOWER ANNANDALE:					
Dalton, Hardgrave		...	34·2	...	1

TABLE III.—*Eskdale.*

Station.	Height above sea level in feet.		Average annual fall of rain in inches.		No. of years from which foregoing average is calculated.
Canonbie, Woodhouselees	120	...	37·8	...	18
Canonbie, School	140	...	34·2	...	5
Langholm, School	270	...	42·8	...	5
Ewes, School	407	...	45·2	...	5
Carlesgill	370	...	55·6	...	17
Carlesgill, Hill-top	1164	...	53·8	...	4
Westerkirk, School	420	...	48·2	...	5
Eskdalemuir, ditto	612	...	52·1	...	5
Eskdale, Pentop	2268	...	67·0	...	4

The difference which occasionally exists between two near stations probably originates in some peculiarity in the position of the instruments. The general teaching of the table, however, is clear, and is not affected by such differences. That much rain falls in the district is an unavoidable conclusion, and that more falls in it than in any other district of Scotland is rendered equally clear by the following table:

RAINFALL OF SCOTLAND.

Mean of four years from 1856 *to* 1859.

	No. of stations.		Average height of stations above sea level.		Average rainfall in inches.
North District	7	...	62	...	38·6
East ditto	31	...	251	...	28·7
South ditto	18	...	578	...	47·1
West ditto	10	...	314	...	45·5
All Scotland	66	...	314	...	37·7

It thus appears that the mean annual rainfall in the eighteen southern stations, which are all in Dumfriesshire, exceeds that for all Scotland by ten inches, and by two inches exceeds that of the proverbially rainy west. Mr. Stewart, of Hillside, says that the fall of rain on the east side of Scotland is not more than two-thirds of that which falls in the *upper districts* of Dumfriesshire, and about three-fourths of what falls on *the sea coast of that county.*

In every sense it must be regarded as the wettest district of our country, for not only does more rain fall, as has just been shown, but it also falls on a greater number of days. Rain falls on an average in Scotland on 161·5 days during the year, while at Carlesgill, 370 feet above sea-level, the mean of nine years gives 194 days, and in 1857 the number rose to 201. At Applegarth, again, a mean of twenty-four years shows 188; and even at Dumfries, at the sea-level and clear of the dale, an average of six years gives no less than 189. At the last place, it occasionally greatly exceeds the average; in 1849 rising to 203, and in 1850 to 209, which last represents three-fifths of the whole year. It is, in fact, the great rainfall which makes the upper parts of these three valleys better for grass and green crops than for corn. In Eskdale, wheat is not at all cultivated, and in all three oats are the favourite crop. Temperature, of course, has also to do with this, and to it we shall now direct attention.

The two following tables (V. and VI.) exhibit the mean temperature of Upper and Middle Nithsdale and Annandale, and embrace observations from eighteen stations:

TABLE V.—*Middle and Upper Nithsdale.*

Elevation above sea. Feet.	Station.	Mean annual temp.	No. of years from which means are calculated.
186	Drumlanrig	47°·0	4
244	Morton Thornhill	44·8	1
155	Keir, Manse	45·5	1
629	Tynron, Auchenbrack	44·7	1
354	Glencairn, Hastingshall	43·8	1
534	Kirkconnel, School	42·3	2
499	Sanquhar, Chrichton ditto	45·8	3
1564	Wanlockhead	40·6	1
	Average	44·9	

TABLE VI.—*Upper and Middle Annandale.*

Above sea level. Feet.	Stations.	Mean tempe-	No. of years from which means are calculated.
680	Hutton, Corrie School	42°·5	2
296	Dryfesdale, Lockerbie ditto	45·9	2
374	Johnstone, Goodhope ditto	42·8	2
317	Wamphray School	43·7	2
224	Applegarth, Manse	46·3	30
338	Kirkpatrick-Juxta School	46·5	3
348	Moffat Town	45·0	1
549	Ditto, Ewes Water School	43·0	2
239	Kirkmichael, School	45·3	1
171	Lochmaben, ditto	45·0	2
	Average	45·6	

From these tables it appears that the mean temperature for the two districts is 45°·2, or nearly two degrees below that for all Scotland, which is 47°·0. There is perhaps no large tract of land in Scotland of equal average elevation with that whose climate I am describing, and this, I doubt not, accounts for much of this difference; but it does not explain all, since there still remains a difference in the same direction after the correction for elevation has been applied. Latitude, again, ought to give an opposite result; yet, in fact, though it is the south district of Scotland, it has a temperature below the mean for the whole country, and below that of many stations in the far north. Elevation, distance from the sea, and *local peculiarities or configuration*, probably unite in producing the effect. The range between summer and winter temperatures ought to be reduced by the greater general elevation of the stations. This, however, does not appear to be the case. An examination of the various documents condensed into the two short tables which precede these remarks, shows a summer temperature above and a winter temperature below the average for the whole country.

I have no materials for the examination of the other elements of climate, but it is clear that both rainfall and temperature are out of rule and exceptional. I do not, however, wish it to be understood that I regard or do not regard this as in any way the cause of goître. A disease prevails here which does not prevail elsewhere in the country, and it is desirable to ascertain what other conditions are found which are not found elsewhere. This appears to me to be the correct procedure. No effort is at present to be made to establish a connexion or dependence between the one and any other or all other peculiarities which may be discovered. The facts, however, may yet become available for such a purpose, and are therefore of interest and importance.

On another point, the quality of the waters used by the people for domestic purposes, I have some remarks to offer.

The presence or absence of iodine in such waters, notwithstanding all that Chatin and others have written on the subject, I did not attempt to determine, as I regarded the researches of Dr. Stevenson Macadam conclusive as to the uselessness of such an inquiry.

Lime in large quantities has been so often charged with causing this affection, that I embraced the opportunity of determining its amount in several of the waters used by the people of Upper Nithsdale. This was kindly done for me by Dr. Stevenson Macadam and his assistant, Mr. McHattie. The specimens were selected by Drs. Kay and Chalmers, and are all said to be largely used by the inhabitants. I subjoin the results of the analyses in a tabular form :

TABLE VII.—*Partial Analysis of some of the Drinking and Cooking Waters of Upper Nithsdale.*

	Solid matter.		
	Residue in one imperial gallon.	Lime, estimated as carbonate.	Magnesia.
I. Water from Doctor's pump, Sanquhar. Much used. Considered harder than V. . .	33·44	14·08	Very decided amount.
II. Water from Crichton School pump, Sanquhar. Much used by the school children and others. Considered very hard	30·40	10·08	Ditto.
III. Spring water from neighbourhood of Thornhill. Much used by the people generally	32·80	7·52	Ditto.
IV. Water from Queensberry-square well, Sanquhar, which the children attending the parish school drink. Used by the inhabitants of the district .	14·24	5·60	Considerable, but smaller than 1, 2, 3, & 7.
V. Town-foot pump, Sanquhar. Much used, and considered the pure and good	11·36	4·16	Ditto.
VI. Water from Kirk Sykes, Sanquhar. Considered to be soft, and much used. It issues from St. Bride's Well, and flows through some meadow land to the place from which it is taken for use	11·20	4·16	Very decided amount.
VII. Well water, Thornhill. Largely used by the inhabitants . .	8·80	3·52	Considerable, but smaller than 1, 2, 3.
VIII. Chalybeate spring, Sanquhar. (Analysis by Dr. Penney) .	14·71	5·65	Carbonate of magnesia, 0·65.

It will be seen from this table that lime is present in large quantities in several of these waters, but in the majority the amount is not excessive. I know many waters, however, much richer in lime than the richest of these, and which are habitually used, yet never give goître as a result. Of one I subjoin an analysis which shows more lime by 75 per cent. than any of the Nithsdale waters:

TABLE VIII.

	Solid matter.		
	Residue in one imperial gallon.	Total lime, estimated as carbonate.	Magnesia.
Water from a well much used by the inhabitants of a district where goître is unknown . .	32·64	24·33	Considerable.

These results give support to the increasing distrust in the lime theory. All the specimens contain considerable quantities of magnesia, as also does the water of Table VIII. Numerous writers have attributed the evil to the presence of this body alone or along with lime. Grange and Bouchardat have written strongly in support of this view. Recently, however, during the march of the French army through Lombardy, an analysis was made at Milan of thirty specimens of waters from goîtrous districts, and in all there was a total absence of magnesia. It appears, therefore, that it may exist without goître, or be absent with it, and consequently that there is no necessary relation between the occurrence of the disease and the presence or absence of magnesia in the water. No decided peculiarities, in short, have been detected in the waters of this district, and I am inclined to think that more extended researches would give the same negative results.

The physical configuration of the locality also deserves notice. The great valleys of Scotland, as a rule, run from east to west, or *vice versâ*, but here the three great dales run from north to south. One effect of this, as has been already shown, is an increase of the rainfall; another is a late sunrise and an early sunset, and a consequent curtailment of light. The valleys, however, are neither narrow nor confined, and Upper Nithsdale in particular, at two or three points, widens into circular basins, without any seeming inlet or outlet. Sanquhar is situated in one of these, and in the other, Closeburn and Morton. Prof. Jamieson draws attention to this feature as one of interest. He states that the same thing is to be seen in Strathdon,* and in the valleys of the Rhine, Danube, and Elbe. There certainly is a striking resemblance between Upper Strathdon, some of the expansions of the valleys of the three Continental rivers, and the valleys or basins of Sanquhar, Closeburn, and Annan.

"The sides of the valleys are generally smooth and covered with vegetation," and their "bottoms are usually covered by alluvial, or water-worn land."†

After leaving the shores of the Solway, nearly the whole of the county is composed of transition rocks, chiefly greywacke and greywacke slate, but also, here and there, a flinty slate, alum slate, and transition greenstone. On looking at Nicholl's or Knap's 'Geological Map of Scotland,' this is at once seen. There is, in fact, no tract of country in Scotland at all of the same magnitude consisting of this formation.

Upon these rocks as a basis, we find at various points the "independent coal formation," as at Kirkconnel, Sanquhar, Closeburn, Whitehill, Cornock-muir, Chapelhill near Moffat, &c.‡

This formation occurs at various points along each of the three valleys. It occupies a considerable portion of the bottom of the

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* I have made careful inquiries as to the existence of goître in Strathdon. Only two cases were heard of: one large and of long standing, and the other doubtful. All unite in pronouncing the disease a very rare one in that locality.

† Jamieson's Geology of Dumfries, pp. 12 and 13.

‡ This brief and necessarily imperfect sketch of the geological features of the country is confined chiefly to those parts of it which are removed somewhat from the Solway.

Valley of Sanquhar and Kirkconnel, resting there as elsewhere on transition rocks. Good coal, a continuation of the Ayrshire bed, is worked above Kirkconnel, but nowhere below that. The sandstone (new) at Sanquhar is of a greyish white colour, but lower down in the Closeburn valley it becomes a reddish brown. At Closeburn and Barjarg, stratified limestone, belonging to the same formation, is largely worked. Lower down still, towards Dumfries, the sandstone retains its darker colour.

Patches of the same formation occur along the vale of the Annan, as near Lochmaben, at Rotchel in St. Mungo, &c. In the lower part of this valley, at Ecclefechan and Kellhead, the same limestone appears as at Closeburn and Barjarg. No coal is found in Annandale, though often looked for.

Eskdale, as far down as Langholm, is almost entirely composed of transition rock. Below that the more recent formation appears, with a whitish sandstone and a bluish grey limestone.

For this very brief notice of the geological peculiarities of Dumfries, I am indebted to the works of Professor Jamieson and Nicholl. Its general accuracy I had an opportunity of confirming by personal observation.

Kombst regards the inhabitants as Pictish Scandinavian, exactly the same race as the people of the northern counties of England, and differing, therefore, from the rest of Scotland. Whether he is or is not correct in this I am unable to say, nor do I think it of much importance. It is of importance, however, to know that, as a people, they are intelligent, energetic, and generally well-doing and prosperous. They are tall, active, and well-made. Physical deformity or defect is not frequent. Agriculture is the chief employment, but weaving is largely carried on in Sanquhar, and there is a mining population at Kirkconnell and Wanlockhead, while professions and trades have their usual number of followers. The dwellings in general are good and sufficient. Those of the poorest are greatly superior to the houses occupied by the same class in the North of Scotland, and especially in the north-west. As a rule, the people appear to be comfortably and warmly clothed, and without any peculiarity of costume. Their eating and drinking differ in no respect from what is usual in other parts of our country.

Ague is not now known in the district. The people die of the same diseases there as elsewhere in Scotland.

The pathogeny of the district, however, is distinguished by another peculiarity in addition to the goîtrous one, and in this respect also it stands alone in Scotland.

My attention was first directed to this by Boudin,* whom nothing seems to escape, and who says, "Il règne, ou il a règné autrefois en Ecosse, et notamment dans les comtés d'Ayr, de Galloway, et de Dumfries, une maladie spéciale, offrant des analogies avec le pian, et décrite par les auteurs sous le nom de *sibbens*, ou *sivvens*." I afterwards saw the following remark by Mr. Joseph Duncan, in the 'New Statistical

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* Géog. Méd. tom. ii. p. 702.

Account."* "Sibbens, a disease almost peculiar to Dumfriesshire, Galloway, and the West Coast of Scotland, is of frequent occurrence here among the dissipated and squalid." This refers to the parish of Dumfries. In 1754, Gilchrist wrote a paper on it, in the 'Physical and Literary Essays of Edinburgh;' and Dr. Hill, of Dumfries, in 1772, wrote a small treatise, which I have seen, entitled 'Observations on Sibbens.' I believe Dr. Wells also wrote on the *Yaws*, an analogous affection, as occurring at Cumnock. There cannot be a doubt, then, as to the existence of the disease in former times, and I was assured by various medical men that it still exists, though it is now not common. No case happened to come under my notice. That it is infectious all seem to admit, but there is great diversity of opinion as to its real nature. Bell† and Swediaur‡ regard it as a variety of the venereal disease, and so do many others; but the difference appears to be in some points well marked, as, for instance, in the mode of propagation; and Dr. Adams, a good authority, regards it as the result of a poison *sui generis.*§ Its presence in that part of Scotland, and in that part only, which is the territory of goître I regard as accidental, and I am not able to see any connexion between the two diseases, nor do I think it probable that any such connexion exists.

I shall conclude this paper with a few facts illustrative of the natural history of goître as it occurs in Scotland, and I shall give them without any comparison with what has been found in other countries, beyond the general remark that such a comparison would have exhibited no striking points of difference.

The goîtres which present themselves in Scotland seldom assume an aggravated form, but occasionally they do so; and I heard of three or four deaths attributed to this disease. In the largest which I had an opportunity of seeing, the gland was increased to the size of two oranges. Usually, however, the swelling is not equal to one orange. When this embraces the three lobes it does not cause inconvenience, and when the patient is in good condition the disfigurement is slight. Medical advice in such cases is rarely asked. When it is applied for, the preparations of iodine, externally and internally, are prescribed, and generally with good results. My attention was frequently called to what appeared to be an abnormal liability to iodism on the part of patients in the goîtrous district. I felt inclined, however, to attribute this to overdosing by those who, in their haste to be well, had exceeded the orders of their advisers.

The female sex is much more liable to bronchocele than the male. This important fact, I think, should be borne in mind in any inquiry into the causes of goître. In Scotland, eighty or ninety per cent. of all cases will be found to be women. It seldom appears before the approach of puberty; but I was assured, on competent and trustworthy authority, that cases of congenital goître are occasionally encountered. Such, undoubtedly, is the fact in the Swiss valleys.

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* Vol. iv. p. 2. 1845.

† Treatise on Gonorrhœa Virulenta and Lues Venerea, vol. ii. p. 224.

‡ Practical Observations on the more Obstinate Venereal Complaints, p. 175.

§ Whitehead on Hereditary Diseases, p. 65.

In nearly all the cases which I saw, the right lobe was more enlarged than the left. In very many it alone was affected. In no case was the left lobe either larger than the right, or alone the subject of the disease.

The size of the tumour fluctuates considerably, and sometimes periodically, either with the seasons or with some physiological condition of the patient. Spring appears to be the season of maximum size. During the period of menstruation there is often a temporary increase, which is said to be still more marked during pregnancy and lactation.

I had sufficient evidence that no class of society in the district has an immunity; that there does not exist, even, a difference in the degree of liability, I heard often asserted.

The majority of the cases which I saw were in young women from fifteen to thirty years old. Most of these were stout, clear complexioned, well made, and healthy-looking. Its origin did not appear to be associated with impairment of the general health; but when it had lasted long, and *resisted treatment*, I observed slight emaciation, or rather a departure from *embonpoint*, a sallowness of complexion, and a dryness and roughness of the skin. Dyspepsia, too, was in such cases often complained of. I saw this so frequently as to make me regard it as nearly constant. The sufferers, however, generally blamed the iodine and not the goître.

I heard of several persons who had apparently acquired the disease by coming to reside in the locality, and of others who, when labouring under it, had obtained a cure simply by going to live in a county where the affection was unknown. These cases, however, were not seen; but I did see two or three females, who had left Nithsdale with goîtres, and had resided for years in Fifeshire and elsewhere, and who had returned in no degree improved.

More instances were seen in the villages than in the wide-spread rural or agricultural population; but not more in proportion to the number of the inhabitants. This impression was confirmed by those who had more accurate and more extensive local information than myself.

ART. V.

The Influence of Mercury upon the Urine. By EDWARD R. HARVEY, M.A., M.B. Oxford.

THE following experiments were made with the view of determining what effect, if any, the administration of mercury had upon the secretion of urine among the lower animals. In spite of the numerous experiments that have been carried out, the action of mercury upon the body remains sufficiently a matter of conjecture to make further observations desirable, and although experiments upon animals are open to the objection that the results arrived at lead to no certain conclusion as to the effect the same course of treatment would exercise upon man, yet from the fact that mercury produces certain effects common to man and the lower animals—such, for example, as salivation and ulceration of the mouth—it is fair to conclude that in the case of this drug, experiments upon animals are likely to be of value to the physician, and are certainly worthy of his attention.

To obtain the urine voided, a cage was constructed three feet long, two feet six inches wide, and three feet high. The sides and top were of wood, the lower part of the sides for twenty inches being lined with zinc, and the upper part and roof, which was attached by hinges, were formed of wooden bars an inch and a half apart, to admit air and daylight. The bottom was of the same material as the zinc sides, to which it was united, and was shaped like a kitchen dripping-pan, so that all the urine flowed into a well, the well being perforated with small holes, and so fastened as to be easily removed for the purpose of cleansing. Within the cage was placed a false bottom of strong galvanized iron wire sufficiently removed from the sides and bottom to permit of no hindrance in the flow of urine. This, like the well, was daily removed and cleaned. Upon it the animal lived. The cage thus constructed was placed upon a four-legged stand, which allowed a bottle with a funnel to stand beneath the well and receive the urine as it ran from the cage. To keep the cage quite clean, and so prevent any decomposition of the urine, it was daily washed with buckets of water, and occasionally with a dilute solution of acetic or hydrochloric acid. Young dogs and puppies were used in preference to older animals, on account of the readiness with which they submit to confinement.

On November 1st, 1860, a puppy five months old was placed in the cage and fed daily upon six ounces of paunch as sold at the dog's-meat shops, with half a pint of water. The whole of the urine passed every twenty-four hours was measured, its specific gravity taken, and the urea, phosphoric acid, and entire ash were estimated. The urea was determined by Liebig's standard solution of the nitrate of mercury; the phosphoric acid by Dr. Bence Jones's method with chloride of calcium and ammonia. The gramme measure was used throughout the experiments.

Table I. shows the amount of urine, &c., passed each day for five days by the puppy, and the average daily quantity has been calculated and is placed on the last line of the table. The same plan is adhered to throughout the tables.

TABLE I.—*The Amount of Urine, and of certain of its Contents, passed daily by a Puppy five months old.*

Date.	Urine. Grammes.	Specific gravity.	Urea. Grammes.	Ash. Grammes.	Entire Phosphates. Grammes.
Nov. 1st to 2nd . .	233·250	1040	12·100	2·275	0·490
„ 2nd to 3rd . .	201·480	1040	10·450	0·940	0·306
„ 3rd to 4th . .	435·400	1035	20·950	2·138	0·331
„ 4th to 5th . .	435·400	1035	20·950	2·131	0·330
„ 5th to 6th . .	162·340	1050	8·660	2·974	0·180
Average daily amount	293·574	1040	14·622	2·092	0·328

From November 6th to 9th, the puppy being fed as before, took daily two and a half grains of blue pill. On the 9th the mouth was ulcerated, and the dog was removed from the cage. He died on the 10th.

TABLE II.—*The Amount of Urine and of certain of its Contents passed daily by the same Puppy as in Table I., when under Mercury.*

Date.	Urine. Grammes.	Specific gravity.	Urea. Grammes.	Ash. Grammes.	Entire Phosphates. Grammes.
Nov. 6th to 7th	233·250	1050	13·130	3·571	0·169
„ 7th to 8th	149·280	1044	8·190	1·900	0·143
„ 8th to 9th	149·280	1044	8·190	1·879	0·137
Average daily amount	177·270	1046	9·837	2·450	0·150

Judging from the large quantities of blue pill that were taken without detriment by other dogs, it is probable that the illness of this puppy, due to other causes than the mercury, operated upon the urine, and produced the great diminution in quantity, as well as in the amount of urea and phosphates, that is to be observed. By themselves the experiments are consequently of little value, but they become interesting when compared with subsequent results.

On November 9th a young dog was placed in the cage, and fed daily for seven days upon twelve ounces of paunch with half a pint of water. The results are stated in Table III.

TABLE III.—*The Amount of Urine, and of certain of its Contents, passed daily by a Young Dog.*

Date.	Urine. Grammes.	Specific gravity.	Urea. Grammes.	Ash. Grammes.	Entire Phosphates. Grammes.
Nov. 9th to 10th	174·570	1046	11·700	2·456	0·396
„ 10th to 11th	220·300	1042	13·576	2·677	0·390
„ 11th to 12th	269·000	1034	15·200	1·542	0·386
„ 12th to 13th	115·651	1056	10·438	1·860	0·395
„ 13th to 14th	216·726	1042	13·609	1·945	0·360
„ 14th to 15th	202·060	1042	13·000	1·963	0·366
„ 15th to 16th	224·000	1040	13·510	2·620	0·394
Average daily amount	203·187	1043	13·000	2·152	0·384

From November 16th to 30th the dog was kept on the same diet, and took daily doses of blue pill, beginning with two grains and a half, which were gradually increased to fifteen grains. In the fortnight he swallowed exactly one hundred grains. Beyond a slight tendency on two days to diarrhœa, the animal was in perfect health, always hungry and lively. Mercury was found in abundance in the fæces. Table IV. shows the results.

TABLE IV.—*The Amount of Urine, and of certain of its Contents, passed daily by the same Young Dog as in Table III., under Mercury.*

Date.	Urine. Grammes.	Specific gravity.	Urea. Grammes.	Ash. Grammes.	Entire Phosphates. Grammes.
Nov. 16th to 17th	212·500	1040	11·650	1·392	0·363
„ 17th to 18th	212·500	1040	11·650	1·392	0·375
„ 18th to 19th	212·500	1040	11·650	1·392	0·350
„ 19th to 20th	311·000	1030	14·290	1·824	0·388
„ 20th to 21st	311·000	1030	14·290	1·824	0·388
„ 21st to 22nd	124·400	1050	9·300	0·932	0·057
„ 22nd to 23rd	155·500	1045	10·700	0·932	0·060
„ 23rd to 24th	183·970	1043	13·183	1·600	0·114
„ 24th to 25th	283·000	1040	17·560	1·936	0·218
„ 25th to 26th	283·000	1040	17·560	1·936	0·218
„ 26th to 27th	171·050	1056	12·970	1·563	0·039
„ 27th to 28th	171·050	1054	12·955	1·563	0·039
„ 28th to 29th	171·050	1048	11·463	1·374	0·178
„ 29th to 30th	171·050	1048	11·469	1·374	0·178
Average daily amount	212·390	1043	12·906	1·502	0·212

Hence it appears that while the dog was under mercury the amount of urine and urea was as nearly as possible the same as it was before the administration of that drug; the ash was diminished 40 per cent., and of the ash the phosphates showed a still greater decrease.

On December 7th, 1860, a puppy was placed in the cage, and fed daily for eleven days upon twelve ounces of paunch and half a pint of milk. On December 11th, in addition to his food, he took one drachm of the pharmacopœia solution of bichloride of mercury, which the next day was increased to one drachm and a half. On the three following days (20th, 21st, and 22nd) two drachms were taken daily; slight diarrhœa then occurred, and the dose was reduced to one drachm. With the exception of the diarrhœa, the animal was in good health. The urine, from December 7th to 24th, was examined each day, except on December 14th and 15th. The results are stated in Tables V. and VI.

TABLE V.—*The Amount of Urine, and of certain of its Contents, passed daily by a Puppy six months old.*

Date.	Urine. Grammes.	Specific gravity.	Urea. Grammes.	Ash. Grammes.	Entire Phosphates. Grammes.
Dec. 7th to 8th	531·900	1032	9·460	4·305	0·254
„ 8th to 9th	531·900	1032	9·460	4·305	0·252
„ 9th to 10th	322·800	1024	5·750	2·591	0·343
„ 10th to 11th	322·800	1024	5·750	2·590	0·295
„ 11th to 12th	155·009	1027	5·460	2·419	0·508
„ 12th to 13th	435·410	1020	7·100	1·509	0·238
„ 15th to 16th	394·330	1028	13·470	1·370	0·212
„ 16th to 17th	410·520	1029	13·680	1·340	0·215
„ 17th to 18th	438·511	1022	6·500	1·350	0·343
Average daily amount	393·687	1026	8·515	2·420	0·296

TABLE VI.—*The Amount of Urine, and of certain of its Contents, passed daily by the same Puppy, under Mercury.*

Date.	Urine. Grammes.	Specific gravity.	Urea. Grammes.	Ash. Grammes.	Entire Phosphates. Grammes.
Dec. 18th to 19th	466·500	1032	6·920	1·312	0·274
„ 19th to 20th	466·500	1036	9·680	1·313	0·275
„ 20th to 21st	356·100	1022	5·410	1·719	0·193
„ 21st to 22nd	232·250	1022	7·950	1·718	0·191
„ 22nd to 23rd	186·600	1038	11·060	1·111	0·088
„ 23rd to 24th	186·600	1038	11·060	1·112	0·088
Average daily amount	315·760	1031	8·580	1·381	0·185

Here, as in the former case, the most marked alteration in the character of the urine excreted by the dog while taking mercury was the diminution of the salts. The entire ash was diminished forty-three per cent., and the phosphates thirty-eight per cent., a decrease out of all proportion to the lessened quantity of urine voided. This is the only instance in the series of experiments of a dog in health passing less urine when under mercury than before the administration of the drug, and may not improbably have been the consequence of change in the temperature.

Further experiments were delayed until the weather was warmer, when on May 21st, 1861, a young dog was placed in the cage, and fed daily on twelve ounces of paunch and half a pint of milk. On June 10th he began to take the pharmacopœia bichloride solution, the dose being gradually increased from one drachm to two drachms. The dog's health remained perfectly sound.

TABLE VII.—*The Amount of Urine, and of certain of its Contents, passed daily by a Young Dog.*

Date.	Urine. Grammes.	Specific gravity.	Urea. Grammes.	Entire. Phosphates. Grammes.
May 21st to 22nd	124·400	1044	9·770	0·069
„ 22nd to 23rd	460·000	1028	18·400	0·184
„ 23rd to 24th	400·000	1030	18·000	0·157
„ 24th to 25th	376·000	1026	12·150	0·107
„ 25th to 26th	313·000	1030	14·370	0·126
„ 26th to 27th	313·000	1030	14·370	0·127
„ 27th to 28th	350·300	1022	15·500	0·157
„ 28th to 29th	316·000	1032	18·740	0·214
„ 29th to 30th	90·000	1034	5·300	0·087
June 7th to 8th	410·000	1024	18·200	0·212
„ 8th to 9th	458·000	1030	23·000	0·276
„ 9th to 10th	458·000	1030	23·000	0·276
Average daily amount	339·06	1030	15·900	1·166

TABLE VIII.—*The Amount of Urine, and of certain of its Contents, passed daily by the same Young Dog as in Table VII., under Mercury.*

Date.	Urine. Grammes.	Specific gravity.	Urea. Grammes.	Entire Phosphates. Grammes.
June 10th to 11th	545·000	1023	16·9	0·261
„ 11th to 12th	185·000	1026	8·2	0·095
„ 12th to 13th	305·000	1021	11·3	0·053
„ 13th to 14th	305·000	1021	11·3	0·124
„ 14th to 15th	365·000	1027	14·7	0·053
„ 15th to 16th	372·000	1024	20·3	0·116
„ 16th to 17th	373·000	1024	19·8	0·115
„ 17th to 18th	252·000	1018	7·1	0·087
„ 18th to 19th	407·000	1016	15·7	0·149
„ 19th to 20th	408·000	1016	15·3	0·148
„ 20th to 21st	210·000	1021	9·5	0·047
„ 21st to 22nd	211·000	1021	9·8	0·046
„ 22nd to 23rd	427·000	1020	19·0	0·121
„ 23rd to 24th	427·000	1020	19·0	0·121
„ 24th to 25th	427·000	1020	19·0	0·122
„ 25th to 26th	348·000	1012	8·8	0·129
„ 26th to 27th	272·000	1030	10·9	0·114
„ 27th to 28th	272·000	1030	10·9	0·114
„ 28th to 29th	272·000	1030	10·9	0·115
„ 29th to 30th	272·000	1030	10·9	0·113
„ 30th to July 1st	443·000	1032	22·9	0·206
July 1st to 2nd	443·000	1032	22·9	0·206
Average daily amount	342·77	1024	14·323	0·130

On comparing these two tables (VII. and VIII.), it will be seen that the phosphates again were very considerably diminished in the urine when the dog was under mercury. Circumstances prevented the estimation of the entire ash. The urea was slightly diminished, and the amount of urine was as nearly as possible the same during the exhibition of mercury as it was before.

Hence it appears that as long as the animal retains its health, mercury exercises no influence upon the amount of urine or urea, the quantity being sometimes a little more, sometimes a little less than before the mercury; but that the phosphates and entire ash, as far as these experiments go, are always in a very remarkable degree diminished.

The effect of mercury upon the urine is therefore to diminish the secretion of the salts. Upon what this diminution depends it would be useless to enter, until a careful series of blood-analyses before and after mercury has been taken, has been made. Such a series of analyses I contemplate shortly undertaking.

PART FOURTH.

Chronicle of Medical Science

(CHIEFLY FOREIGN AND CONTEMPORARY).

HALF-YEARLY REPORT ON MICROLOGY.

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PART I.—PHYSIOLOGICAL MICROLOGY.

NERVOUS SYSTEM.

Olfactory Bulb and Nerves.—Lockhart Clarke has investigated the minute structure of the olfactory bulb and olfactory mucous membrane in the sheep, cat, rabbit, and frog.* The bulb is seated on the anterior extremity of its peduncle, which it partially envelopes, as a slipper invests a foot. Its under surface, or part corresponding to the sole, is convex, and received into the concavity of the cribriform plate through which its nerves are transmitted. Within, near its centre, is a large and more or less oval ventricle, lined by a layer of beautifully-ciliated epithelial cells, both round and pyriform. Its peduncle as it passes backward becomes wider, and terminates gradually by expanding to form the broad and prominent convolution which covers in the cornu ammonis. At its anterior extremity its free or under surface is crossed obliquely by a set of fibres which connect some portions of the bulb with the *anterior perforated space* and *corpus striatum*. For about a quarter of an inch from the bulb it is entirely free or unattached; but behind this point, its substance is directly continuous with that of the cerebral convolution by which it is overlaid. By sections made in proper directions, it was also found that the grey substance of the cerebral convolution is reflected forward on to the peduncle, and passes with it into the bulb; and that the white substance of the peduncle itself is an offset or process of the white substance of the anterior cerebral lobe. Continued into the bulb, this white substance, composed entirely of fibres, forms a second layer round the whole of the ventricle, external to the first or epithelial layer which lines that cavity. This, in its turn, is surrounded by a third of nearly the same depth and shape, but consisting of nuclei and nerve fibres. The nuclei are disposed round the lamina in more or less oval or fusiform groups; and between these, a multitude of fibres, derived from the next inner or second layer, form a continuous plexus of bundles, in which the fibres become much finer, and appear to be connected with the nuclei, which are also connected together by a common network of fibres. All these are crossed by another set of fibres derived from the same source, and which radiate through the layer, just as the fibres of the white substance of the cerebral convolutions radiate through the cortical or grey portion. This third layer is called by Clarke the *plexiform nucleated lamina.* Next in order comes a thicker, but very soft layer of grey substance, which entirely surrounds the one just described, although very lightly attached to it. This is called by the author, the *substantia gelatinosa* of the bulb, and may be divided into two distinct laminæ or parts. The most internal of these is composed of nuclei connected together at various intervals by a network of fibres, in which large nerve-cells with branching processes are embedded. The nucleated network is somewhat coarser than that of the preceding layer, but is directly continuous with it. The large nerve-cells are disposed in an irregular row concentric with the lamina, and differ from each other in shape and size. They are round, triangular, stellate, oval, and fusiform, with their longer axes at right angles to the plane of the layer—that is, in a direction inward and outward. Inward, their processes extend into the next nucleated plexiform layer, with the network of which their branches are continuous; and outward, they become continuous in the same way with the nucleated network of the lamina in which their cells are embedded. Laterally, they connect these cells with one another. The nuclei in many parts of this network resemble those of the one preceding; but some are more distinctly granular, while others are twice or three times as large, and have clear, round nuclei resembling those in the larger nerve-cells of the cerebral convolutions, in the peduncle of the bulb, and in the gelatinous substance of the spinal cord. The *outer* lamina of the *substantia gelatinosa* of the bulb consists of numerous dark and roundish masses, embedded at short but irregular intervals, and without any great regularity, in the nucleated network common to both the laminæ, but which in this one is rather looser, and contains a greater number of nuclei, especially of a larger kind. The dark, roundish masses are seen under a high magnifying power to be themselves composed of a nucleated network interspersed with granules, and apparently of the same kind as that by which it is surrounded, and with which it is directly continuous. They are richly supplied with bloodvessels. The fibres already described as radiating outward across the third, or nucleated plexiform layer, from the second or next internal, and being derived through the peduncle from the fibrous or white substance of the cerebral convolutions, continue their course outward into the *gelatinous substance* of the bulb, where they become exceedingly fine, and cross each other in all directions, to be continuous, apparently, with the nuclei of the network.

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* **Ueber den Bau des Bulbus olfactorius und der Geruchsschleimhaut: in Kölliker's Zeitschrift für Wissen. Zoologie, Band xl. Heft 1.**

The last or superficial layer of the bulb consists of the proper fibres of the olfactory nerves, with pia mater and bloodvessels. The nerve-fibres are collected into bundles of different thickness, which cross or join each other obliquely to form a close plexus. At the inner border of this plexiform nerve-layer, the fibres appear to originate as a continuation of the granular nucleated network of the dark roundish masses in the external lamina of the *substantia gelatinosa.* From the outer border of the plexus separate bundles of nerve-fibres make their way to the foramina of the cribriform plate, to be distributed to the mucous membrane of the olfactory region.

From the above description, then, it appears that the olfactory bulb is really a cerebral convolution modified in its structure, and giving origin to nerves at its free surface.

With regard to the structure of the olfactory mucous membrane, Clarke differs to some extent from other recent authors, or rather he has supplied some additional facts, which are chiefly as follows: At right angles to the plane of the mucous membrane, and reaching quite to the surface through the epithelium, there are numerous cylindrical or slightly conical columns beset with granular nuclei, and having some resemblance to branches of the peptic glands of the stomach. These vertical columns are made up of smaller nucleated masses, and are offsets from the *sub*-epithelial glandular layer of nucleated cells. They are very closely surrounded and separated from each other by the cylinders of epithelium, which are applied against their sides, and appear to be connected with them. These epithelial cylinders, below their nucleus, are more or less tortuous or angular in their course, and have at intervals triangular enlargements that give off processes which are connected with numerous nuclei, and through these, with each other, and *apparently* with the glandular columns, in one continuous network. At their lower ends—that is, at the under surface of the epithelial layer—they terminate in several branches, which become continuous with the septal fibres or walls of the nucleated cells in the *sub*-epithelial glandular layer, and which fill up the interspaces between those cells with angular dilatations precisely similar to those which are found on the processes of the epithelial cylinders themselves. Amongst these epithelial cylinders, and the glandular columns which they surround, in the sheep, cat, rabbit, and probably in all mammalia, there is no appearance of those peculiar cells which are so numerous in the frog, and are called by Schultze the *olfactory cells.* In some fishes, particularly in the pike, the peculiar varicosities on the processes of these cells were found by Clarke to be much more conspicuous than in the frog.

The way in which the olfactory nerves terminate in the mucous membrane is a question of peculiar difficulty. In the *sub*-epithelial layer some of the branches break up into a beautiful network or plexus of fibres between the round glandular masses, while others ascend obliquely to the under surface of the epithelium, in which they disperse, and lose themselves by becoming *apparently* continuous with the nucleated network in connexion with the processes of the epithelial cylinders. If such a continuity really exist, and the nucleated network be common to both the glandular and epithelial elements of this layer, it would then follow that the nerve-fibres are as closely connected with the one element as with the other; and in the *sub*-epithelial layer they would terminate in the glandular nucleated cells, or at least be connected with their septa or walls, which are only extensions from the processes of the epithelial cylinders. This question, however, needs further investigation.

On the Minute Structure of the Olfactory Bulb. By Dr. G. Walter, of Euskirchen.[*]—It seems that the above-mentioned communication was written before its author had had the advantage of reading either the observations of Owsjannikow,[†] or those of Lockhart Clarke, which we have condensed above. The paper is accompanied by two excellent plates containing nineteen illustrations, resulting from the examination of the olfactory bulb of the calf, rabbit, carp, &c.; and has a rather lengthy postscript, which was written after the reading and consideration of the observations of Owsjannikow and L. Clarke, which led him to submit his own to a renewed testing. We will not here give the results detailed by Walter in his original paper; but will epitomize the postscript, wherein he compares his own inferences with those of the two observers mentioned, confining ourselves to such passages as indicate the agreement or difference between their researches. On the whole, Walter agrees conspicuously with Clarke in his description of the organ in question. We find Walter agreeing with Owsjannikow as regards the ependyma of the cavity of the olfactory bulb, but objecting that Clarke has failed to notice the connexion of the processes of the epithelial cells with the more deeply placed connective-tissue elements. He acknowledges the ciliary motion of the cylindrical cells at this place. He agrees with Owsjannikow in having met with a thin connective-tissue layer containing capillary vessels placed between the epithelial layer and the nerve elements (which Clarke does not notice);

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[*] Virchow's Archiv, 1861, Band xxii. Hefte 3 und 4, p. 241.

[†] Reichert and Dubois-Reymond's Archiv, 1860; see also our Micrological Report for April, 1861, p. 516.

and speaks of the stellate connective-tissue cells with their processes far outwards in the white substance, which Clarke, "who appears to have overlooked their processes," took for free nuclei of epithelial cells; whilst Owsjannikow mistakenly connects them with nerve-fibres as small multipolar nerve-cells. Walter agrees with Clarke as to the structure of the white substance. The nuclei between the meshes of the nerve-texture (which Walter calls bipolar nerve-cells), and which appear to be connected with nerve-fibres by their very fine processes, are recognised by Clarke and Owsjannikow; but the latter speaks of them as being found first in the grey substance, whilst Walter describes them as existing already in the outermost layers of the white substance. Walter agrees with Clarke in not finding nerve-fibres in connexion with multipolar cells in the outermost layer of the white substance, and thinks that Owsjannikow, who describes such, has mistaken connective-tissue cells for nerve cells. Walter agrees with Clarke as regards the connexion of the large nerve-cells of the grey substance with the very fine fibres proceeding from the ganglionic polar nerve-cells. He refers to Clarke's description of the union between the peripheric projection of the large nerve-cells and the network of the outermost granular layer of the grey substance, and speaks of the method of this connexion and the transition into the finest olfactory fibres as undoubtedly the most difficult in the whole investigation of the structure of the olfactory bulb, and a point upon which his investigations have as yet thrown no very clear light. He then alludes to the existence of certain globular bodies (alluded to in Lockhart Clarke's paper, see above), which M. Schultze of Bonn had delineated as having been met with by him on the surface of the bulb, and from which he had seen the olfactory nerve-bundles proceed. Walter had observed such structures in rabbits and in the calf, in the outermost layer of the grey substance, which consists of a finely granular matrix containing free nuclei; and lately has met with these globular masses of matrix, surrounded by nucleus-holding investment of pia mater (the "olfactory cells" of Schultze), in the rabbit, fox, marmot, calf, sparrow, &c. This granular matrix of the base of the bulb is traversed by fine fibre-processes from the large nerve-cells lying in bundles, and passing to the globular masses without previously uniting with the small free nuclei. These primary fibres appear to become enveloped by a portion of the contained granular substance along with free nuclei, with which they are covered in their passage through the cribriform bone as far as their periphery, where they become divested (as is well known) of granular substance and nuclei.

Walter describes round cells, of the size of large nerve-cells, with one or more nuclei, very like the surrounding free nuclei, as existing in the globular masses or olfactory cells, as well as in the grey substance outside them. These cells possess a very fine lacerable membrane and granular contents. Walter concludes by describing the appearance of the bloodvessels of the olfactory bulb.

Notes of Researches on the Intimate Structure of the Brain. (Second Series.) By J. Lockhart Clarke.*—For a minute account of these observations we regret that we have not space. They concern mainly the minute structure of the pons Varolii, corpus striatum, and pineal gland.

On Nerve-Ganglia in the Excretory Ducts of Glands. By Professor Manz.†—This observer's attention was drawn to these minute structures by reason of the rhythmical movements which had been observed in the ducts. He had not succeeded in establishing their existence in mammals, but in birds, as in the fowl and pigeon, he found in the ureter two nerve-branches united by a tolerably fine transverse anastomosis, and forming a wide-meshed network; and at the points of departure of the transverse branches the ganglia were found in greatest abundance. Most of the ganglion cells show traces of a single process, but bi-polar cells also exist. The ganglia are situated external to the muscular tissue. In the course of the nerve-branchlets, also, small groups of ganglion cells were met with. Similar ganglia were seen in the vas deferens, in the ductus choledochus and cysticus, and in the pancreatic ducts throughout their entire length.

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* From the Proceedings of the Royal Society for June 20th, 1861.

† Medizinische Jahrbücher, 1861, Heft 6, p. 221: quoted from the Verhandlungen der natur. Ges. zu Freiburg, Band ii. p. 163.

Ganglion-Cells in the Ciliary Muscle and in the Nerves of the Choroïd of Man. By H. Müller.[*]—In this structure Müller found two different kinds of formation—distinct cells, situated here and there in the twigs of the first and second order, which the ciliary nerves divide into on their entrance into the ciliary muscle; and knotty swellings of the fibres, consisting of oval corpuscles, contained within the swollen darkly-contoured nerve-fibres. Both formations have also been recognised by Krause.[†] The latter he found in both eyes in twelve bodies which he examined, both in the interior of the ciliary muscle and in the branches of the first order of the ciliary nerve.

H. Müller also found that the nerves of the choroïd, which for the most part are derived from the ciliary nerves, possessed large and small groups of ganglion-cells, with distinct processes; the number of cells being in distinct relation with the development of the muscle—viz., the circular fibres of the arteries of the choroïdeal muscle. The nervous network possesses probably an important influence in the regulation of the circulation of this part, &c.

On the Mode of Termination of Nerves in Muscle.—Kühne[‡] describes granular corpuscles situated on the intra-muscular extension of the axis-cylinder of nerves in the case of batrachiæ, fishes, birds, mammalia, and man, which he designates peripheric nerve-knots (*nerven-knospen*). These are from 0,005 to 0,01 mm. in size, and pointed at one end. By means of this apparatus, the motor nerves are placed in immediate contact with the contractile substance.

GLANDULAR SYSTEM.

Hypertrophy of the Coccygean Gland. Heschl.[§]—Luschka, who discovered this gland, suggested that probably certain congenital cysts arose in its structure. The observation of Heschl confirms the supposition. In a fœtus of full growth he found, besides imperforation of the rectum and double vagina and uterus, a tumour of the size of a nut situated on a level with the summit of the coccyx, to which it was attached by filaments, thus corresponding exactly to the situation of the coccygean gland. Externally, this tumour was very analogous to a salivary gland, the surface presenting circumscribed lobules; it was of a greyish colour and tolerable consistence, somewhat rounded, and slightly flattened in an antero-posterior direction. On section it was seen to enclose a number of small cavities, varying in size from that of a hemp-seed to that of a bean, and filled with a mass of cholesteatomatous material, found by the microscope to consist of pavement epithelium. There seemed no doubt that the tumour arose from hypertrophy and degeneration of the gland in question. Heschl supplements the above description by that of a case in which, in an embryo three and a half months old, this gland was represented by an alveolar mass, with walls formed of embryonic tissue, and enclosing a number of nucleated cellules. From this it would appear that at this period of embryonic development the coccygean gland is still simple and not converted into alveoli.

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[*] Medizinische Jahrbücher, 1861, Heft vi. p. 220.

[†] Anatomische Untersuchungen, 1861, p. 91.

[‡] Comptes rendus, 1861, Band lii. p. 316.

[§] Quoted in the Gazette Hebdom., tome vii. No. 38.

BONE, TENDON, CONNECTIVE-TISSUE.

On the Ossification of Tendinous Tissue. By N. Lieberkühn.[*]—The author reviews the contending opinions of Virchow,[†] Förster,[‡] and Henle,[§] and Baur,[‖] on the ossification of connective-tissue, &c., and then proceeds to give in detail the results of his examination of ossified tendons of various kinds of birds. These structures present nothing remarkable until the commencement of the ossifying process, which occurs when the bird is almost fully grown, and is initiated by a plentiful cell-production. On making a *transverse* section of a piece of tendon near a decided point of ossification, after being dried and then moistened with water, primary septa or partitions, with largish vessels and secondary ones proceeding therefrom, separating more and more the connective-tissue bands or bundles, are to be seen, in some cases the alternate septa being as thick as the intervening bands. No connective-tissue cells are visible on this section. Longitudinal section, however, of the tendons of small birds shows rows of cells with homogeneous transparent intercellular substance between all distinguishable bands, and especially where three or four bands meet. The cells are either globular or oval, or almost cubical or elongated, occasionally possessing a nucleus or finely granular fatty matter. These cells cannot be looked upon as modified connective-tissue corpuscles, as at no time do the latter observe the requisite disposition in tendon: their origin is most uncertain.

On transverse section, and after the addition of acetic acid, the broad and elongated bands of Donders—i.e., the undulating edges of the larger divisions of the tendon—are no longer apparent, but each of the slighter secondary bundles projects out of its position in the form of a hemisphere attached to the partition wall. On section made exactly at right angles with the longitudinal axis of the tendon, cells are seen at the projected part in such numbers that they entirely correspond to the thickness of the section. When, however, ossification commences in the tendon, changes occur in the tissue, the deposition of calcareous salts being indicated by fine light refracting puncta throughout the entire tendinous tissue, and gradually so approximating to each other that no interval remains, the entire structure becoming opaque; the deficiencies which occur, owing to the partitions being not yet ossified, are however, easily recognisable. At this period, in a longitudinal slice, bone-corpuscles of various forms are visible in the situation of the rows of cells, with numerous processes running in all directions; and apparently the processes of one bone-corpuscle communicate with those of a neighbouring one. Smaller structures like bone-corpuscles are also seen (sometimes so diminutive as to be scarcely visible) under the strongest powers, and others are either disposed longitudinally or are placed between the larger true bone-corpuscles, being, in fact, parts of larger bone-corpuscles and sections of their processes. Finally, certain elongated configurations are observable, very like long bone-corpuscles, but departing as ossification advances; and these are owing to the imperfect ossification of many of the spaces between the bands or partitions.

As regards the changes undergone by tendinous substance during the deposition of calcareous matter, they may be judged of as follows: After taking an entire tendon and removing this calcareous matter by prolonged maceration in acetic acid, the tendon is seen to be transparent and much swelled as far as the edge of the ossified part, where it is less swelled and more opaque, both the transparent and non-transparent parts containing cells. In many places the enveloping and unresisting partitions become burst, the cells remaining arranged longitudinally in their interstices. On watching the action of acetic acid upon a longitudinal fine section of dried tendon at the part deprived of calcareous matter, the bands are seen slowly to swell and the rows of cells to approximate, the cells gradually becoming visible. The refractive power of the partitions and intervening columns has become much altered, but not so that of the striped substance. Moreover, on both sides of the limit dividing the ossifying from the non-ossifying parts, numerous transverse stripes are visible coursing over the bundles, corresponding to the loops which pass from one side of a bundle to the other, according to Henle's supposition respecting normal tendinous tissue.

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[*] Reichert and Du Bois-Reymond's Archiv, 1860, p. 824.

[†] Archiv, 1847, s. 136.

[‡] Schluss, Supplement z. Atlas d. Mik. Path. Anat., Taf. xxxiv. Fig. 5.

[§] Jahresbericht, 1859, s. 95.

[‖] Entwicklung der Bindesubstanz. Tübingen, 1858.

On examination of transverse sections of tendon prepared as the above, it is easily seen that the stripes or markings visible in longitudinal sections represent partitions. The secondary bundles are clearly distinguishable, and are surrounded by a fine layer of a more refracting substance; and in some places, instead of the secondary ones, slighter bundles exist, which may be termed tertiary, surrounded in like manner. Where this formation is most abundant, bone-corpuscles, with processes passing into the interspaces of the bundles, are to be seen; and inside both the latter and the former formation circular or elliptic foramina of divided vessels are visible.

By acting on the preparation with strong sulphuric acid the tendinous bundles no longer swell up in hemispherical bodies. It soon becomes less transparent, and one part refracts the light more than the remainder. After a time the contents of the secondary partitions become gradually more transparent, and we have the appearance as of a network of elastic fibres; but on tearing up the preparation and getting a longitudinal view, it is manifest that the apparent fibres are only transverse sections of the partitions. The bone-corpuscles of a longitudinal section make their appearance, and also the so-called elastic fibres. Presently some of the bundles become less transparent; quite dark by transmitted, white by reflected light. The process affects the other parts, and the bone-corpuscles become invisible; and neighbouring longitudinal bundles are seen as if permeated by fine dark stripes, and as if split into fine fibres.

Hitherto the ossified tendon has possessed the tendinous structure. Further changes consist in the tendon assuming the structure of bone, but then only when its whole length is ossified. On making transverse section of a completely ossified tendon, a large number of circular and elliptical cavities, of variable sizes and corresponding to the vascular canals of bone, are visible. Round the vascular canals are concentrically arranged the bone lamellæ, in which are the bone-corpuscles, and the lamellar systems and bone-corpuscles exactly resemble those of true bone; the processes of the corpuscles arising with more or less breadth, and branching out during their course in many places, those of neighbouring corpuscles uniting. The bundles and partitions of tendon are no longer visible.

On longitudinal section, also, we have in all respects the appearances of true bone, the bone-corpuscles being somewhat long, and arranged in rows like the original cells at variable distances; but the apparent anastomoses are more numerous, and the processes shorter. Sometimes the corpuscles are so near as to touch. The secondary bundles, impregnated with calcareous salts, are very clearly distinguishable as separated from each other; and where three meet we have generally a dark spot resembling a cavity, having dark contoured streaks projecting, which in some cases penetrate between and isolate the whole number of bundles. In other cases much finer processes from those just described pass through the secondary bundles, dividing them into two or four or more subdivisions. In places where the substance consisting of tertiary bundles adjoins pure bone-tissue, the processes and the bone-corpuscles are lost in the markings encircling the tendinous bundles, which may be thicker or finer than the processes of the bone-corpuscles. On adding strong hydrochloric or nitric acid to a transverse section, the fine bundles become well-marked and the partitions very prominent, their contents becoming invisible, and a network of exceedingly fine and equal-sized meshes, which, disposed in the form of lamellæ, surrounds the Haversian canals, is seen in transverse sections. On tearing this up with fine needles, some portions of the preparation are seen showing a longitudinal aspect, which may thus be compared with the transverse one. In the longitudinal aspect a substance may be seen with long stripes, evidently the boundaries of tertiary bundles near to each other, some of which have the elongated bone-corpuscles attached, but without any projections. After more prolonged action of the concentrated acid, the network of the transverse section disappears, the vascular portions in the Haversian spaces and the remains of the bone-corpuscles, along with portions of the partitions, remain: in a longitudinal section, irregular, often spirally-bent threads, very like those structures described by some as elastic fibres, of tendinous tissue, are to be seen. These threads lie in spaces between the partitions, becoming free by the action of the acid.

The author, towards the close of his communication, demonstrates that the observations therein contained go to show how groundless is the supposition held by some, that in ossification of cartilage the foundation (*Grundlage*) of the bone is not the cartilage itself, but a new blastema formed by the medullary spaces subsequent to solution of the so-called calcified cartilage.

He concludes by making certain inferences antagonistic to some of the positions advanced by Virchow, and lately brought prominently before the profession in England. He points out that "the stellate connective-tissue corpuscles as described by that author *do not exist* in tendon, the formation of connective-tissue in bundles having some other origin; and that the existence of partitions is a necessary attendant upon the composition of whole tendons out of individual bundles. The so-called perfect connective-tissue with the completely formed bundles ossifies without the existence of bone-corpuscles; but where, indeed, bone-corpuscles arise during ossification in fully formed connective-tissue, a cartilaginous substance with cells first appears by a special process, as in tendinous tissue." The statement put forth by Virchow in his treatise upon the identity of bone, cartilage, and connective-tissue corpuscles, that, generally speaking, the fully formed connective-tissue may ossify, and its supposed corpuscles become bone-corpuscles, is to be considered, according to our author, as entirely a mistake.

Of course, also, if the anastomosing stellate cells above alluded to do not in fact exist in connective-tissue, the doctrine of the so-called juice-channel system, as promulgated by Virchow and accepted by Kölliker and others, and also that of the supposed transition from the connective-tissue cells of Förster to bone-corpuscles, are perfectly untenable. The author goes on to urge several points as proving the unreasonableness of such a view.

In a postscript he notices the paper by Dr. Martyn on connective-tissue in Beale's 'Archives of Medicine,' No. 6, p. 99, alluded to in our Micrological Report for Oct. 1861, p. 522.

MUSCULAR SYSTEM.

On the Histology of Muscular Fibre. By Dr. Otto Deiters, of Bonn.*—The researches of this observer appear to prove that certain tissues belonging to the group of connective tissues may be transformed into muscular elements of animal life; and that in this transformation, the so-called plasmatic or connective-tissue cells which retain their fusiform or stellate form are concerned. The striated muscular substance proceeds from the cellules, and is deposited upon thin envelopes. It is therefore an intercellular substance, and appears under the form of a fringe corresponding to a fibril, which afterwards increases in thickness by the deposition of successive layers. This fringe exists most frequently on one side only of the cellule, but it may equally be formed on both sides at once. Contemporaneously the cellule increases in size, and its nucleus multiplies; and the fringe, which is not slow to present transverse striæ, elongates sometimes so much as to surpass the extremities of the cellule. One cellule divided may of itself give origin to a primitive fibre; but more frequently many cellules contribute to the formation of a fibril. These cellules are not generally arranged in a straight line, but placed obliquely one after the other, or one at the side of the other, transversely. The cellules connected with this development unite with the plasmatic cells of the tendons. The sarcolemma is the part of the primitive bundle which is formed last, and is not derived from the enveloping membranes of the cellules, but appears rather to belong to the formations termed cuticular.

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* Archiv f. Anat. Phys. und Wissenschaft. Medecin, 1861.

PART II.—PATHOLOGICAL MICROLOGY.

PUS, MUCUS, ETC.

On the Origin of Pus on Mucous Membranes. By Dr. E. Rindfleisch.[*]—The author refers to former observations made by himself on the origin of pus in parts containing connective-tissue,[†] and to the views put forth by Henle,[‡] Guterbock,[§] Vogel,[||] Förster,[¶] Buhl,[**] Remata,[††] Eberth,[‡‡] as well as the later one by Virchow, which we need not particularize; and then passes on to give in detail the results of certain experimental researches which he had instituted with special reference to the mode of formation and the method of separation of the newly-formed pus corpuscles. For this purpose he not only examined bodies after death, but resorted to the artificial production of catarrh in lower animals. He describes the results obtained by irritating the conjunctival sac (mucous membrane) of the frog with acetic acid. The surface becomes gradually opaque, and a small quantity of sticky whitish mucus is shed, containing, along with a large number of epithelial cells, a considerable number of pus corpuscles. When this has been effected, which is about the third day after the application, the frog is decapitated, and the bloodvessels of the head thus emptied, by which means the moistening with blood of the surface of the mucous membrane is prevented; very fine perpendicular sections are then made, the mucous membrane being dried upon cork, and re-moistened with acidulated water.

We have not space to give the minutiæ of his observations, but the general conclusions deducible are as follows—viz.: That the origin of pus on the surface of mucous membranes occurs in various ways, according as we are concerned with an epithelial or a purulent catarrh. In the first case, the pus corpuscles are direct descendants of the sub-epithelial areolar-tissue cells, which become free by an upraising and rupture (penetration) of the epithelial layer; whilst in the second, the pus corpuscles arise by an endogenous process within the epithelial cells themselves. This endogenous development probably begins with a multiplying of the nuclei; a supposition for which, in addition to a small number of positive observations upon the division of nuclei inside the mother cells of cylindrical epithelium, the author adduces the negative one, that in the mother cells during and subsequent to the completion of the endogenous brood, certainly only in occasional cases can a normal nucleus be observed. Then follows a stage in which the cells assume a homogeneous, slightly glistening appearance, unchangeable on the addition of acetic acid. The cell contents arrange themselves (perhaps around the contained nucleus) in large roundish balls, varying in number in different kinds of cells, being in the case of the pavement form from three to twelve in number, and in that of the cylindrical form mostly two. In place of these roundish balls, or within them, there appear later on, after the addition of acetic acid, round cells exhibiting all transitions, from small elements containing a simple, round, glittering nucleus, to complete pus corpuscles, which become free, escaping from the mother cell and leaving a hollow space corresponding to them in size and afterwards enlarging. The loss of the cells engaged in the formation of the pus is supplied by fissiparous cell-growth in the deeper epithelial layers.

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[*] Virchow's Archiv, Band xxi. Hefte 5 und 6, p. 486

[†] Ibid., Band xvii. p. 239.

[‡] Hufeland's Journal f. prakt. Heilk., Band lxxxvi. 1838.

[§] De pure et granulatione. Berolini, 1837.

[||] Ueber Eiterung, &c. Erlangen, 1838.

[¶] Würzburger Medicin. Zeitschr., Band i. Heft 2.

[**] Virchow's Archiv, Band xvi. s. 168.

[††] Ibid., Band xx. s. 198.

[‡‡] Ibid., Band xxi. s. 106 ss.

In the purulent catarrhal condition of the respiratory mucous membrane of phthisical patients the same conditions are observable, the tracheal and bronchial ulcers proceeding originally from an accumulation and deposition of cells arising from the connective tissue of the membrane.

TUMOURS.

Fibro-Plastic Tumour of the Latissimus Dorsi Muscle, having Osseous Parietes.—This tumour was removed from a patient of Dr. Azam of Bordeaux,* and was of the size of a large walnut, being situated in the axilla. It was found to exist within the substance of the muscle at its external border, being partially covered by muscular fibre, and partially by the fibrous envelope of the muscle, and partially by a thicker and more resistant membrane resembling a periosteum, on which were inserted muscular and tendinous fibres. It was completely covered by a shell of bone varying in thickness from two to four millimètres, of a remarkable spongy texture. This bony envelope was smooth externally, but gave origin on its inner surface to a large number of trabeculæ or very delicate osseous projections crossing each other in all directions in passing towards the centre of the tumour, between which was situated a quantity of homogeneous material of a pale rose-colour, very lacerable, but having a certain density. At one point a small cavity existed, filled with a viscid liquid.

On microscopical examination this central pulp was found to be constituted of nuclear and fusiform fibro-plastic elements, and of a small quantity of fibrillated tissue.

The patient did well.

This tumour has much analogy with one described by M. Richard to the Société de Chirurgie, December 7th, 1859, which was ossified and situated at the external edge of the latissimus dorsi muscle, and was thought to be probably an ossified ganglion.

MISCELLANEOUS.

On the Trichina Spiralis. By W. Turner, M.B.†—We have already in previous Micrological Reports drawn attention to various experiments performed with the view of elucidating the origin and development of the entozoa. In the communication alluded to above, the author, after giving a short historical *résumé* of what has hitherto been written on the subject quoting the numerous inquiries instituted both abroad and in England, proceeds to give the details of two experiments in which he sought to verify the various observations of others, by feeding cats with the flesh of a subject containing the trichina spiralis. "*The worms were observed to move, though somewhat languidly, on rupturing the cysts.*" In the first experiment a kitten was fed with the flesh and died about thirty-six hours after. The intestine was found at its upper part to contain specimens of the "ascaris mystax," but the remains of the trichina were not seen. Several ova containing a distinct nucleus of granular matter were seen in the mucus as well in the large as the small intestine. These ova differed much from the ova within the generative tube of the ascaris; the latter, in addition to having the contained granules diffused more uniformly, being much larger. Turner cannot decide whether these ova are derived from the trichinæ or not.

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* See Proceedings of the Société de Chirurgie, Oct. 30th, 1861, in L'Union Médicale Dec. 10th, 1861.

† Edinburgh Medical Journal, No. lxiii. p. 209.

In the second experiment, a cat was fed with the trichina-flesh on the 7th, 13th, and 16th of July, being fed on fish, bread, and milk in the intervals. It was doubtful whether, on the last occasion, the trichina was alive. The animal was killed on the 24th. In the jejunum several specimens of the ascaris mystax were found, and in the ileum a small tænia, but no remains of trichina cysts. Three or four thread-like worms were seen by the microscope in the mucus of the large and small intestines, and of the stomach, actively moving about, sometimes elongated, at others coiled up; and free ova were seen, like those in the ovarian tube of the ascaris in all respects. Each of these transparent thread-like worms had a pointed and a rounded end, and were about two-thirds smaller than the trichinæ met with in the muscles of the cat. Through the worm a canal could be traced, but it was difficult to say whether it extended as far as the pointed extremity, or terminated a little on one side of it. In the middle thread, and partly in the narrower end of the worm a sacculated or cellular appearance was observed, apparently surrounding the alimentary canal, and on one side of this a collection of granular matter was seen. The muscles of this cat were found in many cases to contain trichinæ. When the cysts were isolated they were seen to differ in shape from those found in human muscle, being almost round. The wall was slightly granular and transparent, so that the arrangement and anatomy of the contained animal were easily seen. In no instance was more than one worm seen in each cyst; and along with the worm viscid matter, containing granules and nuclei, were contained. The muscular fibres surrounding the cysts were not in a fatty condition, nor did the collections of fat cells exist external to the cysts so often found in man. The author is *quite unable* to support Virchow in his view that the trichina lies within the muscular fibre, and not between adjacent ones, the cyst-wall being formed by thickened sarcolemma, and not by hypertrophy of the connective tissue. In this case, no doubt, the thread-like worms were not derived from the ascarides dwelling in the cat's intestines, as they are not found in the cat in whom the ascarides are found to exist, to which the trichina-flesh is not given; and, again, they were found both in the large and small intestines, whereas the ascaris mystax is only found in the duodenum and jejunum.

The author remarks that, like Owen, he has found the superficial muscles of the human body more abundantly affected with the trichina than the deep ones, and also that the superficial surface of the muscles is more affected than the deeper ones; showing, as he supposes, "the tendency which the worms possess to work their own way towards the exterior." He appends a list of 19 recorded cases, including his own, in which the trichina was found in man: in only one (related by Zenker) was death or remarkable symptoms of any kind produced thereby; all excepting two cases dying in middle or advanced life of debilitating disease. Virchow had found that atrophy of muscle is produced by the trichina. Turner states that between one and two per cent. of the dead bodies which have come under his observation in the last five years have been affected with the trichina.

Peculiar Crystals found after Death in the Blood, in Cases of Leukocythemia. By Dr. J. C. White.[*]—These crystals appear to have formed some time after death, and were met with in cases under the care of Dr. Ellis, with enlarged spleen and liver. Most of these were identical with crystals previously found by the same gentleman under similar circumstances, and described under the name of Leukosin in the 'Boston Journal' for February 2nd and 9th, 1860, but in addition to these, "other crystalline bodies were noticed in the blood, which was still comparatively fresh, in the form of rosettes of a reddish-brown colour." After a short time, when the blood had become putrid, these latter forms, and also the red and white corpuscles, had disappeared, but the leukosin was unchanged, and this was also the case in some blood which had been kept three years. This leukosin was described in 1860 as existing in leukhæmic blood by MM. Charcot and Vulpian.[†] Dr. White speaks of their occurrence in every case of leukhæmia in which it had been looked for, and suggests that this leukosin exists in the blood during life in a soluble form. In all these cases of leukhæmia, "the blood was of a uniform dark purple or moroon colour, of the consistence of thick paint, and, except in one case, showed no disposition to separate into yellow or purulent-looking and dark-red portions."

For the following papers bearing on the histology of healthy and diseased structures we have not space for more than reference.

On the Degeneration, with Atrophy, of the Posterior Column of the Spinal Cord, and its connexion with the disease termed "Ataxie Locomotrice Progressive." Par Dr. Dumenil. (L'Union Médicale, Février 11, 1862.)

On the Histology of Ossified Connective Tissue. Von S. Lessing. (Henle and Pfeuffer's Zeitschrift f. Rationelle Medicin: Dritte Reihe, Band xii. p. 314.)

Three Cases of Acephalocyst Formations. Von Dr. H. Wallmann. (Wochenblatt d. k. k. Gesellsch. d. Aerzte in Wien. Nos. 25 and 26.)

Experimental Researches on the Regeneration of Nerves separated from their Nervous Centres. By Phillipeaux and Vulpian. (Gaz. Méd., 1860. Nos. 27, 29, 32, 34, 35, 37, 39.)

On the Regeneration of Nerve-Fibres. Von Prof. Dr. Remak in Berlin. (Virchow's Archiv, 1862, Band xxiii. Hefte 3 und 4, p. 441.)

On Amyloïd Degeneration. Von E. Wagner. (Archiv d. Heilkunde, 1861, Heft 6.)

Echinococcus-Cysts passed by the Bladder. Von J. Quinquerez. (Oesterr. Zeitschr. f. Prakt. Heilkunde, July, 1861. Quoted in the Medizin Jährbuch, 1862, Heft 1, p. 70.)

On Sycosis, and its Relation to Mycosis Tonsurans. Von H. Köbner, in Breslau. (Virchow's Archiv, Band xxii. Hefte 3 und 4, p. 372.)

On Melanæmia, with Observations on the Natural Structure of the Spleen and Lymphatic Glands. Von Prof. Grohe, Greitsweld. (Do. Band xx. S. 306, und Band xxii. Hefte 5 and 6, S. 437.)

On Inflammation of Serous Membranes. Von Dr. J. Cohnheim, in Berlin. (Do., S. 516.)

On Urinary Sarcinæ. Von Dr. P. Munk, in Berlin. (Do., S. 570.)

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[*] Boston Medical and Surgical Journal, Nov. 28th, 1861.

[†] Gaz. Hebdom. de Médecine et de Chirurgie, tome vii. No. 47; see also our Micrological Report for April, 1861, p. 527.

HALF-YEARLY REPORT ON TOXICOLOGY, FORENSIC MEDICINE, AND PUBLIC HYGIENE.

By Benjamin W. Richardson, M.A., M.D.

Senior Physician to the Royal Infirmary for Diseases of the Chest, and Lecturer on Physiology at the Grosvenor-place Medical School.

I. Toxicology.

Poisoning by Corrosive Sublimate.—Dr. Cabot, of Boston, U.S., reports a case of poisoning by corrosive sublimate in a young woman, twenty-five years old. She was brought to the Massachussetts General Hospital, to which Dr. Cabot is attached, about three o'clock in the morning of August 26th, 1861, vomiting and retching violently. She had been perfectly well until six o'clock of the previous evening, when she suddenly became nauseated, and in about half an hour vomited. No medical man was called until eleven o'clock, when a physician of Jamaica Plain was summoned, who found the fauces inflamed and the uvula swollen. He cauterized the parts with nitrate of silver, but finding that the patient grew worse, he brought her to the hospital in a carriage. She was now conscious, and had strength enough to walk to the water-closet. The whole surface of the body and limbs, with the exception of the abdomen and inside of the thighs, was cold; but the patient complained of feeling hot, and of a burning pain in the lumbar region. Soon after her admission she vomited some pure blood; and as she was almost asphyxiated, Dr. Cabot opened the trachea, after which the breathing became free and the pulse fuller. She continued to vomit, and sank gradually until twenty-five minutes past ten A.M., when she died. She was conscious to the last. At the autopsy the epiglottis was found swollen and stiff, of a brownish colour, with a small part of the mucous membrane eroded. The lining membrane of the trachea and bronchial tubes presented a reddish-brown colour, covered with a thin, slate-coloured substance. The mucous membrane of the œsophagus was in the same condition, but more friable. The stomach, somewhat œdematous externally, showed no perforation; the œsophageal portion of its mucous coat, for a portion of four inches, was in the same condition as the œsophagus. The pyloric portion was thickened, of dark-brown colour, and traversed by numerous high rugæ. The mucous and submucous tissues were almost gangrenous, and very friable. The small intestines were of a pale pink colour externally, but polished and glistening. The mucous membrane was reddened from the pylorus to the cæcum; the reddening was most marked near the pylorus, but there were occasional spots, the size of a dollar, of a brown colour, and almost gangrenous. The lungs, liver, and spleen were natural. The uterus, four inches in length, and two and a half inches in width at the fundus, was evidently enlarged; the neck appeared elongated, and the os was found filled with a mucous plug. Within, the right and left corners of the fundus each contained an ovum a few weeks old; there was a corpus luteum in each ovary, and the orifices through which the ova escaped from the ovaries were plainly visible. The corpus luteum was of a dark colour, except a small portion of the circumference, which was yellow. The kidneys were much injected; and the heart, natural in character, was filled with blood partly coagulated. The analysis, made by Dr. White, showed that the poison used in this case was corrosive sublimate.—*Boston Medical and Surgical Journal*, Oct. 17th, 1861.

[There are two points of great importance in this case. It has been assumed, that in poisoning by corrosive sublimate the stomach and colon may be the seats of active mischief, while the whole of the small intestinal tract remains free; how hastily and incorrectly this conclusion has been set up the above case illustrates. Secondly. It has been asserted, with a positiveness, doubtful, indeed, from its over-protestation, yet misleading, that after poisoning by corrosive sublimate, the slate-coloured condition of mucous membrane, which has been so well described as occurring in inferior animals, never occurs in the human subject. Dr. Cabot's case disproves this assertion.—B. W. R.]

Poisoning by Prussic Acid.—S. Lawrence Gill, Esq., L.R.C.P.E., reports the following case:

M. A. C——, a French chemist, after a few words with his employer, entered the laboratory at half-past four P.M. An hour having elapsed, and he having frequently been heard to say that he always carried a deadly poison, one drop of which would produce sudden death, the gatekeeper felt alarmed, and ventured to open the door of the room; but seeing the chemist sitting in an easy posture (and, as he thought, looking him impudently in the face), retired. The gatekeeper communicated what he had seen to the clerk, who exclaimed, "I believe he has committed suicide. I was dreaming all last night that he had done so. Let us go together and see." They did go, and found the man speechless. In an hour and a half from his being last seen alive, Mr. Gill was on the spot, and found the patient fully dressed, and sitting in an easy, natural posture, his elbow resting on a table, his hands folded together, and his knees crossed. A one-ounce bottle (empty), labelled "Prussic Acid," and with the *stopper replaced*, stood close to his right elbow. There was no indication of the slightest struggle. He sat staring those who approached him in the face, his head slightly thrown back, his forehead suffused and of a lead colour, his cheeks pale, and his lips purple. A clammy, cold perspiration bedewed his face; his eyes were staring, dry, and glassy, the conjunctival vessels brilliantly injected, the pupils greatly dilated, the eyelids rigid; the nostrils dilated and rigid. The tongue and lips were dry, brown, cold, and firmly pressed against the hard palate; the lips and jaws were apart; the body coldish, moist, and rather rigid. Mr. Gill could not detect the slightest odour of prussic acid either from the patient's mouth or in the room, although but an hour and a half had elapsed since the patient had entered it alive. No scream was heard from the man, nor were there any indications of convulsions; but he had passed a black motion into his trousers. Mr. Gill remarks in regard to this case, that ascertained facts respecting death from prussic acid (even if the patient be not seen until after death) may be of value. 1. In this case no scream was heard, although the top of the laboratory window was down some inches, and a person stood outside, within three yards of that window, for some time, and was within ten yards from the time deceased entered until he was found dead. 2. There was a total absence of froth from the mouth, as the lips, tongue, and fauces were perfectly dry and horny, and not the slightest moisture existed either on his neck-tie or the front of his shirt. 3. There was no indication of convulsions, which, owing to posture, must have been evidenced had they been present.—*Lancet*, Nov. 2nd, 1861.

Symptoms of Poisoning from eating Common Honey.—Dr. George Bidie, of Mysore Farm, relates that he obtained some honey from the Coorg jungle. The honey was stored in the joint of a bamboo. It looked clear and wholesome, and had the usual smell and taste. It was placed on the breakfast-table, and on three successive mornings he (Dr. Bidie) partook of a little of it, about a teaspoonful on each occasion. On the second and third day he felt a disagreeable itching over the whole body; but as the weather had suddenly become very cold, he thought the itching had in some way been caused by the change of temperature. On the third night he awoke with intense headache, and a feeling of constriction about the forehead. The headache did not permit of much sleep; so getting up early, he took a long walk, in hopes that exercise might relieve him of the pain. He got worse, however, and returning to his quarters, immediately took a couple of purgative pills and went to bed. He then had a feeling that some article of food had disagreed with him. Towards evening the headache left, but the constriction about the forehead remained. The itching had become worse, and numerous red spots appeared all over the body. On the fifth day he felt quite well, except that the itching was very troublesome. On the sixth he again took some of the honey at breakfast, and shortly afterwards was purged, and the whole body was covered with an exanthematous eruption of prominent, irregular, red patches, most conspicuous on the face, back of the hands, and about the larger joints. The headache and constriction of the forehead returned, and he suddenly became very sick, and felt so prostrated in strength, that he could hardly walk a few yards to a couch. There was no desire to vomit, but a sickness and prostration so intense, that he could hardly move a limb or speak. This condition lasted for about three hours; then a short sleep occurred, and he awoke to find the headache much relieved and the sickness quite gone. The eruption had partly disappeared. There were, however, intense thirst, a feeling of soreness in the throat, and great hoarseness of the voice. Dr. Bidie mentions, that although accustomed to eat the honey of the English hive-bee, it never produced any such effects on his system.—*The Madras Quarterly Journal of Medical Science*, October, 1861.

Poisoning by Cyanide of Potassium.—At a meeting of the Liverpool Medical Society, on October 17th, 1861, Mr. Byerley exhibited the stomach, liver, and intestines of a man who had died from poisoning by cyanide of potassium. The following is the history: A stout, well formed man, forty-six years of age, was seen on Wednesday, October 9th, wandering about the Wallasey sandhills, his manner and appearance suggesting that his mind was disordered. On the succeeding Friday his body was found lying on the left side. Close behind him were a bottle still containing a small quantity of a solution of cyanide of potassium and a three-ounce measure-glass. The coroner requested Mr. Byerley to examine the body on the Monday afterwards. He found discoloration of the abdominal walls from incipient decomposition; a blue hue of the hands and nails; hypostatic congestion of the left side generally; and a livid look of the face, tongue, and lips, the latter being distorted as if by some convulsive movement in the last moments. There was an extremely dark red condition of the small intestines, the bloodvessels of which were greatly engorged. The liver, spleen, and kidneys were darkly congested, and the lungs also in a slighter degree. The heart was empty, and the large veins not remarkably full. The stomach was much contracted, and nearly empty; it had the shape of a large intestine, with a sort of hour-glass constriction midway between the cardiac and pyloric ends. It was greatly engorged, and of a deep red or chocolate colour. When it was opened, about two ounces of grumous fluid mixed with mucus flowed out. The whole of the mucous membrane was intensely dark red (almost black), more especially at the pyloric third. Its folds were thrown into prominent rugæ, and in two or three places were slight patches of abrasion. There was a well-marked odour of prussic acid. The man, who was an habitual drunkard, was an engraver, and practised photography as an amateur. The contents of the stomach afforded the usual precipitate of the cyanide of silver when tested with the nitrate. There was no clue as to whether death was speedy or comparatively protracted after the poison had been taken. From the position of the body close to the bottle which had contained it, the former appeared to be probable; whereas the intensity and extent of the pathological condition of the organs affected might lead to an opposite inference.—*British Medical Journal*, November 2nd, 1861.

Fatal Case of Strychnine Poisoning in a Girl aged eleven years.—Dr. Harley reports the following case. On the evening of June 6th, 1860, he (Dr. Harley) was called to meet in consultation Mr. Cholmondeley and Mr. Wakefield, at the house of a chemist and druggist, whose daughter was stated to have taken strychnine. The patient, a remarkably intelligent girl, only eleven years of age, although she spoke and acted like a girl of seventeen years, was first seen by Mr. Wakefield at seven P.M. On his arrival, he found her in a state of violent tetanic spasm, the limbs extended and rigid, the jaws firmly fixed, and the body in a state of opisthotonos. When the paroxysm passed off, the girl confessed that she had gone into her father's shop and taken a quantity of pure strychnine (the exact amount was not ascertained). Although very much alarmed and agitated, her intellectual faculties remained perfectly clear. She eagerly, and without any apparent difficulty, swallowed an emetic of sulphate of zinc. As the first dose did not act immediately, a second was given, and shortly afterwards the patient vomited a quantity of semi-fluid matter. She then turned upon her back, and lay with her eyes closed. On raising the eyelids in order to ascertain the state of the pupils (which were found natural), a violent tetanic paroxysm was induced. Just as the spasm commenced, the patient shrieked out, "Hold my legs!—hold my legs! I am dying!—I know I am!" Chloroform was now administered, but without the slightest benefit. The attacks rapidly succeeded each other, and gradually increased in severity. During the spasms, the pupils were observed to dilate and the countenance to become livid. In the last and fatal convulsion, the face suddenly became almost black, probably from a non-arterialization of the blood, for the muscles of the chest were so firmly contracted, that the respiratory movements were completely arrested. As the spasm passed off, the facial muscles relaxed, and a quantity of frothy mucus issued from the mouth. There was now, however, no longer any attempt at respiration. Life had passed away with the spasm, for the heart was still.

It was curious to observe that in this case, as soon as life became extinct, rigor mortis commenced. There was no very apparent interval of muscular flaccidity. In five minutes after the cessation of the last spasm, and while the body was quite warm, the muscles of the hands and arms were in a state of rigor mortis, while those of the lower extremities passed into a similar state in the course of ten minutes more.

At the autopsy the legs were found rigid and extended as they were during the spasms. The soles of the feet were strongly arched and inverted. The hands were clenched and remarkably livid; the fingers and nails of an indigo blackness. The upper extremities were no longer livid; the lividity of the face, remarked during life and immediately after death, had also disappeared. The inner side of thighs and arms, also the side of the neck, were of a scarlet colour, very like what is seen in cases of scarlet fever; the outside of the limbs were of the normal colour. The countenance wore an unusually determined look, particularly about the mouth. The surface of the brain was very much congested; the ventricles contained rather more serum than usual, otherwise the brain was healthy. Great effusion existed under the arachnoïd, otherwise the cord appeared healthy. Chest: The lungs were much congested; the blood in the large vessels was quite fluid and dark in colour. The pericardium contained about two ounces of serum. The heart was flaccid, and nearly empty. Abdomen: The stomach presented no appearance of inflammation; near the pyloric end was a yellow spot (from the colouring matter of the bile). Notwithstanding the frequent vomiting, there was still about ten ounces of fluid and solid matters in the stomach; the different foods—beef, potatoes, and bread—were easily recognised. The liver and kidneys were normal. The small intestines very red from congestion. The urinary bladder was quite empty and contracted. Mr. Wakefield tested the contents of the stomach, and readily detected the presence of strychnine.

Dr. Harley goes on to remark that the chief features of interest presented by this case are as follow. The fact that the girl, after taking the fatal dose, deliberately sat down to tea, and continued eating and talking as if nothing unusual had occurred, until the spasms commenced. The fact of the patient being able to swallow liquids, even after the convulsions had begun. The perfect consciousness of the patient seems to be a characteristic feature of strychnine poisoning. The pupils were observed to dilate during the spasms, and to return to their normal dimensions during the intervals. The immediate cause of death in this case appears to have been asphyxia, induced by spasm of the respiratory muscles.—*Lancet*, October 26th, 1861.

Deaths from Chloroform.—Since our last report an unusual number of cases of death from chloroform have occurred. On September 3rd, 1861, a man, named Cassmach, died in the Newcastle Infirmary whilst under the influence of chloroform. He was weak, and laboured under scrofulous disease of the knee-joint, which joint was condemned to amputation. Before the operation he exhibited great trepidation and alarm, and took brandy in consequence. A small quantity of chloroform was then administered, and the patient seemed rapidly to come under its influence; when all of a sudden there were relaxation of the sphincters, quivering of the lips, and death. The breathing continued for a little time after the heart had ceased to beat. On post-mortem examination the right cavities of the heart were found filled with fluid blood. It does not appear that any serious organic lesion of the vital organs existed, and the death may probably be ascribed in part to fear. The jury took this view of the case, and ascribed the death as due to the united effects of fear, debility, and chloroform.—*Medical Times and Gazette*, September 11th, 1861.

On Thursday, the 6th of September, 1861, a *second* death from chloroform occurred at the Cumberland Infirmary, Carlisle. The patient, a young man, had been in the infirmary for about a fortnight, and it was found necessary to perform some surgical operation upon him. The anæsthetic was administered by the house-surgeon, Mr. Devereux, in the presence of Mr. Page, the visiting surgeon. The patient was soon in a state of unconsciousness, and the operation was about to be performed, when alarming symptoms presented themselves. The patient ceased to breathe, and animation seemed to be suspended. The surgeons took immediate steps to revive him. Galvanism and artificial inflation of the lungs were in turn resorted to, but they both failed to bring the patient round. It was now evident that he was quite dead."—*Ibid.*, September 14th, 1861.

A *third* case of death from chloroform gave rise to an inquest on Saturday, November 9th, 1861. The patient was a boy, aged eight years; whose death occurred while under the influence of chloroform on the previous Wednesday. Mr. Edwards, chloroformist to St. Mary's Hospital, deposed that deceased was admitted on the 25th ultimo, to undergo an operation to remove a great deformity occasioned by a burn on the chin, which had the effect of drawing the chin down to an unusual degree, and turning the under lip inside out. On the day appointed, the witness administered the chloroform gently. It took ten minutes to get the boy under its influence, and then the operation was commenced by Mr. Lane, the senior surgeon. Just before the conclusion, the deceased fainted, and with a view to restore him witness promptly commenced artificial respiration, which was kept up for half an hour. Failing in his efforts, the boy was put in a warm bath, and galvanism was applied for an hour and a half, but without success. Witness believed the boy died instantly he fainted, from paralysis of the heart. In a post-mortem examination they found all the organs healthy, and no trace of disease anywhere. Death was solely from paralysis of the heart from the effects of chloroform."—*Ibid.*, November 16th, 1861.

A *fourth* case, similar in kind, is recorded by W. E. Nourse, Esq., of Brighton. Mr. Nourse was requested by Mr. Field to administer chloroform during an operation for the removal of internal piles by the écraseur. The patient was a large, stout, very muscular man, aged fifty. The abdomen was prominent, and the face that of a man accustomed to drink, but not indicating any abnormal condition of the heart or other organ. There was no arcus senilis, and the pulse and respiration appeared natural. Two measured drachms of chloroform, poured on a hollow sponge, were first inhaled in the usual gradual manner. The stage of excitement was strongly marked with much muscular action and vociferation. After several minutes, the chloroform being exhausted, forty or fifty minims more were poured on the sponge, and inhaled at intervals. While the vociferation and struggling yet continued, some stertor appeared, and the sponge was at once withdrawn. In another minute full stertor came on; the face, without any pallor, showed a dusky livid hue; the pulse ceased, and the respiration was becoming visibly slower. Mr. Field had not begun the operation. The surgeons instantly commenced artificial respiration, and slapped the face and chest with a wet towel; but only a few more inspirations, or gasps, could be obtained. They continued the same means, took blood from the jugular vein, which was turgid, rubbed the limbs and applied electro-magnetism with the kind assistance of two other medical men; but after the lapse of an hour no sign of life was elicited, and the body becoming cold, further efforts were evidently useless. On a post-mortem examination, the heart was found loaded with fat, its muscular substance thin and weak, and the walls of the right auricle and ventricle in a state of fatty degeneration. These same cavities (the right) were gorged with fluid blood. The valves and great vessels were healthy, and no other organ appeared to be in any abnormal state. Death had clearly occurred from failure of the heart's action, induced much more readily than would have happened under natural circumstances by chloroform, owing to the weakened and encumbered condition of the heart.—*British Medical Journal*, November 16th, 1861.

A *fifth* case occurred in St. Mary's Hospital on the 3rd of January, 1862. The patient, James Hedger, aged thirty-six, was admitted into the hospital for an affection of the hip-joint. On admission, the thigh was semi-flexed, and the patient complained of great pain in the joint on any movement of it. It was determined to put the limb up in a long straight splint, so as to ensure perfect rest. Owing to the rigidity of the muscles, and the pain caused by movement, he was to be put under the influence of chloroform. The chloroform was administered in the usual manner with one of Snow's inhalers, one drachm being placed in the receiver. It was administered with more than usual care, owing to the patient being extremely nervous. After breathing the chloroform for a minute or so, the patient had a violent shaking fit; he was then quite sensible. The inhaler was removed, and on being spoken to he became quite calm. The chloroform was then continued, another half drachm being put into the apparatus, and he gradually exhibited the usual symptoms of a person coming under the influence of chloroform. He had just arrived at the stage of rigidity of the muscles, and was endeavouring to raise himself up in bed, when the muscles relaxed with great rapidity. He fell back, his face became pale, the breathing laboured, and the pulse, which up to this time had been very good, suddenly stopped. Artificial respiration was immediately commenced, and galvanism was applied over the region of the heart. Artificial respiration, carried on by extending the arms and compressing the thorax, was continued for about one hour and a half. Acupuncture was also tried. On a post-mortem examination, the cavities of the heart were found full of fluid blood; the heart itself was large, and loaded with a deposit of fat; the liver, kidneys, and brain were very much congested, but otherwise they were quite healthy.—*British Medical Journal*, January 11th, 1862.

A *sixth* record of death from inhalation of chloroform vapour is communicated by Mr. George Turnley, of the General Hospital, Hobart Town. The patient, a fine muscular man, thirty-five years of age, was put under the influence of chloroform for the purpose of having the left testis removed, that gland being the seat of malignant disease. The anæsthetic was administered in the usual way, and great care was taken to ensure a proper admixture of atmospheric air. Nothing peculiar occurred during the inhalation; the breathing was remarkably quiet and regular; there was no turgescence of the superficial vessels of the head or face; and in about four minutes from the commencement of the inhalation the patient was fully under the influence of the drug. At the moment that soporose breathing commenced, the pulse became feeble, and the chloroform was immediately withdrawn; the pulse, however, rapidly failed, and in twenty seconds ceased to beat. The ear applied over the præcordial region could not detect the slightest action of the heart. During this period the respiration continued regularly, and after the entire cessation of the heart's action the patient made eight or ten regular inspirations and expirations, two or three irregular ones, and then respiration ceased. Cold water was dashed on the face, fresh air admitted into the apartment, and the Marshall Hall ready method was at once resorted to; ammonia was applied to the nostrils, friction to the extremities, and a galvanic current passed from the spine through the thorax to the diaphragm. All these measures were applied without delay, and unremittingly persevered in for nearly an hour, but without success. On making a post-mortem examination, no trace of organic disease could be detected; the blood was fluid, and the organs generally congested. At the moment that the heart ceased to beat, the ventricles were in a state of dilatation; the valves were perfectly healthy, and the muscular tissue of the organ did not exhibit the slightest trace of fatty degeneration.—*Lancet*, February 22nd, 1862.

Apropos to the facts above stated, we may refer to a paper by Dr. Charles J. Jackson, "On the Action of Chloroform on the Blood," published in the 'Boston Medical and Surgical Journal' for March 28th, 1861. Dr. Jackson relates the case of a healthy young woman who died while inhaling chloroform. After death, the blood taken from the right side of the heart was found to contain formic acid and chlorine, but no undecomposed chloroform. The blood did not coagulate, and remained six years without undergoing change. He thinks this explains the cause of death from chloroform; he is of opinion that when chloroform is inhaled it abstracts oxygen from the blood, and, combining with the formyle, forms formic acid, while chlorine is set free, and combines with the blood as a substitute for oxygen. Thus, a portion of the blood becomes chemically changed, devitalized, and rendered unfit for life. He advocates ether as a much more safe and sure anæsthetic than chloroform, or a mixture in which chloroform is largely diluted with ether.

Treatment of Rattlesnake Bites.—Dr. Weir Mitchell, in a separate work on this subject, discusses the following points: 1. Fallacies in regard to the use of antidotes of all kinds, arising from want of exact knowledge as to the secretion of the venom and the mode in which the serpent uses its fangs and ejects the poison. 2. Fallacies as to antidotes, arising from want of information on the natural history of the disease caused by the venom. 3. General considerations as to antidotes and as to the mode of conducting researches in this direction so as to avoid errors. 4. Description of the phenomena of rattlesnake bites and analyses of symptoms. 5. Local treatment. Experimental examination of the local medication most in repute. 6. General or constitutional treatment. Experimental examination of the principal constitutional remedies. 7. Sketch of the author's views as to treatment, local and general. The work throughout sustains the reputation which Dr. Mitchell has so deservedly earned as a careful and acute observer of natural phenomena. We regret, therefore, to have to state that, according to his observation, the much vaunted general remedies now commonly used as antidotes—such as ammonia, olive-oil, cedron, guaco, arsenic, alcohol, and even Bibron's antidote—have proved experimentally to be one and all fallacious. In addition to local treatment by ligature, destruction of the bitten part, or injection of iodine, on the plan proposed by Brainard, Dr. Mitchell suggests as constitutional remedies the mineral acids and quinine, as those from which most benefit is to be anticipated.—*Essay on the Treatment of Rattlesnake Bites.* Philadelphia, 1861.

SUMMARY OF PAPERS AND REFERENCES.

On the Detection of Strychnia as a Poison, and the Influence of Morphia in disguising the usual Colour-Test. By John J. Reese, M.D.—Dr. Reese, from several experiments, shows that the presence of morphia materially interferes with the colour-test for strychnia, but that it produces no impression on the almost equally delicate frog test. The same author also points out as an original fact, that the microscope may be employed as a means for detecting strychnia. Even the $\frac{1}{1000000}$th of a grain may thus be recognised, when the alkaloid is in a state of purity. The appearance presented to the eye is that of numerous crystals, some acicular, others stellate and scalloped, intermingled with dentated crosslets.—*American Journal of the Medical Sciences,* October, 1861.

A Case of Poisoning by Stramonium. By C. E. Buckingham, M.D., of Boston, U.S.—In this case, a boy, six years old, took from two hundred and fifty to three hundred stramonium seeds. The skin assumed a bright red colour, the pupils were dilated. There was stupefaction, but apparently little anæsthesia. After free emesis the patient slowly recovered.—*Boston Medical and Surgical Journal,* Oct. 31st, 1861.

Three Cases of Poisoning by Stramonium. By J. O. Harris, of Ottawa. *Ibid.,* Nov. 14th, 1861.

Case of Poisoning by Aconite, successful use of Nux Vomica as an Antidote. By D. D. Hanson.—In this case Dr. Hanson, in the last stages of poisoning by aconite in a boy, five years old, gave three drops of tincture of nux vomica, repeating the dose at the end of twenty minutes. The pulse rose immediately, and vigorous emesis followed. The lad recovered. Dr. Hanson infers that nux vomica is, "a complete antidote to aconite, and that conversely aconite is an antidote to nux vomica."—*Ibid.,* Sept. 26th, 1861.

On the Antagonistic Effects of Opium and the Mydriastias. By Charles C. Lee, M.D.—In this communication the author endeavours to show, from the treatment of cases in the human subject, that opium is an antidote to stramonium and belladonna, and that belladonna is a remedy for opium.—*American Journal of the Medical Sciences,* January, 1862.

On Milz-brand Poisoning. By Dr. Moriz Gauster.—The disease milz-brand is an affection attacking cattle, by which the spleen is transformed into a dark fluid mass. The animals often die very rapidly, with symptoms of an acute fever, which seems to be induced by bad food and air. Dr. Gauster reports cases of four women and one man, who, after partaking of the flesh of the affected animals, suffered from serious symptoms, ending in one case fatally, from malignant pustular disease, affecting the head.—*Wochenblat der Zeitschrift der k. k. Gesellschaft. der Aerzte zu Wien*, Nov. 1861, Nos. 45 and 46.

On Poisoning by Opium. By A. Duchek.—*Ibid.*, Nos. 43 and 44, 1861.

On Poisoning by Corrosive Sublimate. By Dr. L. A. Bayer.—*Aerztl. Intell.* B. 43, 1861.

On Poisoning by Stramonium. By M. Liégey.—The case of poisoning recorded by the author presented symptoms which resembled closely those of scarlet fever. The skin was red and the throat showed signs of angina. The case ended fatally with convulsions and coma.—*L'Union Médicale*, No. 6, Jan. 16th, 1862.

On Poisoning by Prussic Acid.—*British Medical Journal*, Jan. 10th, 1862.

Fatal Case of Poisoning by Strychnia, in a Boy, aged Twelve Years. By W. B. Tarleton, Esq. Reported by George Harley, M.D.—*Lancet*, Nov. 16th, 1861.

Case of Poisoning by Strychnia. By William Travers, Esq.—*Ibid.*, Oct. 12th, 1861.

Case of Poisoning by the Bichloride of Mercury. By John J. Skegg, L.R.C.P. Edin.—*Ibid.*, Feb. 1st, 1862.

New Test for Morphia.—M. Lefort publishes, in the 'Répertoire de Pharmacie,' a new method for the detection of the smallest traces of morphia with a degree of certainty hitherto, he says, unknown; his procedure is founded on the remark, that when organic substances decompose iodic acid, the iodine set at liberty is usually absorbed by caustic ammonia, and the mixture completely discoloured; but morphia, on the contrary, which becomes red or brown by iodic acid, acquires by the addition of ammonia *a much deeper colour.* Even if the suspicious solution contains but one ten-thousandth part of morphia, the pale yellow tint produced by iodic acid will become much darker.—*Journal of Practical Medicine and Surgery*, October, 1861.

Case of Poisoning by Opium treated by Belladonna. By Dr. Murray.—*Australian Medical Journal*, Oct., 1861.

II. INSANITY.

The Social Element in Treatment of the Insane.—Dr. McFarland, in his 'Report of the Illinois State Hospital for the Insane,' thus observes on the influence of moral treatment as compared with mere medicinal treatment in the management of the insane: "Success in the treatment largely depends on the ability to engage every mind in some sort of employment or recreation. The doctrine that insanity, even when unconnected with palpable physical disease, is to be treated by the appliances of the apothecary's art merely, belongs to a bygone age. Hence no means is to be despised that will draw any mind from its morbid contemplations.

"In the winter season, mattress-making and mat-braiding, both involving considerable labour in the preparation of material, are excellent methods of engaging the attention of many who would otherwise find the time hang heavily. For in-door's amusements, books, newspapers, games of chance and skill, with an occasional social *réunion*, at which large numbers of the insane of both sexes, with their attendants, join for a few hours in temperate festivities, are all relied upon; and each commends itself to some especial class. Yet after enumerating all the ordinary recreations in use to while away the monotony of hospital life, the list would be radically deficient if no mention was made of what is, after all, the most unfailing of all entertainments—the mutual attrition of minds so abounding in angularities and eccentricities, that thought assumes shapes grotesque enough to amuse the gravest auditory. No one can be long in an institution for the insane without perceiving that the influence of one insane person upon another is generally good, notwithstanding excitements among them are to a degree contagious.

"Some of the most salutary influences have been proved to have arisen from the contemplation by one insane person of another in a still worse condition. But the most striking of these mutual influences is produced by the entrance of some new-comer, who brings into the common social stock some accomplishment of a novel kind, or some new 'sensation idea,' by which he can make himself conspicuous. One gentleman, who could utter vocal sounds from his throat nearly resembling the strains of an Æolian harp, was for some months as good as an ever-present instrument of agreeable music. Another gentleman, of fine education and much general intelligence, with a singular mental activity, has kept those about him for months together on the high wave of interest at a scheme for founding the 'Republic of Pomona in the South Orkney Islands.' From the first conception of this plan to its present complete development, every department of art and science in any way contingent to it has been discussed with a thoroughness that has been quite exhaustive. The disquisitions pronounced upon geography, navigation, purveyance, political economy, municipal government, and State religion, would have done no discredit to the author of the 'New Atlantis.' These spontaneous sources of interest are sometimes better than any set entertainment."

It will be recollected that on former occasions the subject of the treatment of insane prisoners in the ordinary hospitals has been pretty fully discussed, and the arguments of several medical superintendents against the practice presented. Dr. McFarland here gives his views, closing in the following language:

"As all are liable to the sad visitation of insanity—the person of refined and sensitive nature as well as others—and as the radical principle of all insane hospital treatment is that of regulated association of numbers together, this subject cries loudly for the aid of the philanthropic legislator. Now, while the penitentiary at Joliet is in process of construction, is the time when this reproach should be taken away from this institution. Every prison of course has its infirmary, and it only needs the attachment of some rooms of greater strength to give such cases of insanity as may arise comfortable accommodation. Then, the repeal of any Act authorizing the transfer of such cases would for ever remedy the evil. Either this must be done, or the ends of justice and the designs of philanthropy must continue to be infringed.

"The subject reduces itself to the plain question: '*Which is the more proper, to have a hospital attached to a penitentiary, or to have a penitentiary attached to a hospital?*' The former is a necessity in all instances, a human juxtaposition which should never be wanting: the latter is a needless incongruity, corrupting to the whole employed corps of the hospital, and, if suffered to continue, would surely be eventually regarded as a reproach upon the ruling sentiment of the State."—*The American Journal of the Medical Sciences*, July, 1861.

III. OBSTETRIC JURISPRUDENCE.

Early Maternity.—Dr. J. G. Wilson records an instance in which pregnancy occurred in a girl thirteen years old. The case is so important in a medico-legal sense, that we quote it entire, as given by Dr. Wilson:

"Pregnancy before the age of fourteen, although not uncommon in tropical climates, is in this country an event of very rare occurrence. The following case of parturition at an unusually tender age is, I think, deserving of record. For the subjoined report of the case I am indebted to Dr. Macdowall, of Helensburgh, under whose kind and able care the patient was.

"'On the 17th November last I was called to visit J. W——, a girl aged thirteen years on the 4th of July last (according to the record of the family Bible), in consequence of some abdominal enlargement. I soon satisfied myself that the enlargement of the abdomen was due to pregnancy, and intimated my opinion accordingly to the relatives, who received the intelligence with much amazement and doubt. I watched the case from time to time; and as pregnancy is very uncommon at such an early age, I requested my friend, Dr. J. G. Wilson, of Glasgow, to see the patient with me, who at once confirmed my diagnosis. On inquiry, I ascertained that the catamenia appeared for the first time in January, and that she menstruated regularly till the end of April. The patient appeared quite ignorant of her condition, and made no complaint.

"'On 11th January I received an urgent call to visit the girl, and on my arrival I found she had shortly before given birth to a full-grown female child. She could not have been more than three hours in actual labour. I should infer, from the way I found the infant lying in bed, that the presentation was natural. I at once detached the child and removed the placenta. The patient made an excellent recovery. The breasts (which had been suppurating previously) healed up, but without any appearance of milk. The lochial discharge was quite natural as to quantity, character, and duration. Four weeks after delivery, the mother and child were transferred to a neighbouring county, and shortly afterwards the child died, I presume from bad nursing. The age of the lad who acknowledges the paternity is nineteen years.'

"*Remarks.*—From the foregoing report it will be observed that the girl, J. W——, at the early age of thirteen years and six months, gave birth to a full-grown female child. Conception must therefore have taken place when the girl was twelve years and nine months old. Very few examples are on record where pregnancy has occurred at such an early age. I believe the case narrated by Mr. Roberton of Manchester is the earliest authentic instance of pregnancy that has occurred in this country. The girl, who worked in a cotton factory, became *enceinte* in the eleventh year of her age, and at the period of her confinement she was only a few months advanced in her twelfth year. When in labour she was seized with convulsions, but ultimately, without unusual difficulty, was delivered of a full-grown but still-born child. The catamenial function was established before conception took place.* Another well-accredited case of pregnancy in very early life is related by Mr. Smith of Coventry. A girl, named Julia Sprayson, when twelve years and seven months old, gave birth to a full-grown, healthy infant, after a short and favourable labour. This girl began to menstruate when ten years and six weeks old.† La Motte, in his 'Traité des Accouchemens,' records the case of a girl who gave birth to and nursed a child before she was thirteen years of age. (Obs. xxiv. p. 41.) We are told by Dr. Paris, in his 'Medical Jurisprudence,' that 'during the year 1816 some girls were admitted into Maternité at Paris as young as thirteen years; and during the Revolution, one or two instances occurred of females at eleven, and even below that age, being received in a pregnant state into that hospital.' Professor Montgomery states that the earliest instance of impregnation that has come under his own observation was that of a young lady who was delivered of twins before she had completed her fifteenth year. The earliest instance of utero-gestation which is known to have occurred in the Glasgow Lying-in Hospital was that of a girl who was delivered at the full term when fifteen years and a month old."—*Reprint from the Edinburgh Medical Journal*, October, 1861.

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* Roberton's Midwifery, p. 30. † British Record of Obstetric Medicine, vol. i. p. 360.

QUARTERLY REPORT ON SURGERY.

By John Chatto, Esq., M.R.C.S.E.

I. *On Spasmodic Contraction of the Sphincter of the Vagina.* By MM. Débout and Michon. (Bulletin de Thérapeutique, vol. lxi. Nos. 3, 4, and 7.)

M. Débout desires to call attention to an affection of the vagina which, from the little notice taken of it in surgical works or clinical lectures, would seem to be of somewhat rare occurrence. It is especially observed soon after marriage, but it is also met with in women who have borne children. It consists in a spasmodic contraction of the vagina, which, by presenting an obstacle to the completion of the act of copulation, becomes a cause of barrenness. All attempts at sexual approach arouse the spasmodic action of the muscle, producing excessive and unbearable pain until suitable treatment has been resorted to. The contraction being usually a secondary affection, it is in the etiology the practitioner must seek his therapeutical indications. Its causes are, indeed, numerous—as inflammation of the mucous membrane, herpes or eczema of the vulva, and inflammation of the mucous follicles; but the two most frequent causes, especially among women whose nervous system is highly impressionable, are hyperæsthesia of the vulvar mucous membrane and fissures at the entrance of the vagina. It will be easily understood, that an excitable state of the nervous system must predispose women to this spasmodic affection; and, in fact, in such subjects the slightest painful affection of the vulva induces reflex actions, which lead at last to a spasmodic contraction of the muscular plane situated at the lateral parts of the vagina. Another predisposing cause is found in the anatomical disposition of the parts in certain women. The perinæum in such individuals is so highly placed, that when the woman is in the horizontal position, in order to effect the introduction of the finger or the speculum, this has to be carried in a more or less oblique line from above downwards, and from without inwards, so as to form a more or less acute angle with the pubis, in place of being directed horizontally, as in the ordinary state of parts. When this conformation exists in a newly married woman, the coïtus may produce laceration of the upper part of the hymen. Then, again, the ineffective efforts at penetration, in some cases, resulting either from want of perseverance or from defective virility, may create an irritation of the tissues which may be followed by spasm.

In those cases which have been hitherto recorded, the treatment by incision has been put into force; and M. Débout thinks that has been the case in some instances when milder measures, and less alarming to the patient, would have sufficed. He believes that surgeons have too readily endeavoured to relieve the contraction without due reference to its causes. Of these, hyperæsthesia and fissure are the most frequent, and the former condition is best relieved by cold, employed by means of hip-baths and enemata, and especially by ice frequently renewed. The fissure of the vagina is best treated by a concentrated solution of nitrate of silver, applied every two or three days with a pencil over the whole extent of the eroded surface. When the sensibility of the parts has become abated, dilatation of the contraction may be gradually accomplished by the insufflation of caoutchouc *ampullæ* resembling those which are employed to arrest hæmorrhage from the rectum.

M. Débout relates two interesting instances in point, and their publication has caused accounts of many other cases to be forwarded to him. The most interesting of these communications is from M. Michon, who communicates notes of eleven of these cases which have occurred in his own practice, chiefly in consultation with Chomel. The cases were, in some instances, examples of true contraction of the sphincter, a morbid condition of the muscular fibre, which may occur at any epoch, even in a woman who has borne children. But in most of the examples, conjugal relations had never been accomplished. In these latter cases, mere superficial incisions were usually required implicating only the mucous membrane irritated by the unsuccessful attempts at cohabitation, and those portions of the partially destroyed hymen which were always found to persist. These incisions never reached the muscular fibres, and were only made, in fact, to facilitate the introduction of tents, the gradual dilatation by which overcame the contractions. When, however, the contraction was more complete, and resistance of muscles, not of a mere irritated and thickened membrane, had to be overcome, the case assumed a great resemblance to the contraction of the anus resulting from fissure; and forced dilatation by means of the fingers, with or without subcutaneous section, constituted the best treatment. Where the patient objects to this, the somewhat slower process of dilatation by tents must be substituted. As to the primary lesion, so much insisted upon by M. Débout, no fissure has ever been met with in any of M. Michon's cases; and even if it were present, that would not deter him from treating the contraction at once, as the healing the fissure would be the consequence of the removal of this. M. Michon observes, that one reason why this affection has been so little noticed in books is, that it is rarely met with in hospitals. Its subjects usually belong to a higher sphere of society, in which education developes the functions of the nervous system at the expense of the physical powers, and in which sensibility being exalted, spasmodic affections more readily occur.

II. *On the Relation of the Insertion of the Capsule of the Hip-joint to Intra-Capsular Fracture.* (American Medical Times, Dec. 14 and 21, Jan. 25.)

Dr. George Smith, on introducing this subject at the New York Academy, gave the following summary of his views: 1. The insertion varies so much that scarcely any two specimens of the normal capsule taken from different subjects can be said to be inserted at the same point; so that while a fracture of the neck at a given distance from the inter-trochanteric line will be found in one specimen included within the capsule, it will be half an inch or more external to this in another. 2. The descriptions of the insertions given in works of anatomy are even more widely at variance than the differences seen in nature, not one of the writers intimating that the insertion ever varies from the position which he has assigned to it. 3. The insertion is often removed by the morbid changes consequent on the fracture, so that the capsule of the fractured bone cannot furnish reliable evidence that the fracture was within the normal capsule. However much the normal insertion of the capsule may vary, it never takes place at a point so distant as the shaft of the bone, where, however, the capsule of a fractured bone is often found inserted, the neck of the bone having been removed by absorption before union occurred. 4. Capsular ligaments of the opposite femurs of the same subject are always alike in their insertions. After death, a comparison of the two sides will therefore at once determine whether the line of union be altogether within the normal capsule. 5. The line of union in a given specimen cannot be said to indicate the exact position of the line of fracture, if the neck suffered loss by absorption before union occurred, as it is impossible to determine that the loss of structure was entirely at the expense of either fragment of the neck. 6. Under favourable circumstances, fractures of the neck external to the capsule unite readily by bone, as do fractures which are partly within and partly without; and it is highly probable that fractures within the capsule which are followed by absorption are sometimes united by bone after the process of absorption has reached a point external to the normal capsule where bony matter is supplied. But this can never be proved; for if the line of union be partly without the capsule, it is impossible to determine that the fracture was entirely within it. We can never be positive that bony union of intra-capsular fracture has occurred until a specimen is presented in which the line of union is found to be entirely included by the normal capsule. 7. Fractures of the cervix are in most instances followed by the absorption of a part or the whole of the neck; and a careful review of the cases recorded as proofs of bony union of intra-capsular fracture shows that, in the great majority of cases, the posterior surface of the neck had lost very much of its length by absorption before union had occurred, and that the line of union on this surface, although included by the morbid capsule, was too near the shaft of the bone to be included by any normal capsule. 8. Fracture within the capsule is followed by disease of the tissues constituting the hip-joint; and the loss from absorption due to imperfect nutrition after the fracture gives rise to an appearance so similar to that produced by the interstitial absorption from age, that asserted specimens of intra-capsular union have been supposed to be only examples of such changes. In future cases, it is of great importance that the diagnosis of the fracture should be well made out in a view to future confirmation by an autopsy.

Dr. Post, while treating some of Dr. Smith's propositions as not even probable hypotheses, admitted the importance of his demonstration that the extent of the cervix inclosed within the capsule varies much in different subjects, and that there is a considerable portion of the cervix between the insertion of the capsule and the inter-trochanteric lines. It may be inferred, however, from all this, that it is usually impossible during life to determine whether the fracture is entirely within the capsule, while even after death, when absorption has taken place, this may be impossible to do. Dr. Post therefore proposes dividing these fractures into two classes, terming those situated between the caput femoris and the inter-trochanteric lines *intra-cervical*, and those at the inter-trochanteric lines, extending more or less into the shaft of the bone, *extra-cervical*—the division corresponding nearly with that of intra- and extra-capsular. He offers the following propositions: 1. *Intra-cervical fractures* are usually included within the capsular ligament, being near the head of the bone, and often involving a portion of it. 2. They are attended with a shortening of the limb, which in recent cases rarely, if ever, exceeds an inch. 3. Bony union very rarely occurs. 4. Whether bony union takes place or not, the cervix becomes greatly shortened, and after several weeks or months the shortening may amount to two inches or more. 5. As the neck is shortened by absorption, the capsule shifts its position, so that in some cases it ultimately becomes attached to the shaft of the bone. 6. In *extra-cervical fractures* the cervix is driven into the spongy structure at the junction of the trochanters with the shaft; and if the fracture be the result of a moderate amount of force, the upper fragment will be impacted in the lower, the shortening varying from ¼ to 1½ inch. 7. When the amount of force is greater, the trochanters split off, the fragments acquiring a great degree of mobility, and the shortening varying from 1 to 2½ inches. 8. Bony union may be generally expected, if the patient be not infirm or of very advanced age. The union of the trochanters with the shaft takes place earlier than that of the neck. 9. There is not usually any remarkable shortening of the cervix.

III. *On the Treatment of Burns.* By Professor Roser. (Archiv der Heilkunde, 1862, No. 1.)

In truth, there can scarcely be said to be any special treatment of burns, the loss of substance resulting from their influence being repaired by the same processes as prevail after any other form of destruction of the cutaneous surface. Much depends upon the depth to which the injury has extended, for while a destroyed epidermis is rapidly reproduced, when the skin has suffered to a great depth, or throughout its entire substance, the reparation, after the separation of the mortified parts, may be very tedious, in consequence of the great loss of substance, or of the unsuitable character of the parts affected for cicatricial formation. The contraction after cicatrization is also often great, both from this loss of substance and from the position of the parts, as in the bend of the limbs, being favourable to such contractile action. It is, however, an error to suppose that greater contraction follows burns than takes place from similar loss of substance from other causes. When the skin has been destroyed only through a portion of its substance, this contraction is not observed, only smooth cicatrices analogous to those of small-pox being present.

Not only is the period required for reparation and cicatrization after burns dependent upon the depth to which the injury has extended, but it is still more so on the inflammation of the skin which it has given rise to. This may affect not only the part actually burnt, but the adjoining parts, and may give rise to a secondary destruction of the skin, or at all events obstinate suppuration, which will delay the healing process, this then assuming a remarkably chronic character. Although it is this last circumstance which by its complexity and its consequences chiefly renders our prognosis uncertain, yet the other point, the determination of the depth to which the burn has extended, is by no means always an easy matter at first, even for the most experienced observer.

When the epidermis is merely lost or raised up into bullæ, the treatment of burns is simple enough. The serum should be discharged from the bladders by small punctures, without removing the epidermis. The necessity of covering the exposed nervous papillæ explains the success of various empirical applications; and even the solution of nitrate of silver acts only in this way. The exposed cutis becomes covered with a thin layer of gelatinising albumen, and the deposit which ensues upon the application of the nitrate solution produces a protective crust. Lead and chalk waters act in the same way, and cotton wool, the most suitable and simplest of all dressings, thus protects the exposed parts from the action of oxygen, and from mechanical contacts.

After two or three days, the physiological condition of the parts is different, the exposed papillæ having, in consequence of a growth of cells and vessels, become covered with a protective covering. The excessive sensibility has diminished, suppuration may have occurred, and perhaps here and there, deeper penetrating inflammatory action has been set up. We have now to seek whether we can moderate this inflammatory action, and favour the formation of epidermis. But, in fact, we can do little in the matter. If the injury is superficial, the healing will soon take place, under the use of the most different and sometimes absurd means; while if the inflammation has penetrated deeply, do what we will, the cure will be slower. We may resort, according to circumstances, to various local antiphlogistic applications. When the mortification of the skin is only superficial, it is quickly thrown off, leaving a retiform granulating surface, and art has here little to do beyond protecting the parts from injury. When the mortification extends deeper, we have no specific means for hastening the suppuration of parts which must precede separation. Such separation is a physiological process, and our intervention is limited to the prevention of its being obstructed. But the separation having taken place, have we any means of hastening the growth of granulations or of repressing them when too luxurious? Physiology and clinical experience alike declare that nothing need be done. The granulations, if too prominent, will afterwards recede without the aid of caustic; and the smoothest cicatrices will be found in those cases in which least interference has taken place. When, however, the wound is advancing towards cicatrization, the condensation of the connective tissue, and its covering with epidermis, are much favoured by a protective covering of adhesive plaster.

In the extremest degree of burns, when the skin is destroyed throughout its entire substance, we have to do with a condition of great surgical importance; cicatricial contraction, which although of the highest utility in lessening the surface of the wound in some cases, proves in others of the most serious detriment. The employment of adhesive plaster is of great service in regulating this contractile power of the cicatrix. Thus, suppose a longitudinal loss of substance on the anterior surface of the index finger exists, there is no better means of regulating the direction of the cicatrix which will ensue, than surrounding the whole finger by small strips of plaster. These act in three ways. 1. The circular application displaces the skin from left to right, and favours the formation of the cicatrix in the transverse direction, in which no contraction can occur. 2. Contraction in the longitudinal direction is also prevented by the finger being kept stiffly bound up, so that its flexure is difficult. 3. The plaster exerts a very decided influence in preventing or remedying that projection of the cicatrix, which is so remarkable after longitudinal loss of substance at the bend of joints and in the neck. In the same manner, in the most various parts of the body, we may obtain from the use of plaster beneficial effects which no other means will furnish. So also the contractions supervening on burns which have been improperly treated, are so easily, rapidly, and completely remedied by the methodical application of plaster, that the cases must be seen to obtain belief. Most aggravated cases, which those unacquainted with this simpler means would suppose to be amenable only to the knife, and others for which this has been in vain resorted to, have readily yielded to the application of plaster. It was Mr. Tamplin who first drew attention to this mode of treatment, and Professor Roser's experience has amply confirmed the truth of his statements. He has been quite astonished at the success which attends the treatment of the worst cases of contraction of the fingers. The plan consists essentially in surrounding the part with small strips of plaster, and making counterpressure at the contracted flexures by means of small balls of cotton wool.

IV. *Statistics of Operations for Cataract.* By M. Rivaud-Landrau.

(L'Union Médicale, No. 118.)

M. Rivand-Landrau, of Lyons, states that during twenty years' practice he has performed 2317 operations for cataract—viz., 2073 by extraction, 177 by depression, and 67 by division. The following are the general results:—

Extraction.—This was performed 2073 times, the double operations being 317, and the monocular operations 1756. The cataract was lenticular in 1825 instances, "semi-soft" in 83, capsulo-lenticular in 33, traumatic in 28, soft in 22, congenital in 22, and stony in 2. The results were successful in 1764, partially successful in 108, and unfavourable in 201 of the 2073 cases.

Depression was performed in 177 cases, 32 being double, and 113 monocular operations. The cataract was hard in 105, capsular in 23, "semi-soft" in 14, and traumatic in 10 of the cases. Success followed in 102, partial success in 25, and failure in 50 cases.

Division or breaking-up was performed in 67 cases, the double operations being 25, and the monocular 17. In 40 of the cases the cataract was congenital, in 11 "pulpy," in 12 traumatic, in 2 capsular, and in 2 "semi-soft." The results were successful in 55, partially so in 8, and unsuccessful in 4 cases.

As the general result of the 3217 operations, 1921 succeeded completely, 141 did so incompletely, and 255 failed. Nineteen successful secondary operations reduced this latter figure to 236. Comparing the different modes of operating with each other, it is found that the failures in extraction were 9 per cent., and in depression 39 per cent. The author has abandoned this latter operation, save in some special cases. He has not compared the operation by division with the other modes, as it is only applicable to certain cases of congenital and of soft cataract. Taking all the methods together, there were 236 failures in the 2317 operations, or about 11 per cent.

V. *On Urethrorrhœa.* By M. Diday. (Archives Générales, October.)

In this paper M. Diday describes a form of urethral discharge, which he for long regarded as merely a degree of ordinary blennorrhagia, but of the distinct nature of which he has now abundantly assured himself. It is produced, in fact, by copulation with women having the menstrual discharge upon them, and under a variety of circumstances may be mistaken for a mild form of gonorrhœa. The mildness of the attendant inflammation, the paucity and non-purulent character of the discharge, and the obstinacy with which this persists, are the characteristics of the affection; and that these characteristics are not due to individual peculiarities, is shown by the fact of the ordinary train of acute symptoms being induced in these same persons when they have been exposed to the contagion of the ordinary gonorrhœal discharge. Several cases are cited by the author, in which this distinction is well made out. The chronic persistent benignity of the disease from beginning to end is its pathognomic character. The period of its incubation is very short, that of its duration very long, lasting, if left to itself, for many months; and, although attended with little or no pain, leaving behind frequently local irritability. A point in which this form of discharge differs from that of ordinary gonorrhœa is the resistance which it offers to the action of abortives, and to that of copaiba and cubebs. In fact, little as it is in amount, and slight as is the irritation which accompanies it, M. Diday has found that the best way of managing it consists in employing mild antiphlogistics and diluents for twenty or thirty days (a course very unwillingly submitted to by patients for what seems to them a trifling ailment), and then resorting to various forms of injections, which, if they are employed at first, are almost certain to fail. Whether this form of urethral discharge is contagious M. Diday is unable to say, although inclined to believe that it is not so. As to the question whether it is not producible by leucorrhœal discharges as well as by the menstrual flux, he replies in the negative. Simple leucorrhœa occurring in honest women, does not give rise to urethral discharge from the male urethra; and when the result has been observed, it will be found, to have proceeded from women whose antecedents justify the belief that their leucorrhœa, whether known to themselves or not, was commingled with blennorrhagic poison.

VI. *On Perchloride of Iron in Aneurysm.* By M. Bribosia. (Bulletin de l'Acad. de Méd. de Belgique, December, 1861.)

M. Bribosia relates to the Brussels Academy a case of aneurysm of the frontal region, which he cured by the injection of a large quantity of the perchloride. From the consideration also of the various other cases of this kind which have been published, he arrives at the following conclusions:—1. Certain descriptions of aneurysm are curable by this method. 2. Its employment is not exempt from danger. 3. It is most suitable for aneurysms which consist entirely of liquid blood, their walls not being lined with fibrine, for those which are not situated too near the trunk, and particularly for aneurysms of small arteries, such, for example, as those of the cranium. 4. When the injection is made, compression should always be made above and below the aneurysm, in order to prevent the substance being carried into the circulation, or the coagulum being dissolved in the albumen of the blood. 5. The perchloride at from 20° to 25° produces complete coagulation, and if employed in a more concentrated form mortification of the walls of the vessel may result. 6. It is better to inject too small a quantity of the fluid, and have to repeat the operation, than risk too large a dose. 7. Four or five drops suffice for the coagulation of a centilitre of blood.

M. Bribosia also relates two cases in which the perchloride was employed with success in the treatment of *erectile tumours* in infants. In this affection, he regards it as an excellent means, but one which requires great precautions. He nearly lost one of his cases by reason of the injection penetrating on one side as well as within the substance of the erectile tumour. One, two, or three drops at 15° to 20° should be employed, repeating the injection if necessary.

[In the 'Moniteur des Sciences Méd.' 1862, Nos. 15 and 16, M. Sentoux reports 7 cases of *varices* successfully treated by the perchloride by M. Deaucé, of Bordeaux. Of 126 cases, collected chiefly in provincial practice, 100 terminated in cure, 19 in amelioration, and 1 in death. In 6 no results were obtained.

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Cornea.—Furnari on "Conjunctival Tonsure" in Chronic Affections of the Cornea. (Gazette Médicale, 1862, Nos. 4, 6, 8.)

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Palate.—Baizeau, Review of Procedures for Treatment of Perforations of the

Palate. (Archives Générales, Dec., 1861.) Débout on Prothesis and Autoplasty in Congenital Division of the Palate. (Bull. de Thérap., Dec. and Jan.)

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Salivary Calculi.—Immisch on Salivary Calculi. (Deutsche Klinik, 1861, Nos. 44, 45, 47, 48.)

Spine.—Wood on an Apparatus for the Treatment of Pott's Disease of the Spine. (Med. Times, Feb. 8th, 1862.)

Stricture.—Jütte. Cases of Perineal Urethrotomy in Stricture. (Deutsche Klinik, 1861, Nos. 44, 45, 46, 47.)

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Vesico-Vaginal Fistulæ, Verneuil on the Operation for. (Archives Générales, Jan. and March, 1862.)

Wounds.—Ashhurst on Penetrating Wounds of the Chest and Throat. (American Journal of Med. Science, Jan., 1862.) Atlee on Penetrating Wound of the Abdomen, with Protrusion of Omentum. (*Ibid.*)

QUARTERLY REPORT ON MIDWIFERY.

By Robert Barnes, M.D., F.R.C.P.

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I. The Anatomy and Pathology of the Non-Pregnant State.

1. *Researches on the Anatomy and Physiology of the Secretory Apparatus of the External Genital Organs of the Female.* By C. A. Martin and H. Leger. (Arch. Gén. de Méd., Jan., Feb. 1862.)
2. *Tumours of the Labia, Clitoris, and Vagina.* By Dr. McClintock. (Dublin Quart. Journ., Feb. 1862.)
3. *Incarcerated Hæmatometra, in consequence of Atresia of the Os Externum Uteri.* By Dr. U. Prell. (Mon. f. Geburtsk., Dec. 1862.)
4. *Chronic Uterine and Tubal Catarrh; Hæmatoma of Uterus; Peritonitis.* By Dr. Wagner. (Arch. f. Heilk., 1861.)
5. *A Simple Method of Lowering the Vesico-Vaginal Septum to facilitate the Operation for Fistula.* By Dr. Bourguet. (Bull. Gén. de Thérap., Jan. 1862.)
6. *On the Galvanocaustic in Gynæcology.* By Dr. O. v. Grunevaldt. (Peterb. Med. Zeitschr., 1 Heft, 1861.)
7. *Injections of Chloroform and Brandy into the Uterus.* By Dr. Paul Lubin. (L'Un. Méd., Jan. 1862.)
8. *Death following the Injection of Iodine into an Ovarian Cyst.* By M. Demarquay. (L'Un. Méd., Jan. 1862.)
9. *Report on an Uterine Pessary, with Jointed and Moveable Pieces, of M. Grandcollot.* By M. Robert. (L'Un. Méd., Jan. 1862.)

1. MM. Martin and Leger give the results of a careful anatomical investigation of the secretory apparatus of the external organs of the female. Their conclusions are as follow:

(1.) The secretory apparatus of the female external genital organs is exclusively composed, with the exception of the vulvo-vaginal gland (Bartholini's), of sebaceous glands "en grappe," and some sudoriparous glands, which are only found on the external or cutaneous surface of the labia majora.

(2.) The sebaceous glands increase in number and diminish in size from the external aspect of the labia majora to the internal aspect of the labia minora. On the limit of the labia minora they cease suddenly; no trace of them is found on the vestibule.

(3.) The muciparous follicles of the vestibule, meatus, and urethra, described by authors, do not exist; they have probably been confounded with the mucous crypts.

(4.) The sebaceous glands of the labia minora do not exist in the fœtus; they only arrive at complete development at puberty; after the menopause they undergo atrophy. During pregnancy they acquire their greatest size.

2. Dr. McClintock has laid before the Dublin Obstetrical Society an admirable series of drawings and preparations, illustrating the pathology and treatment of tumours of the labia, clitoris, and vagina, and accompanied by concise histories of the cases. The cases included malignant and non-malignant diseases. We can only direct attention to the report of the proceedings and the woodcuts in the 'Dublin Quarterly Journal.'

3. A case of incarcerated hæmatometra, related by Dr. Prell, is an interesting illustration of a rare condition. A woman, aged forty-three, married sixteen years, had never been pregnant. In first years of married life she suffered from obstinate leucorrhœa, and from that time the menses were frequently attended by uterine colics. In March, 1860, the catamenia ceased; the breasts began to swell, vomiting appeared, and in July the abdomen was sensibly enlarged. On the 23rd, after a sudden fall, she had retention of urine and a sense of a body being forced out of the pelvis. The pelvis was filled with a round elastic tumour. No os uteri could be found. The case was thought at first to be one of retroversion of the gravid womb. During a forcible attempt to replace the tumour by pressure from the rectum, blood escaped by the vagina, and a small opening became visible, which turned out to be through the cervix uteri. The patient got better, and the catamenia afterwards flowed through the restored opening. Careful examination led to the conclusion that there was a deformity and extreme stenosis of the os externum uteri.

4. Dr. Wagner relates a case which belongs to a class still comparatively rarely observed. A young woman died after symptoms of peritonitis. On dissection, there was found serous fluid in the abdomen, and towards the pelvis a thinner fluid, mixed with yellowish, very soft, purulent, fibrinous masses; both ovaries were much injected. The Fallopian tubes in their outer halves were much contorted, thickened, and contained a copious thin purulent fluid. The uterus contained a thin purulent offensive fluid, and on the posterior wall of the lower part a soft blood-clot closely adherent to the mucous membrane. It was concluded that there was, primarily, chronic inflammation of the mucous membrane of the uterus and tubes; secondarily, escape of offensive matter into abdominal cavity, and peritonitis.

5. Dr. Bourguet describes a method of bringing the vesico-vaginal septum within sight and reach, which, in a difficult case related, much facilitated the operation for the cure of vesico-vaginal fistula. The patient was at first placed on her back; then a catheter (Belloc's) was passed into the urethra, carrying the end out by the vaginal orifice. A riband-shaped loop was passed through

the eye of the catheter. The instrument was then withdrawn, bringing the ends of the thread with it so as to leave the loop astride between the urethra and vagina. The portion of the loop coming out of the meatus was firmly attached to an elastic catheter; this catheter was then passed into the bladder until the loop came opposite the fistula. Traction was exercised upon the loop hanging in the vagina, and permitted the vesico-vaginal septum to be drawn down at pleasure.

6. Dr. Grünewaldt refers the first use of the galvanic cautery in Petersburg to the late Dr. Crussel, in 1846. Dr. Crussel used wire brought to a white heat for purposes of cutting. Dr. Middeldorff published a book on the subject at Breslau in 1854. Dr. Grünewaldt says the galvanic cautery is applicable in three forms to the treatment of uterine cancer: 1. To amputate the cancer by means of the cutting loop; 2. Where the degenerated parts cannot be amputated, to transfix them after the manner of a cauterization *à flèche*, and to separate them from the sound parts by inducing them to be cast off; 3. To cauterize the ulcerated surfaces, and to restrain their growth, where the two first methods cannot be carried out. He admits, however, that the prognosis remains unfavourable. He relates four cases of amputation of the cervical portion of the uterus on account of malignant disease, in which the method is presumed to have been successful.

7. Dr. Paul Lubin relates a case of an hysterical woman, in whom, to allay intense uterine pains, he injected chloroform into the uterus. The pains were removed, but for two years afterwards the patient became quite blind. To cure this he injected brandy, and the patient is said to have recovered her sight.

8. M. Demarquay relates a case in which the injection of a solution of iodine into an ovarian cyst was followed by death. The patient, aged fifty, had been the subject of a similar operation a year before, the cyst having filled again. The solution consisted of water and tincture of iodine, equal parts, and about forty grains of iodide of potassium. The symptoms were those of shock; they supervened rapidly, and death took place on the following day.

9. M. Grandcollot's pessary is well spoken of by M. Robert. It consists of an abdominal belt provided with two truss-cushions. Between these is a metallic frame with double lateral joints, in the interval of which is attached a swan-neck stem, curved downwards, to support the pessary. This stem is thus susceptible of varied adaptation. To the lower end of the swan's neck is fixed an infra-vaginal stem, straight, formed of two hollow cylinders, one enclosing the other, and capable of sliding up and down so as to lengthen or shorten the vaginal stem at will. The uterine cup is attached to the upper end by a spring, which admits of lateral motion as well as of a certain amount of rotation. This mechanism renders the introduction easy, whilst the power of adapting the length of the stem to the exigency of the case, and the freedom of motion belonging to the vaginal stem and the swan's neck attached to the hypogastric truss, obviate most of the inconvenience of the ordinary pessaries.

II. Pregnancy.

1. *Extra-uterine Pregnancy: discharge of pieces of fœtal bone through an Abdominal Section: Recovery.* By Dr. Pauk. (Allg. Wien Med. Zeitung, 1862.)
2. *Case of Tubo-ovarial Gestation.* By Dr. Castelani. (Mon. f. Geburtsk., December, 1861.)

3. *On Abdominal Gestation, and especially on the attendant Internal Hæmorrhages.* By Dr. Birnbaum. (Mon. f. Geb., November, 1861.)
4. *Spontaneous Amputation of the Left Fore-arm* in utero. By Dr. Lingen. (Peters. Med. Zeitschr., 1 Heft, 1861.)
5. *On Chorea Gravidarum.* By Dr. Fr. Mosler. (Virchow's Arch., 1861.)
6. *On the Diagnosis of the Sex of the Fœtus.* By Dr. C. Steinbach. (Mon. f. Geb., December, 1861.)

1. Dr. Pauk relates a case which throws useful light upon the treatment of extra-uterine gestation. The patient, when in sixth month of pregnancy, was seized with diarrhœa, which recurred periodically. Extra-uterine gestation was diagnosed. An egg-shaped bladder at the umbilicus was opened, and vent given to foul pus. Cranial bones were felt by the finger passed into the opening. Two days later, the incision was extended three inches downwards towards the symphysis. A sac was thus exposed in which several fœtal bones bared of soft tissue were found. These being removed, the wound was closed by twisted sutures and an abdominal bandage. The patient had quite recovered in a fortnight.

2. A woman, aged thirty-two, who had never borne children, was brought to the hospital at Lille, suffering from acute abdominal pains. She had received blows on the abdomen, and abortion was apprehended. The os uteri did not expand, and the patient died next day. Large coagula were found in the abdomen, and in the midst a three-months' fœtus with torn navel-string. The uterus was enlarged, as in the third month, softened, clothed with a thick decidua, and with a mucous plug in the cervix. The right tube and ovary were healthy. At the end of the left tube was found the fœtal sac, the size of the first, with a rent in the wall, and the placenta inside. There was no trace of left ovary. It was concluded that the sac was formed between the end of the tube and the ovary, completely involving the ovary. If this point were clearly established, this would be another case illustrating the possibility of ovarian gestation, a condition which has lately been altogether denied.

3. Dr. Birnbaum relates two cases of abdominal gestation, which he makes the text for a dissertation upon the several questions connected with this condition. The following is a summary account of the cases:

(1.) A woman, aged thirty-eight, had borne two children eight and nine years ago. Having experienced symptoms of pregnancy, she died under sudden collapse. In the abdomen was found from eight to ten pounds of blood. A sac containing a six-months' fœtus was connected with the left tube. The tube was so completely involved with the sac, and covering the ovary, that this latter could not be distinguished.

(2.) A woman, aged forty-two, had several abortions and two children during her first marriage. After ten years' pause she became pregnant again. Complaining of abdominal pains, she was examined. The uterus was found much deviated from its normal position, the cervix being forcibly compressed against the anterior wall of the pelvis. At a later period, symptoms of abdominal inflammation and hectic supervened, with extreme distension of the abdomen. The head and breech of the fœtus were plainly made out through the abdominal walls, and the fœtal heart was heard. In this condition she died. Many recent peritoneal adhesions were found. A mature fœtus was contained in a transparent extremely thin sac. This was attached to the transverse colon, and fused in one mass with the omentum. The placenta was developed on the fimbriated extremity of the right tube.

4. Dr. Lingen relates a case of a girl four days old brought to hospital, in

which the left fore-arm was wanting. The skin covering the stump was puckered in two places by scars. There was no trace of rudimentary fingers. The mother had borne three well-formed children. Careful inquiry determined that nothing beyond the placenta had been expelled from the uterus. No trace of the fore-arm presumed to have been amputated in utero was found.

5. Dr. Fr. Mosler contributes an elaborate memoir on the chorea of pregnant women. He enters into a minute analysis of twenty cases collected from various sources, with a view to deduce a pathological history of this rare affection. He relates an original case. The patient, aged twenty-four, had enjoyed good health until nineteen years old, when she had an attack of chorea which lasted six months. She then had a normal pregnancy and labour. From this time she suffered from headache, sickness, pains. She suffered a fright and concussion from falling into a sand-pit fourteen days before present attack. This consisted in involuntary movements of the most varied kind in every part of the body, most marked in the upper extremities, and the whole right side was more affected than the left. Her gait was insecure, standing difficult, attitude very changing. The head was drawn to one side, then to the other, shaken for a long time, and then drawn backwards; the face was drawn into the most varied grimaces. Even the tongue was morbidly moved. During sleep the movements were interrupted. She slept much from fatigue. The movements recommenced immediately on waking. Attacks like hysteria also appeared; and afterwards strong epileptic convulsions. At certain periods the fits recurred regularly, about 10 a.m. and 4 p.m. The breath and heart-sounds were normal; pulse, 60 to 70. When admitted, the patient was about three months pregnant. She was quite cured of the chorea when seven months pregnant; and was delivered in normal labour at term. The treatment consisted in washings with cold water. But abortion being feared, these were abandoned for pills of valerian, oxide of zinc, assafœtida, of each one grain, three times daily. After three weeks, five to ten grains of saccharated carbonate of iron was given three times a-day, with strengthening diet. The iron was of chief value.

6. We have in a former report drawn attention to Dr. Frankenhaüser's proposition that the sex of the fœtus in utero may be known by observing the rate of the heart-pulsations. Amongst others, Dr. C. Steinbach has made observations on the subject. He submitted 56 women to auscultation. Amongst exceptional cases, he once found the fœtal pulse as low as 108 in the minute; this turned out to be a boy; and in another as high as 192; this was a girl. He found the mean number of pulsations in boys to be 131; for girls, 144. This so far agrees with Frankenhaüser's doctrine, that the mean for girls is higher than that of boys; but the range from minimum to maximum in either sex is too great to allow of any certainty in dealing with a given case.

III. Labour.

1. *Rupture of the Uterus, in which Recovery took place.* By Dr. John A. Byrne. (Dubl. Quar. Journ., Feb., 1862.)
2. *A New Case of Spondylolisthesis of the Pelvis: Death of Mother and Child.* By Prof. Breslau. (Mon. f. Geb., Dec., 1861.)
3. *Gradual Pelvic Narrowing from Growth of a Fibrous Tumour on the First Sacral and Last Lumbar Vertebræ.* By Dr. L. Mayer. (Monatsschr. f. Geb., Nov., 1861.)
4. *A Fibrous Tumour in the Small Pelvis.* By Dr. Olshausen. (Mon. f. Geb., Nov., 1861.)

1. Dr. J. A. Byrne relates a case of rupture of the uterus ending in recovery.

The patient, who had borne several children, aged forty, was seized with vomiting after being some hours in labour. The pelvis was roomy, but the head was hydrocephalic. There was slight discharge of blood; the fœtal sounds could not be heard. She had all the symptoms of shock, with great abdominal pain and tympanitis, and a symptom not generally described, but which Dr. Byrne says he has marked in other cases—namely, a spasmodic pain passing through the upper part of the sternum and back to the spine. Uterine action quite ceased. Delivery was effected by craniotomy. Opium was given in grain doses every hour; next day eighteen leeches were applied to the abdomen. Vomiting with prostration continued for some days; rigors occurred on the fifth and eighth days. On the twelfth day a purulent discharge from the vagina took place. Thirty-seven days after labour she left hospital quite well. Examined by speculum, there was observed a ridge of granulations across and through the os uteri.

2. Professor Breslau contributes a full description of the labour and dissection of a woman whose pelvis was deformed by "spondylolisthesis." The patient, aged forty-three, pregnant for the first time, was in labour on the 18th and 19th April, 1861. Dr. Breslau was summoned. On introducing the finger in the direction of the promontory, he easily struck the posterior wall of the brim, but could not be certain whether it was the true or a false promontory. The conjugate diameter seemed contracted to three inches. Dr. Breslau endeavoured to turn, but having with much difficulty brought down one foot, he could not make the head ascend; the fœtus remained doubled up. A copious flooding supervened. An attempt to seize the head with the long forceps failed. Pains continued for some hours; but the head was not much driven down. Dr. Spöndli next attempted to turn by introducing his right arm, but failed also. The leg which had been brought down, being in the way of the perforator and cephalotribe, was amputated by the bone-forceps of Stein and Mesnard. Scanzoni's cephalotribe was then introduced, but only a small segment of the head could be grasped. A trepan-perforator was therefore applied, and the skull with great difficulty pierced. By this time the patient was extremely exhausted. She died undelivered. Autopsy next day. The fœtus was of full size. The lower fourth of the placenta was loosened from the posterior wall of the uterus. Looking into the pelvis from above, the brim was seen divided into two parts; the narrowest diameter measured two inches ten lines. Great mobility was observed in the sacro-iliac joints. The deformity consisted in a sliding-down of the last lumbar vertebra on the upper sacral vertebra; the rounded lower third of the last lumbar vertebra even sank below the upper third of the second sacral vertebra. A displacement so complete and extensive has not hitherto been noticed in cases of spondylolisthesis. The last lumbar vertebra was amalgamated with the upper sacral. Dr. Breslau regrets that he did not in the first instance resort to the Cæsarean section.

3. The case of Dr. L. Mayer affords another illustration of pelvic contraction. The patient, aged thirty-two, had borne a living child in difficult labour at the age of twenty-six. A second labour, in less than a year following, was terminated by perforation. During this second childbed she suffered much from sacral pain and burning in the rectum. Profuse suppuration persisted. The source of this was ascertained to be an irregular wound, having cicatricial edges in the upper third of the left side of the vaginal roof. A sound passed from this opening by a fistulous canal to a swelling attached to the first sacral and last lumbar vertebræ. This tumour was of cartilaginous consistence, of roundish form, but with irregular projections. The uterus was of normal size. There was a slight general contraction of the pelvis, and the

conjugate diameter was reduced by three-quarters of an inch. Dr. L. Mayer believes that the bruising and tearing of the soft parts, including the periosteum of the first sacral and last lumbar vertebræ, was the cause of a chronic inflammatory process, giving rise to the profuse suppuration, and development of the osteophytes in the seat of inflammation. In this state she fell pregnant a third time. It was proposed to induce labour prematurely, but the patient refused. A full-sized child was delivered with great difficulty by the cephalotribe. Fever followed, and the tumour and uterus were very painful. She recovered, but profuse suppuration again set in from the fistulous opening leading from the vagina to the tumour. In two years this fistula had healed. The woman was again pregnant. The conjugate diameter was now still further contracted. She consented to the induction of labour. Warm water was injected into uterus between membranes and uterine walls; the membranes were ruptured; secale given, then infusion of ipecacuanha, but uterine action was not excited. The forceps was tried, but could not be locked. Some time after the operation had been given up, a dead child was expelled, forty-seven hours after the first injection of water. The patient made a good recovery.

4. Dr. Olshausen has recorded a case in some respects similar. A woman, aged thirty-three, had been four times pregnant. The first pregnancy ended prematurely; the child, in the second, died soon after delivery; in the two next, full-grown living children were born. The last labour was difficult. A fibrous tumour was then found in the pelvis. In her fifth pregnancy she was admitted to the Lying-in Hospital, the tumour having increased in size since last delivery. The tumour at this time sprang from the posterior wall of the small pelvis, considerably narrowing the cavity. The mass consisted of a number of connected knobs, mostly of hard consistence. It was not connected with the uterus. Examination by rectum proved that the tumour was seated entirely behind the bowel, and was in close relation to the sacrum. Labour was excited by intra-uterine injections; delirium followed these; a dead child was expelled. Rigors and abdominal pains succeeded labour, but she eventually recovered. The tumour remained.

The following titles are given, the memoirs to which they refer being worthy of record and consultation, but which cannot be analysed for want of space:—

At the thirty-sixth gathering of German naturalists and physicians, held at Spier in 1861, in the gynæological section Dr. Kirby read a paper on "Precipitate Births," detailing several cases. At the same meeting, Dr. V. Hüter read a paper on "The Fœtal Pulse." Professor Hecker also read a paper on "The Fœtal Bladder as an Obstruction to Labour," and Professor Lange read a paper on "Semmelweis' Theory of the Origin of Puerperal Fever." [As a summary of the views prevalent in Germany, the discussion on the latter subject is an interesting appendix to the previous discussion in the French Academy of Medicine. The remarkable circumstance, that puerperal fever is in great part a *manufactured* disease, and therefore an easily preventible one, was altogether overlooked by the Germans, when disputing upon various theories of etiology. The most prolific cause is the system of hospital-succour. Upwards of 4000 women were delivered at their own homes in 1861, by the Royal Maternity Charity of London, without a single death from puerperal fever.—R. B.]

Dr. Simon, of Rostock, read a paper on "Vesico-Vaginal Fistula."

On the Sexual Relations of New-born Children. By Professor Breslau. (M. f. G., Dec., 1861.) In this paper Dr. Breslau enters into controversial arguments, in reply to Dr. Ploss, concerning the influence of the father upon the sex of the child.

On the Use of Anæsthetics in Midwifery. By Dr. Fordyce Barker. (New York, 1861.)
On Chloroform-Narcosis in Obstetrics. By Prof Hohl. (Deutsche Klinik, 1861.)
Case in which an Ovarian Cyst was Punctured Thirty-five Times. By Dr. Bertrand. (M. f. G., Nov. 1861.)
On Post-mortem Cæsarean Section. Discussion in the French Academy of Medicine. Drs. Dépaul, Devergie, Kergaradec, Trébuchet. (Gaz. d. Hôp., 1861.)
Lemariey. (Gaz. d. Hôp., 1861.)
Mattei. (Ibid.)
Otterbourg. (Ibid.)
On the Extraction of the Fœtus after the so-called Spontaneous Evolution. By Professor Gustav Veit. (M. f. G., Dec. 1861.)

MEDICAL INTELLIGENCE.

Museum of Sanitary Appliances.

At the quarterly business meeting of the Metropolitan Association of Medical Officers of Health, early in the year 1861, it was unanimously resolved—"That it would materially assist the labours of health officers, and promote the cause of sanitary science, if all the more direct sanitary appliances, such as drain-pipes, stench-traps, cheap water-closets and urinals, ventilators, filters, gas-purifiers, smoke-consumers, &c. &c., were brought together in the International Exhibition, 1862, and if a permanent sanitary museum could be formed in the metropolis." A sub-committee was accordingly appointed to endeavour to effect these objects. We understand that after some delay Her Majesty's Commissioners for the Exhibition were at the time induced to allot a separate space, and nominate a distinct committee for sanitary appliances in connexion with Class X. (Civil Engineering, Architectural Machines, and Building Contrivances). We are, however, sorry to learn that, although a committee for sanitary appliances was constructed, the Commissioners have refused any separate place for the exhibition of the sanitary machinery, and distributed it in various departments according to its material. It is to be hoped that a collection will be made of these various appliances, to be rendered permanent in the metropolis for reference, and form the nucleus of a public museum.

To establish such a permanent sanitary museum, the only difficulty which, as we believe, the committee of health officers has to contend with is the finding a convenient room, of easy access, for the proper display of models and appliances. Several manufacturers, engineers, and inventors, have promised to present their works to such a museum free of cost.

Application has been made for a room to the Society of Arts, to the Metropolitan Board of Works, and to the Government, through the Chief Commissioner of Works. The Society of Arts could only devote their room to this purpose for a few months in the year. The chairman of the Metropolitan Board of Works, cordially as he approved the idea of a sanitary collection, finds that the new offices in Spring-gardens are already taxed to the utmost of their capabilities. The Right Hon. W. F. Cowper assures the health officers that he deeply sympathizes with their object, but is sorry to say that every building under his control is completely occupied. There is no room in Burlington House, or corner in the British Museum, that is not already occupied. He offers part of a room at the old Board of Health offices, which is sufficient only to contain a very few appliances.

We trust that the Health Officers will not abandon their intention of forming such a permanent Museum. We congratulate them on having secured a committee for sanitary appliances in the International Exhibition. The practical utility of this collection will be apparent to any one who has perused the classification of its contents, as drawn up and published by Mr. Edwin Chadwick in the 'Journal of the Society of Arts.' To take one of the many illustrations that might be given of its utility. The smoke nuisance in the metropolis is a great one; the smoke not only thickens and contaminates the atmosphere, arrests a great portion of the sun's rays, and dirties every object, but blights vegetation and enfeebles the vital powers of the people. That smoke destroys vegetation is a matter of everyday experience, and as vegetation absorbs carbonic acid gas and exhales pure oxygen, being in fact the only known source of any importance from which the oxygen of the atmosphere is derived, its destruction amidst a population of three millions must have a baneful effect on the public health. This nuisance also exerts an indirect influence on the health of the metropolis by necessitating the closure of windows to keep out "the blacks," and thus prevents the adequate ventilation of dwelling-rooms, counting-houses, and manufactories. The suppression of this nuisance would not only save the pockets of the inhabitants and increase the splendour of the metropolis, but greatly enhance the physical health and vigour of the people.

From returns* as to the execution of the Smoke Act, 1853, made to the last session of Parliament, at the instigation of the associated Health Officers, it appears that the provisions are carried out in a careless, inefficient manner. For instance, the returns which are before us state that formerly three *engineers* were employed as Inspectors of Furnaces in the Metropolitan Police District. At the present time this duty is entrusted to the ordinary policeman, with an appeal to an engineer. Within the City of London, the decision as to the efficiency or non-efficiency of any particular apparatus for the consumption of smoke is entrusted to the five Inspectors of Pavements. While the policemen approve as many as fifty-four different constructions of furnaces, the Inspectors of Pavements sanction one hundred and seven as efficient smoke consumers! A bill to amend this state of things has been introduced into the House of Commons: we trust that the execution of its provisions will be entrusted, as at Manchester, to the local sanitary authorities, and that they will apply not only to furnaces, but to at least the kitchen stoves of hotels and club-houses. We place, however, more reliance on a good display at the International Exhibition, of the numerous inventions for consuming smoke, than upon the compulsory clauses of an Act of Parliament. It is to the interest of every one, especially manufacturers, to consume the smoke arising from the coal they burn; it is from sheer ignorance that they do not employ the best apparatus to effect this object. At present they often incur considerable expense to little or no purpose, whereas, could they examine the different inventions side by side, they would be able to select the one best adapted for the purpose, and thus not only economize their fuel, as we are informed that one large brewing firm does to the extent of 2000*l.* per annum, but prevent an intolerable nuisance and injury to the health of the neighbouring population.

Believing as we do that a museum of sanitary appliances would tend in no inconsiderable degree to abate not only the smoke, but the other sanitary evils of the metropolis, we cordially wish that the Medical Officers of Health may succeed in their beneficent design.

Medical and Surgical Degrees in the University of Cambridge.—New Museums and Lecture-rooms contemplated.

The regulations for the new degree of Master in Surgery (M.C., *Magister in Chirurgia*) have passed the Senate, and some alterations have been made in the regulations for medical degrees (M.B. and M.D.). We subjoin a summary of the course which a student (commencing, say October, 1862) may follow:—

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* See Return to an Address of the House of Commons, dated July 19th, 1861, ordered to be printed.

He enters at one of the colleges, and pursues Classical and Mathematical studies till October, 1863.

Previous examination (in Classics and Mathematics), October, 1863.

Medical study in the University (by attending Lectures on Anatomy, Medicine, Surgery, Chemistry, Botany, &c., and the practice of Addenbrooke's Hospital) for two years, from October, 1863.

Natural Sciences Tripos Examination (in Comparative Anatomy, Chemistry, and Botany), December, 1865. This admits to B.A. degree.

Medical Study, in London or elsewhere, two years.

First Examination for M.B. and M.C. (in Anatomy and Physiology, Materia Medica, Pathology, Celsus and Aretæus. It is the same for both degrees), May, 1866.

Second Examination for M.B. (in Physiology, Pathology, and Practice of Physic, Clinical Medicine, Medical Jurisprudence, the Medical Treatment of Surgical and Obstetrical Diseases), November, 1867.

Second Examination for M.C. (in Surgical Anatomy, Pathology, and the Principles and Practice of Surgery, Clinical Surgery, Midwifery, Medical Jurisprudence), November, 1867.

M.B. degree or M.C. degree, or both, November, 1867.

Till the first examination the course of study for M.B. and M.C. is the same. After that it differs. Still a student may pursue the courses for both at the same time. The two degrees confer the right to practise every branch of the profession in any part of the United Kingdom.

The degree of M.D. can be taken three years after that of M.B.

It will be interesting to many of our readers to hear that the University are on the point of erecting New Museums and Lecture-rooms for Comparative Anatomy, Zoology, Botany, Chemistry, Mechanics, Mineralogy, and other branches of Natural Science. The plans for the building are, we understand, under discussion by the Senate at the present time, and it is probable the building will be proceeded with during the summer.

The Liverpool Training School and Home for Nurses.

The important subject of nursing has engaged the attention of the Liverpool public during the last two or three months, and with an earnestness which augurs favourably for its success.

Some years ago, the adoption of the scheme of the Nursing Committee of the Epidemiological Society, for the Training of Pauper Nurses,* was urged upon the Committee of Management of the Liverpool Workhouse Hospital, but it was at that time considered that it would be difficult, if not impossible, at the Liverpool Workhouse, to secure a staff of trustworthy, sober, and efficient nurses amongst female paupers; and even if such could be obtained, to exercise proper supervision and control over a class who would be partly paupers and partly stipendiary servants. This project was therefore never carried out.

The new movement originated with the Committee of the Royal Infirmary, seven of its members having been nominated as the Committee, with the Mayor, R. Hutchison, Esq. (whose warm sympathies have, it appears, always been enlisted in the cause of the sick poor), as its chairman. The institution is called "The Liverpool Training School and Home for Nurses." Its objects are,—

1st. "To provide thoroughly educated professional nurses for the infirmary," and it is to be hoped for the other hospitals of the town also.

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* See the Number of this Journal for July, 1856.

2nd. "To provide district and missionary nurses for the poor."

3rd. "To provide sick nurses for private families."

The nurses are to be trained in the Royal Infirmary, by the appointment of a number of head or training nurses, who are to have probationers under them as assistants for the period of four months. If then deemed qualified, the probationers are to have the charge, first, of a surgical ward for four months, and of a medical ward for a similar period, but still under the superintendence of a training nurse: making in all twelve months' training.

The nurses, subject to certificates of good behaviour and competency, are to be engaged for three years. Of these, the first year is to be employed in the infirmary, and the other two in hospital, district, or private nursing, according to circumstances. At the expiration of this term, the nurses may be re-engaged for another period of three years. After the first twelve months, they are either to be drafted into districts of the town, where they will be located, and superintended by district committees, or remain at the "Home" (a residence to be erected for their special use within the Infirmary grounds), for the use of private families.

About 4000*l.* have already been subscribed for the establishment of the "Home," independent of the cost of erection, the funds for which have been provided by the committee; and above 1000*l.* per annum promised as subscriptions for the payment and maintenance of the district nurses. A large sum has also been subscribed for providing the district nurses with appliances and comforts necessary for the sick poor.

To show the importance of, and value attached to this movement in Liverpool, we append the following resolution, passed unanimously at a full meeting of the Medical Institution, held on March 6th:—

"That to our hospital physicians and surgeons a reliable supply of nurses, educated to their work, would be a boon long desired. That the intelligent and kindly co-operation of nurses in poor patients' homes will be hailed with thankfulness by dispensary and other district visitors. And the power of obtaining good nurses from the Training School and Home for private families, is a great advantage to the profession as well as to the public. The members now present, therefore, are happy to express their hearty approval of the principles of the Nurses' Training Institution, and pledge themselves to aid its work by all the means in their power."

New British Pharmacopœia.

As we have heard many inquiries made regarding the long-anticipated appearance of the New Pharmacopœia for the United Kingdom, we are happy in being able to announce, upon the highest authority, that this work is expected to be published in October next. We are informed that the manuscript is very nearly completed, and that the printing will be almost immediately commenced.

BOOKS, &c., RECEIVED FOR REVIEW.

On Dropsy connected with Disease of the Kidneys (Morbus Brightii), and on some other Diseases of those Organs associated with Albuminous and Purulent Urine, &c. By W. R. Basham, M.D., F.R.C.P., Physician to the Westminster Hospital. Second Edition. London, Churchill. 1862. pp. 347.

On the Health of Merchant Seamen. By T. O. McWilliam, M.D., F.R.C.P., &c. (Reprint. Pamphlet.)

Essay on Anæsthetics in Midwifery. By B. F. Barker, M.D. (Reprint from the Transactions of New York Academy of Medicine. 1861.)

Table of the Weights of the Human Body and Internal Organs in the Sane and Insane. Arranged from 2614 post-mortem Examinations. By R. Boyd, M.D. (Reprint from the Proceedings of the Royal Society.)

Braithwaite's Retrospect of Practical Medicine, July to December, 1861.

Ranking and Radcliffe's Half-Yearly Abstract of Medical Sciences. July to December, 1861.

Ozonized Cod-Liver Oil, and its Medicinal Administration. By E. E. A. (Pamphlet.) Hughes and Butler.

Handbuch der Lehre von den Knockenbrücken. Von Dr. E. Gurlt. Berlin. Erster oder Allgemeiner Theil. Zweite und dritte Lieferung. Berlin. 1862. pp. 800.

Uterus Duplex Bicornis cum Vagina Simplici. Af E. Winge. Christiania. 1861. (Pamphlet.)

Clinical Surgery. Surgery of the Mouth, Pharynx, Abdomen, and Rectum, including Hernia. By T. Bryant, F.R.C.S. Part III. 1861. London, Churchill.

On the Immediate Treatment of Stricture of the Urethra, by the Employment of the "Stricture Dilator." By B. Holt, F.R.C.S., Senior Surgeon to Westminster Hospital, &c. London, Churchill. 1861. pp. 56.

The Placenta, the Organic Nervous System, the Blood, &c., Physiologically Examined. By J. Reilly, M.D. New York and London. 1861. pp. 204.

A Treatise on the Physiological Anatomy of the Lungs. By J. N. Heale, M.D. London, Churchill. 1862. pp. 84.

On the Teething of Infants, its Errors, Neglect, and Dangers, &c. By H. Hanks, L.R.C.P., Edinburgh. London, Davies. 1862. pp. 124.

Hints on Medical Ethics. An Address to the Glasgow University Medical Society. By J. A. Easton, M.D., Professor of Mat. Medica. (Pamphlet.)

Public Health in Relation to Air and Water. By W. T. Gairdner, M.D., F.R.C.P., Edinburgh, &c. Edinburgh, Edmonston and Douglas. 1862. pp. 369.

An Effectual and Simple Remedy for Scarlet Fever and Measles. By C Witt, M.R.C.S. Third Edition. (Pamphlet.)

Litzmann's Contributions to the Knowledge of Osteomalacia. Translated by J. Matthews Duncan, M.D. Edinburgh, Oliver and Boyd. pp. 41.

Proceedings of the Pathological Society of Philadelphia. Vol. I. Lippincott and Co. 1860. pp. 304.

Sketch of the Life and Works of Erasmus Darwin, M.D., F.R.S. By John Dowson, M.D. London, H. K. Lewis. 1861. pp. 61.

Nauenahr: A New Spa on the Rhine. By J. Miller, F.R.S.E. pp. 35.

Illustrations of Puerperal Diseases. By R. Uvedale West, M.D., Vice-President of the Obstetrical Society of London. London, Churchill. 1862. pp. 84.

Specimen of J. C G. Lucae's Plates on the Human Skeleton, to be Used with Dimmed Glass. Frankfort-on-the-Maine. 1861.

On the Diseases and Injuries of the Hyoïd or Tongue-Bone. By G. D. Gibb, M.D., M.R.C.P., &c. London, Churchill. 1862. pp. 48.

Annuaire de l'Association Générale de Prévoyance et de Secours Mutuels des Médecins de France. Première Année. 1858–1859–1860–1861. Baillière. 1862. pp. 468.

Des Affections Nerveuses Syphilitiques. Par D. A. Zambaco, M.D. Ouvrage Couronné par l'Acad. Impériale de Médecine. Prix Civrieux. Concours de 1859. pp. 596.

On the Therapeutic Law of Specific Remedies. By A. De Noé Walker, M.R.C.S.E. (Pamphlet.) London, Clayton. 1862.

Handbuch der Systematischen Anatomie des Menschen. Von Dr. J. Henle. In Drei Banden. Zweiter Band. Eingeweidelehre, Erste Lieferung. Braunschweig. 1862. pp. 286.

Ten Days in Athens, with Notes by the Way. By Dr. Corrigan, Physician-in-Ordinary to the Queen in Ireland. London, Longman and Co. 1862. pp. 227.

The Ambulance Surgeon, or Practical Observations on Gunshot-wounds. By P. L. Appia, M.D., of Naples. Edited by T. W. Nunn, Assistant-Surgeon to the Middlesex Hospital, and A. M. Edwards, F.R.S.E., Lecturer on Surgery, Edinburgh. Edinburgh, A. and C. Black. 1862. pp. 265.

Reviews, Reports, &c., Journals, &c.

Edinburgh Medical Journal. January, February, March, 1862.

Edinburgh Veterinary Review. January, February, March, 1862.

West India Quarterly Magazine. No. II.

The Australian Medical Journal. No. XXIV. October, 1861.

Boston Medical and Surgical Journal. Dec. 12, 1861. Jan. 16.

Dublin Quarterly Journal. Feb., 1862.

The American Journal of the Medical Sciences. January, 1862.

Report on the Sanitary Condition of the City of London for Quarter ending Dec. 28, 1861. By Dr. Letheby. (Pamphlet.)

Report on the Results of the Experiment of Applying Charcoal to the Sewer Ventilation. By H. Letheby, M.D., and W. Haywood, Engineer. (Pamphlet.)

The Intellectual Observer. Review of Natural History, Microscopic Research, and Recreative Science. No. I.

Sixth Annual Report of the Lunatic Asylum for Nottingham. Year 1861.

NOTICE TO READERS.

THE Editor is particularly desirous of having all Reports of Hospitals, Asylums, Sanitary Boards, Scientific Societies, &c., forwarded to him; as also Inaugural Lectures, Dissertations for Theses, Medical and Scientific Addresses, &c.

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